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FROM OCTOBER 1929 TO JULY 1930

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PROCEEDINGS AT THE MEETINGS OF THE PHYSICAL SOCIETY

SESSION 1929–1930

Except where the contrary is stated the meetings were held at the Imperial College of Science and Technology, South Kensington.

October 25, 1929.

W. H. ECCLES, D.Sc., F.R.S., and subsequently
Prof. F. L. HOPWOOD, D.Sc., in the Chair.

1. The following was elected a Fellow of the Physical Society: Thomas Herbert Harrison.

The President announced that the following had been elected Student Members of the Physical Society: Wallace Scott, Leonard Ernest Sharp, Stanley John Welton, Ronald James Cox, Brian Clifford Fleming-Williams, Ernest Riddick.

2. The President laid on the table for inspection by Fellows a biographical notice of the late Mr T. H. Blakesley, which its author, Mr W. A. Price, had kindly presented to the Library.

3. The President welcomed Prof. Sir C. V. Raman, F.R.S., who was present on a visit from India.

4. A paper entitled "Some additional lines in the secondary spectrum of hydrogen," by F. C. CONNELLY, A.R.C.S., B.Sc., D.I.C., was taken as read in the absence of the author.

5. A paper entitled "The transverse velocity gradient near the mouths of pipes in which an alternating or continuous flow of air is established," by E. G. RICHARDSON, B.A., Ph.D., D.Sc., and E. TYLER, M.Sc., F.Inst.P., was read by Dr RICHARDSON.

6. A paper entitled "Resolving-power tests on microscope objectives used with ultra-violet radiation" was read by B. K. JOHNSON, F.R.M.S.

November 8, 1929.

W. H. ECCLES, D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Physical Society: Donald William Read, Eric Oswald Hercus, Arthur Douglas Constable, Geoffrey Holte Aston, Frederick Daniel Smith, James Thewlis, G. Fergus Wood, Salih Mourat.

The President announced that the Council had elected the following a Student Member of the Society: Donald Walter Carter.

2. A lecture entitled "Diamagnetism and molecular structure" was given by Prof. Sir C. V. RAMAN, F.R.S.

A vote of thanks to the lecturer was proposed by Prof. E. N. DA C. ANDRADE, seconded by Mr T. SMITH, and carried by acclamation.

November 22, 1929.

W. H. ECCLES, D.Sc., F.R.S., in the Chair.

1. On the motion of the President, the meeting passed a resolution congratulating Prof. O. W. RICHARDSON, F.R.S., Past President of the Society, on the award to him of the Nobel Prize for Physics.

2. A paper entitled "Heaviside's operational method" was read by D. P. DALZELL.

3. A paper entitled "The dynamical theory of resonators" was read by E. T. HANSON, B.A.

4. A paper entitled "Escapement errors of pendulum clocks" was read by E. C. ATKINSON, M.A.

5. A demonstration of a model skidding vehicle was given by J. BRADLEY, B.A.

December 13, 1929.

W. H. ECCLES, D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Physical Society: F. D. Murad, Donald Arthur Oliver, George Alexander Cruickshank, Dattatraya Gopal Matange.

2. A paper entitled "Apparatus for determining the specific heat of a material in powder form," by J. H. AWBERY, B.A., B.Sc., and EZER GRIFFITHS, D.Sc., F.R.S., Physics Department, The National Physical Laboratory, Teddington, Middlesex, was read by Mr AWBERY.

3. A paper entitled "The determination of the parameters in an empirical formula," by W. EDWARDS DEMING, Ph.D., Associate Physicist, Bureau of Chemistry and Soils, Washington, D.C., was read by Mr J. H. AWBERY, in the absence of the author.

4. A paper entitled "The J-phenomenon in X-rays," by N. S. ALEXANDER, M.Sc., National Research Scholar, Auckland University College, New Zealand, was taken as read, in the absence of the author.

5. A demonstration of a new periodic effect in a neon discharge tube was given by Miss W. A. LEYSHON, Ph.D.

6. A demonstration of a method of showing the phase relation between the flash and the fork in a fork-controlled flashing neon-tube circuit was given by Miss W. A. Leyshon, Ph.D., and Miss Teresa M. Dillon.

7. Some apparatus for use in a hospital radium service was exhibited by Prof. F. Ll. Hopwood, D.Sc.

January 3, 1930.

Prof. J. C. Philip, F.R.S., President of the Science Masters' Association, in the Chair.

A discussion was held on examinations in practical physics. Members of the Science Masters' Association participated.

January 7, 8 *and* 9, 1930.

The twentieth annual exhibition of the Physical Society and the Optical Society was held in the Imperial College of Science.

Discourses were delivered as follows:

January 7th: Right Hon. Lord Rayleigh, M.A., Sc.D., F.R.S., "Iridescent colours in nature from the standpoint of physical optics."

January 8th: Mr S. G. Brown, M.I.E.E., F.Inst.P., F.R.S., "Gyro-compasses for gun-fire control."

January 9th: Sir Ambrose Fleming, M.A., D.Sc., F.R.S., "Television, present and future."

January 24, 1930.

W. H. Eccles, D.Sc., F.R.S., in the Chair.

1. The President stated that the Council had sent a message of deep sympathy to the relatives of the late Prof. H. L. Callendar in the name of the Society. The Society had been represented at Prof. Callendar's funeral by Prof. A. O. Rankine and Mr Robert Whipple.

2. The following were elected Fellows of the Physical Society: Frederick L. Uppelmann, E. Nightingale, G. R. Jain, James Stanley Rogers.

3. A paper entitled "A method of examining stereoscopic photographs," by J. M. Nuttall and E. J. Williams, was read by Mr T. Smith, in the absence of the authors.

4. A note on "The photography of Fabry and Perot interferometer fringes by the use of a simple optical system" was read by S. E. Green, B.Sc., Imperial College of Science.

5. A paper entitled "Characteristics of discharge tubes under 'flashing' conditions, as determined by the use of a cathode ray oscillograph" was read by Miss W. A. LEYSHON, Ph.D.

6. A demonstration of a *p*H apparatus was given by W. E. DORAN, M.A., The Cambridge Instrument Co., Ltd.

February 14, 1930.

W. H. ECCLES, D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Physical Society: Oscar Ulrich Vonwiller, Frank Allen Mitchell, John William Cottingham, Herbert Leslie Adams, Rhys Gerran Lloyd.

2. The President announced that the Council had awarded the Duddell Medal for 1930 to Prof. A. A. MICHELSON.

3. A paper entitled "A determination of the coefficient of diffusion of water vapour" was read by W. E. SUMMERHAYS, B.Sc., The University of Birmingham.

4. A paper entitled "A method of calculating the numerical equation of state for helium below 6° abs., and of estimating the relative importance of gas degeneracy and interatomic forces" was read by M. C. JOHNSON, M.A., M.Sc., Physics Department, University of Birmingham.

5. A paper entitled "The magnetostriction constant for alternating magnetic fields" was read by F. D. SMITH, M.Sc., A.M.I.E.E.

6. A demonstration of a model illustrating the mosaic theory of the compound eye, due to Altenburg, lent by the Zoological Department, King's College, University of London, was given by Captain C. W. HUME, M.C., B.Sc.

February 28, 1930.

W. H. ECCLES, D.Sc., F.R.S., and subsequently Prof. F. L. HOPWOOD, D.Sc., in the Chair.

1. The following were elected Fellows of the Physical Society: V. D. Dabholkar, William Band, David Kirk McCleery, Frank Lucas Warburton, Donald Percy Dalzell.

2. A paper entitled "A new type of Dewar flask, for use as a calorimeter" was read by W. L. WATTON, B.Sc., A.R.C.S., D.I.C., Imperial College of Science.

3. A paper entitled "Field intensity measurements around some Australian Broadcast stations," by R. O. CHERRY, M.Sc., Natural Philosophy Laboratory, University of Melbourne, was read by Mr R. A. WATSON WATT, in the absence of the author.

4. A paper entitled "The equations of motion of a viscous fluid in tensor notation" was read by C. N. H. LOCK, M.A.

March 14, 1930.

W. H. ECCLES, D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Society: Charles Albert Edward Whish, Samuel Morris Bower, O. A. Ankersmit, Robert W. King and Dorothy Agnes Goodman.

2. A paper entitled "A harmonic analyser" was read by J. HARVEY, A.R.C.S., B.Sc., City and Guilds Engineering College.

3. A paper entitled "A method of comparing small magnetic susceptibilities" was read by R. A. FEREDAY, B.Sc., East London College.

4. A paper entitled "The masses of the proton and electron" was read by H. T. FLINT, University of London, King's College.

5. Dr EZER GRIFFITHS, F.R.S., of the National Physical Laboratory, exhibited some lantern slides in connection with his recent visit to New Zealand.

Annual General Meeting, March 28, 1930.

W. H. ECCLES, D.Sc., F.R.S., in the Chair.

1. The minutes of the preceding Annual Meeting were read and confirmed.

2. The Duddell Medal, 1929, was presented *in absentia* to Prof. A. A. MICHELSON.

Mr David McK. Key, a Secretary of the United States Embassy, received the medal on behalf of Prof. Michelson and expressed his thanks for this honour.

3. The Reports of the Council and of the Hon. Treasurer were presented and adopted.

4. Dr H. BORNS and Mr J. NICOL having been appointed scrutineers, the following officers and members of Council were elected for the year 1930–31:

President: Prof. A. S. Eddington, M.A., D.Sc., F.R.S.

Vice-Presidents (*who have filled the office of President*): Sir Oliver J. Lodge, D.Sc., LL.D., F.R.S.; Sir Richard Glazebrook, K.C.B., Sc.D., F.R.S.; Sir Arthur Schuster, Ph.D., Sc.D., F.R.S.; Sir J. J. Thomson, O.M., D.Sc., F.R.S.; Prof. C. Vernon Boys, F.R.S.; Prof. C. H. Lees, D.Sc., F.R.S.; Prof. Sir W. H. Bragg, K.B.E., M.A., F.R.S.; Alexander Russell, M.A., D.Sc., F.R.S.; F. E. Smith, C.B., C.B.E., D.Sc., F.R.S.; Prof. O. W. Richardson, M.A., D.Sc., F.R.S.; W. H. Eccles, D.Sc., F.R.S.

Vice-Presidents: R. W. Paul, M.I.E.E.; Prof. A. O. Rankine, O.B.E., D.Sc.; J. S. G. Thomas, D.Sc.; J. Guild, A.R.C.S., D.I.C.

Hon. Secretaries: Ezer Griffiths, D.Sc., F.R.S.; Allan Ferguson, M.A., D.Sc.

Hon. Foreign Secretary: Prof. O. W. Richardson, M.A., D.Sc., F.R.S.

Hon. Treasurer: R. S. Whipple, M.I.E.E.

Hon. Librarian: J. H. Brinkworth, M.Sc., A.R.C.S.

Ordinary Members of Council: Miss A. C. Davies, D.Sc.; J. H. Awbery, B.Sc.; T. Smith, M.A.; W. Jevons, D.Sc., D.I.C.; Prof. W. Wilson, Ph.D., D.Sc., F.R.S.; D. Owen, B.A., D.Sc.; Sir Richard Paget, Bart.; Major I. O. Griffith, M.A.; D. W. Dye, D.Sc., F.R.S.; A. B. Wood, D.Sc.

5. Votes of thanks to the auditors (proposed by Dr J. S. G. THOMAS and seconded by Dr D. W. DYE), to the retiring officers and Council (proposed by Dr J. H. VINCENT and seconded by Major E. O. HENRICI), to the Governors of the Imperial College for permission to meet at the College (proposed by Mr T. SMITH and seconded by Prof. F. L. HOPWOOD), to Dr DYE for temporarily acting as Hon. Secretary for Business (proposed from the Chair), and to the scrutineers (proposed from the Chair), were carried unanimously.

Ordinary Meeting following the Annual General Meeting.

Prof. Sir ARTHUR EDDINGTON, M.A., D.Sc., F.R.S., in the Chair.

1. The following was elected a Fellow of the Physical Society: Albert Ernest Roberts Westman.

2. The meeting adjourned to the Electrical Engineering Department of the City and Guilds (Engineering) College, where by arrangement with the Delegacy, the staff and students of the department showed some of the research work now in progress.

April 11, 1930.

Prof. Sir ARTHUR EDDINGTON, M.A., D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Physical Society: William Herbert Aldous, Ronald Curry, Geoffrey Price Gowlland, Neville Melton Bligh.

2. The Fifteenth Guthrie Lecture was delivered by Prof. P. DEBYE of the University of Leipzig, who took as his subject "The scattering of X-rays in gases in relation to molecular structure."

3. A vote of thanks to the lecturer was moved by Prof. O. W. RICHARDSON and seconded by Prof. F. G. DONNAN.

May 9, 1930.

J. S. G. THOMAS, D.Sc., and subsequently Mr J. GUILD, in the Chair.

1. A paper entitled "Surface resistivity measurements on solid dielectrics" was read by L. HARTSHORN, D.Sc., A.R.C.S., D.I.C.

2. A paper entitled "The generation of sound by the siren principle," by E. SIMEON, was read by Dr J. S. G. THOMAS.

3. A demonstration of the regional absorption of dyes by growing crystals was given by Dr A. G. MILLIGAN, of the Admiralty Research Laboratory.

May 23, 1930.

Prof. Sir ARTHUR EDDINGTON, M.A., D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Physical Society: Lyndon Bastings, Edward George Lewin, Charles Vivian Jackson, Carl Howard Collie, Patrick Johnson, Wilfred Earnshaw Benham, Derek Jackson.

A discussion on magnetism was held.

The discussion was opened by Sir ALFRED EWING, F.R.S., with a paper on "Ferromagnetism and hysteresis."

The following also contributed papers:

Dr E. C. STONER, "Magnetism in the twentieth century." Prof. H. S. ALLEN, "Magnetism and the quantum theory." Prof. C. G. DARWIN, F.R.S., "The polarization of the electron." Mr W. SUCKSMITH, "The gyromagnetic effect and paramagnetism." Mr F. C. POWELL, "On the change in size of a ferromagnetic at the Curie point" (communicated by Mr R. H. Fowler). Prof. W. PEDDIE, "The interrelations of magnetization and temperature in crystals." Prof. P. WEISS and Dr R. FORRER, "L'aimantation à saturation des ferro-cobalts et des nickel-cobalts et les moments atomiques des trois métaux." Prof. W. GERLACH, "Neue Zusammenhänge von magnetischen u. elektrischen Erscheinungen." Dr P. KAPITZA, "Methods of experimenting in strong magnetic fields." Dr W. L. WEBSTER, "Magnetostriction and change of resistance in single crystals of iron and nickel." Dr L. F. BATES, "Observations on the specific heats of ferromagnetic substances." Dr J. R. ASHWORTH, "Relationships among the ferromagnetic constants."

June 13, 1930.

Prof. Sir ARTHUR EDDINGTON, M.A., D.Sc., F.R.S., in the Chair.

1. The following were elected Fellows of the Physical Society: Geoffrey Grime, Harry Buckley, H. E. Beckett, William Eric Newsome.

2. A paper entitled "The induction of electromotive forces in a moving liquid by a magnetic field, and its application to an investigation of the flow of liquids" was read by E. J. WILLIAMS.

3. A paper entitled "The motion of a liquid in an enclosed space" was read by E. J. WILLIAMS.

4. A paper entitled "Some notes on wireless methods of investigating the electrical structure of the upper atmosphere (II)," by Prof. E. V. APPLETON, F.R.S., was read by Mr G. A. RATCLIFFE.

5. A paper entitled "A simple method of showing the modes of vibration of a wire" was read, and a demonstration was given by C. R. DARLING, F.Inst.P., F.I.C., Assistant Professor of Science, Royal Military Academy, Woolwich.

6. A demonstration of "A modified Callendar recorder for the automatic control of a high-temperature oil bath" was given by Dr H. R. LANG, of the Imperial College of Science.

Visit to Rugby, June 14, 1930.

On arrival at Rugby the visitors made a tour of the works of the British Thomson Houston Company, Ltd. The party were the guests of the Company to lunch, which was served in the Staff Restaurant. In the afternoon the high power wireless station at Hillmorton was inspected, and later the party visited Rugby School where they were the guests of the Headmaster at tea. Mr MEIER, the Senior Science Master, organized the arrangements for a tour of the school.

The president, Sir ARTHUR EDDINGTON, M.A., D.Sc., F.R.S., expressed the thanks of the Society to the authorities of the British Thomson Houston Company and the Headmaster of Rugby School for their hospitality.

June 26, 1930.

Mr R. W. PAUL in the Chair.

1. A paper entitled "The effect of photosensitized mercury vapour on the walls of silica vacuum tubes" was read by M. C. JOHNSON, M.A., M.Sc.

2. A paper entitled "A sensitive rotating-coil magnetometer," by H. R. NETTLETON, D.Sc., and F. H. LLEWELLYN, B.Sc., Birkbeck College, was read by Mr NETTLETON.

3. A paper entitled "The frequency errors of rectifier instruments of the copper-oxide type for alternating current measurement" was read by L. HARTSHORN, D.Sc., A.R.C.S., D.I.C.

4. A paper entitled "A method of eliminating the effects of magnetic disturbance in highly sensitive galvanometers" was read by D. S. PERFECT, M.A., D.Phil., F.Inst.P., National Physical Laboratory.

5. The following papers were read in title:

"The thermal insulating properties of fabrics," by M. C. MARSH, M.A., B.Sc., A.Inst.P., British Research Association for the Woollen and Worsted Industries.

"Classical quantum theory and X-ray excitation by canal rays and alpha-particles," by WILLIAM BAND, M.Sc., Yenching University.

"Diffusion into an infinite plane sheet subject to a surface condition; with a method of application to experimental data," by A. T. MCKAY.

"Intensity modifications in the spectrum of mercury," by S. TOLANSKY, B.Sc., Research Fellow, Armstrong College.

"A vacuum tube commutator; the production of a periodic pulse of potential of square wave-form," by C. F. POWELL and K. H. MANNING.

REPORT OF COUNCIL FOR THE PERIOD ENDING FEBRUARY 28TH, 1930

MEETINGS

DURING the period covered by the Report 20 Ordinary Science Meetings were held at the Imperial College of Science and elsewhere. At these meetings 46 papers were presented and 13 demonstrations given. By the kind invitation of Prof. S. W. J. Smith about 60 members of the Society and their friends visited Birmingham on July 6. Five papers were read by workers in the Laboratory. The members of the Society inspected the Physics Block where a number of demonstrations were given and also some demonstrations in the Mechanical Engineering Department.

On November 8 a lecture was delivered by Sir C. V. Raman entitled "Diamagnetism and Molecular Structure."

On April 26 a discussion on the Teaching of Geometrical Optics was held, 95 Fellows and visitors being present.

EXHIBITIONS

Since the period covered by the last Report two Exhibitions, arranged jointly by the Physical and Optical Societies, have been held, through the courtesy of the Governing Body, at the Imperial College of Science.

Nineteenth Annual Exhibition. This was held on January 8, 9 and 10, 1929, and trade exhibits were arranged by 83 firms. The Research and Experimental Section and the Historical Section contained exhibits from 30 sources. Discourses were given by Prof. F. Lloyd Hopwood on "Experiments with High Frequency Sound Waves"; by Mr Conrad Beck on "Lenses" and by Mr A. J. Bull on "Some Colour Problems in Photo-Engraving."

Twentieth Annual Exhibition. This was held on January 7, 8 and 9, 1930. Eighty-three firms exhibited, and the Research and Experimental Section and Historical Section contained exhibits contributed from 40 sources. Discourses were given by the Right Hon. Lord Rayleigh on "Iridescent Colours in Nature from the Standpoint of Physical Optics"; by Mr S. G. Brown on "Gyro-Compasses for Gun-Fire Control" and by Sir Ambrose Fleming on "Television, Present and Future."

With the object of encouraging Craftsmanship in the Scientific Instrument Trade a new section in the Exhibition was formed in which the work of apprentices and learners was exhibited in competition.

OFFICERS

During the latter part of the year Dr D. W. Dye acted as Business Secretary whilst Dr Ezer Griffiths was absent in South Africa and New Zealand. Mr T. Martin resigned his position as Assistant Secretary and Dr J. J. Hedges was appointed as his successor.

REPRESENTATIVES ON OTHER BODIES

Dr A. Ferguson and Dr Ezer Griffiths have been appointed as representatives of the Society on the Board of the Institute of Physics. Dr Ferguson was appointed the representative of the Society on the Colloid Committee of the Faraday Society and Mr T. Smith on a Joint Committee to consider the Teaching of Geometrical Optics. Dr D. Owen and Dr A. Ferguson represented the Society on the Science Abstracts Committee.

AWARD OF THE DUDDELL MEDAL

At the meeting on April 19 the Sixth (1928) Duddell Medal was presented to Dr C. E. Guillaume, an Honorary Fellow of the Society. The Council has awarded the Seventh (1929) Duddell Medal to Prof. A. A. Michelson.

GUTHRIE LECTURE

Prof. P. W. Bridgman delivered the Fourteenth Guthrie Lecture on April 19, the subject being "The Properties of the Elements under High Pressure." About 150 Fellows and visitors were present.

OBITUARY

The Council records with deep regret the deaths of: Prof. H. L. Callendar, a Past President of the Society; Mr J. Walker, who joined the Society 47 years ago and served as a Vice-President; Mr T. H. Blakesley, who joined the Society 45 years ago and served for some time as Secretary; Mr A. A. Campbell Swinton, one time member of Council; and also Sir H. T. Wood, Mr A. C. Cockburn, Mr J. M. Gorham, Mr E. F. J. Love, Mr F. J. Mudford, Mr R. H. Rinaldi, Mr T. A. Garrett, Mr W. R. Bower and Mr P. G. Gundry.

The practice of publishing Obituary Notices of eminent Fellows of the Society has been resumed.

MEMBERSHIP ROLL

The number of Honorary Fellows on the Roll on December 31, 1929 was 11. At the same date Ordinary Fellows numbered 724 and Students 51.

The changes in the membership of the Society are shown in the appended table:

	Total Dec. 31, 1928	Changes during 1929		Total Dec. 31, 1929
Honorary Fellows	10	Elected	1	
		Net increase	1	11
Ordinary Fellows	690	Elected	45	
		Student transfers	5	
			50	
		Deceased	7	
		Resigned or lapsed ...	9	
			16	
		Net increase	34	724
Students ...	47	Elected	10	
		Trans. to Fellow ...	5	
		Resigned	1	
			6	
		Net increase	4	51
Total Membership	747	Net increase	39	786

REPORT OF THE TREASURER

THE income for the year 1929 exceeded the expenditure by £182. 18*s*. 7*d*. Of this amount £100 is due to the revaluation of the stock of publications. The Society has received the refund of taxes on its investment income for the years 1926, 1927 and part of 1928. It is greatly indebted to the Council of the British Association for the help given in connection with this matter.

The Cambridge University Press being unable to undertake the work connected with the publication of the Proceedings and other publications of the Society, it was decided in 1928 that, with the help of the Institute of Physics, the Society would act as its own publisher. This arrangement has worked satisfactorily.

The Society desires to thank the Council of the Royal Society for a grant of £150 towards the cost of its publications. It is estimated that more than this sum will be absorbed in payment for a special report in course of preparation.

£600 of 2½ % Consolidated Stock was purchased in October at a cost of £325. 3*s*. 6*d*.

The Society's investments have been valued at market prices through the courtesy of the Manager of the Charing Cross Branch of the Westminster Bank. Owing to the serious depression of the stock market their value was approximately 5 % less on December 31st, 1929 than at the corresponding period in the previous year.

(Signed) ROBERT S. WHIPPLE
Honorary Treasurer

March 7th, 1930

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED 31ST DECEMBER, 1929

1928 £ s. d.	Expenditure	£ s. d.	£ s. d.
	To		
500 0 0	Stock of Publications at 1st January, 1929		500 0 0
535 0 0	"Science Abstracts"		647 0 0
	Ordinary Publications:		
959 8 0	"Proceedings"	1126 1 11	
61 14 9	"Bulletin" and Notices	65 13 1	
93 17 2	General	56 6 3	
			1248 1 3
103 2 7	Postage on Publications		120 0 7
62 3 6	Reporting at Meetings		72 17 1
32 3 1	Expenses at Meetings		34 13 5
576 13 5	Exhibition Expenses	610 15 8	
45 12 6	Optical Society Exhibition Account	56 2 3	
			666 17 11
13 10 10	Periodicals and Library		6 4 7
	Administration Expenses:		
104 14 8	Institute of Physics	178 18 8	
100 18 0	Office Expenses	110 7 9	
			289 6 5
20 0 0	Guthrie Lecture (Honorarium)		20 0 0
	Provincial Meeting (Birmingham):		
47 3 9	Railway and Luncheon Expenses		28 15 4
52 5 9	*Reprinting parts of "Proceedings"*		
96 6 1	*Special Expenses on Transfer of Publishing*		
98 3 7	Balance, being excess of Income over Expenditure, carried forward		182 18 7
£3502 17 8			£3816 15 2
	To		
	Subscriptions written off		25 4 0
	Balance carried to Accumulated Fund		214 11 11
			£239 15 11

1928 £ s. d.	Income	£ s. d.	£ s. d.
	By		
500 0 0	Stock of Publications at 31st December, 1929		600 0 0
	Subscriptions:		
1128 14 2	Fellows*	1198 11 10	
24 19 6	(Voluntary†)	8 8 0	
16 13 0	Students	26 4 6	
60 7 0	For "Science Abstracts" and Advance Proofs	74 17 0	
			1308 1 4
726 5 5	Sale of Publications		663 17 11
40 3 0	Advertisements in "Proceedings"		141 8 0
	Dividends from Investments and Bank Interest	235 13 1	
	Add: Income Tax refunded Jan.–April 4, 1929	14 19 1	
	Income Tax claimed April 5–Dec. 31, 1929	20 2 9	
		270 14 11	
188 11 1	*Less:* Transferred to Duddell Memorial Fund	20 0 0	
			250 14 11
620 0 9	Exhibitors' Payments on account of Exhibition Catalogue		673 17 8
150 0 0	Royal Society Grant for Publications		150 0 0
47 3 9	Provincial Meeting (Birmingham): Receipts from Sale of Railway and Luncheon Tickets		28 15 4
£3502 17 8			£3816 15 2
	By		
	Balance brought forwa.d		182 18 7
	Income Tax refunded for 1926, 1927 and 1928		56 17 4
			£239 15 11

* Eighty-one Fellows paid reduced subscriptions by the arrangement with the Institute of Physics, the total rebate being £27. 19*s.* 2*d.*

† Voluntary subscriptions are subscriptions paid by Fellows who compounded for the low sum of £10.

BALANCE SHEET AT 31ST DECEMBER, 1929

Liabilities	£ s. d.	£ s. d.
Accumulated Fund:		
As per last Balance Sheet	2131 17 2	
Entrance Fees, 1929	35 14 0	
Balance brought forward from Income and Expenditure account	214 11 11	
	2382 3 1	
Less Decreased value of Investments	220 3 6	
		2161 19 7
Life Compositions:		
As per last Balance Sheet	2289 10 0	
Received during year	31 10 0	
		2321 0 0
Duddell Memorial Trust Fund:		
As per last Balance Sheet	437 12 9	
Less Decreased value of Investments	11 0 0	
		426 12 9
W. F. Stanley Trust Fund (For the "Bulletin"):		
As per last Balance Sheet	361 0 0	
Less Decreased value of Investments	17 0 0	
		344 0 0
A. W. Scott Bequest		250 0 0
Sundry Creditors		452 4 4
Subscriptions paid in advance		46 18 3
		£6002 14 11

Assets	£ s. d.	£ s. d.
Investments at Market Value on date:		
£399 London Midland and Scottish Railway 4 % Debenture Stock	303 0 0	
£1000 London Midland and Scottish Railway 4 % Preference Stock	703 0 0	
£211 London County Council 4½ % Stock	196 0 0	
£400 Lancaster Corporation 3 % Redeemable Stock	272 0 0	
£254. 2*s*. 9*d*. New South Wales 5 % Stock 1935/55	228 0 0	
£500 London and North Eastern Railway 4 % Debenture Stock	370 0 0	
£500 India 3½ % Stock	305 0 0	
£650 Funding Loan 4 % 1960/90	552 0 0	
£650 War Loan 5 % 1929/47 Inscribed "A" Account	650 0 0	
£150 Southern Railway 5 % Debenture Stock	142 0 0	
£600 Consolidated Stock 2½ %	315 0 0	
£400 War Loan 5 % 1929/47 Inscribed "B" Account	400 0 0	
£300 Southern Railway Preferred Ordinary Stock	216 0 0	
£442 Southern Railway Deferred Ordinary Stock	128 0 0	
		4780 0 0
Stock of Publications (as per Treasurer's Valuation)		600 0 0
Subscriptions due		19 8 6
Sundry Debtors		50 13 7
Inland Revenue—Income Tax reclaimed for 1929		20 2 9
Cash at Bank:		
On Deposit Account	400 0 0	
On Current Account	125 10 0	
Cash in hand	7 0 1	
		532 10 1
		£6002 14 11

ROBERT S. WHIPPLE, *Honorary Treasurer.*

We have audited the above Balance Sheet and have obtained all the information and explanations we have required. We have verified the Bank Balances and the Investments. In our opinion such Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of our information and the explanations given to us and as shown by the books of the Society.

SPENCER HOUSE, E.C. 2
18*th March* 1930.

KNOX, CROPPER & CO.,
Chartered Accountants

Examined and approved on behalf of the Society { (Signed) E. H. RAYNER. (Signed) J. H. AWBERY.

LIFE COMPOSITION FUND AT DECEMBER 31ST, 1929

	£	s.	d.
128 Fellows paid £10	1280	0	0
3 Fellows paid £15	45	0	0
1 Fellow paid £20	20	0	0
1 Fellow paid £20. 10s.	20	10	0
14 Fellows paid £21	294	0	0
21 Fellows paid £31. 10s.	661	10	0
168	£2321	0	0

W. F. STANLEY TRUST FUND (FOR THE "BULLETIN")

	£	s.	d.		£	s.	d.
£300 Southern Railway Preferred Ordinary Stock	216	0	0	Carried to Balance Sheet	344	0	0
£442 Southern Railway Deferred Ordinary Stock	128	0	0				
	£344	0	0		£344	0	0

DUDDELL MEMORIAL TRUST FUND

Capital

	£	s.	d.		£	s.	d.
£400 War Loan 5 % 1929/47 Inscribed Stock	400	0	0	Carried to Balance Sheet	400	0	0

Revenue

	£	s.	d.		£	s.	d.
Balance at 31st December, 1928	26	12	9	Honorarium to Medallist	20	0	0
Dividends	20	0	0	Balance carried to Balance Sheet	26	12	9
	£46	12	9		£46	12	9

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THE PROCEEDINGS OF THE PHYSICAL SOCIETY

VOL. 42, PART 1 *December* 16, 1929 NO. 231

THE TRANSVERSE VELOCITY GRADIENT NEAR THE MOUTHS OF PIPES IN WHICH AN ALTERNATING OR CONTINUOUS FLOW OF AIR IS ESTABLISHED

BY E. G. RICHARDSON, B.A., PH.D., D.SC. AND E. TYLER, M.SC., A.INST.P.

Received June 23, 1929. *Read and discussed October* 25, 1929.

ABSTRACT. A comparison is made of the alternating and direct flow of air near the mouths of tubes of various sections, by traversal of a hot-wire anemometer across the tube. After a correction for the use of the hot wire in the neighbourhood of the walls of the tubes has been made, the theoretical and observed gradients of mean velocity across the sections are compared. In the alternating flow a peak of high average velocity is found near the walls of the pipe, but in one-way flow this annular peak is absent, the velocity falling continuously from the centre of the tube to within a short distance of the walls according to the law $v = ay^q$, where q varies from 1/2 for stream-line flow up to 1/7 for turbulent flow. The existence of a layer of laminar flow close to the walls, when the main body of air is in turbulent motion, is demonstrated.

§ 1. INTRODUCTION

IN measuring the velocity in the alternating flow of air in the orifice o a resounding Helmholtz resonator one of us has shown that, owing to inertia, the velocity in annuli remote from the centre of the orifice was much greater than at the centre itself, except very close to the walls of the orifice, where friction caused a rapid drop down to zero velocity. At the end of the paper* in which this effect was described appeared a few measurements of the velocity across the end of a pipe in which alternating flow of low frequency was produced by a piston oscillating in the pipe†. Warren‡ has since pointed out the analogy between the flow in such an orifice and the motion of a flexible diaphragm oscillating in the

* *Proc. Phys. Soc.* **40**, 206 (1928). The following errata in that paper should be noted: p. 208, Table I, for "$n = 1000$" read "$n = 1100$"; p. 212, a minus sign is to be attributed to v_2, so that a minus sign is to be placed before the second term of the equation following, (7).

† Just recently, Carrière, in the *Journal de Physique*, **10**, 198 (1929), has confirmed these measurements by observation of the motion of microscopic particles in a sounding pipe.

‡ *Proc. Phys. Soc.* **40**, 296 (1928).

orifice. The experiments described in this paper were undertaken in order to examine the alternating flow of air in a pipe in more detail, and to compare it with continuous (one-way) flow.

§ 2. APPARATUS AND RESULTS FOR ALTERNATING FLOW

The apparatus is shown in Fig. 1. The alternating flow was produced by an electric motor oscillating a disc on the end of a fairly long crank with a stroke which could be varied at will. The frequency was regulated by observation of a strobo-

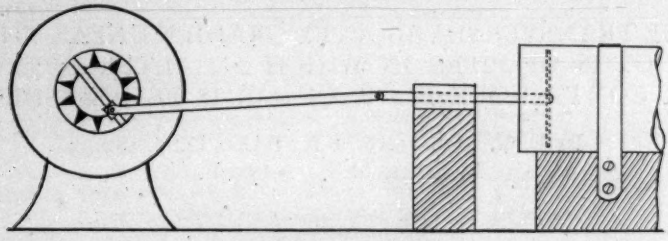

Fig. 1. Diagram of apparatus.

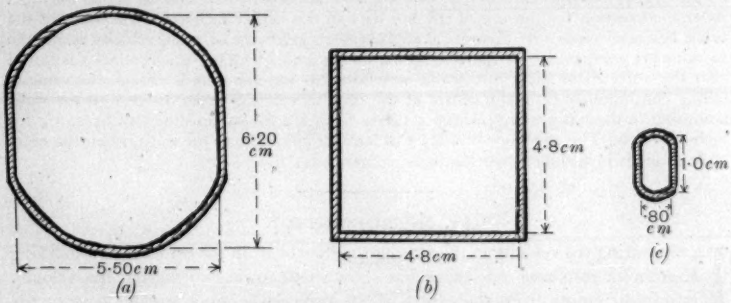

Fig. 2. Sections of tubes employed.

scopic disc on the motor which was illuminated by a neon tube at 50 ~. The piston moved to and fro near one end of a tube 33·0 cm. long, which it just cleared. The hot wire anemometer consisted of a piece of 0·002 in. nickel wire, 1·0 cm. long, supported vertically on a long fork attached to a travelling micrometer, so that it could be traversed across the tube at any section between the orifice and the piston. Two tubes were used, (*a*) one of circular section, 6·20 cm. in diameter, but with two "flats" formed at the sides to be parallel to the vertical hot wire; (*b*) a tube of square section 4·8 cm. wide, Fig. 2.

The wire was supplied with a current of 0·20 amp., and the change of resistance corresponding to a given average wind speed was measured on a Kelvin double

bridge. In this case the wire was calibrated by oscillating it on the piston itself at various amplitudes, and at the same frequencies as those used in the research. The calibration curve of resistance against maximum velocity at a given frequency

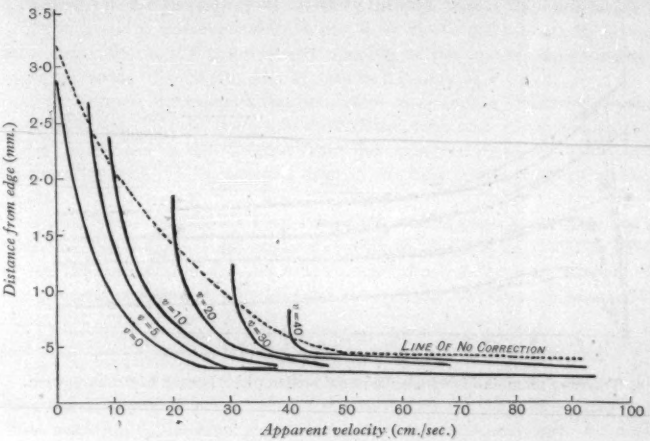

Fig. 3. Relation between true velocity v and apparent velocity.

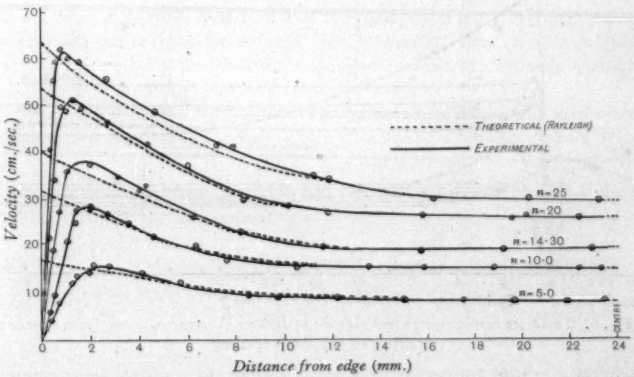

Fig. 4. Traverses 1 mm. inside opening of square section tubes. Length of stroke 0·95 cm.

was of the now familiar parabolic type. In the use of the hot wire close to a large metallic surface an error arises in the estimation of velocity due to the loss of heat by conduction across an intervening thin film of air. A correction for this at

steady velocities is available from some experiments published elsewhere*, in which a specimen of the wire was whirled on a fork round a brass cylinder of large diameter, at various small distances from the brass surface. From the resistance at

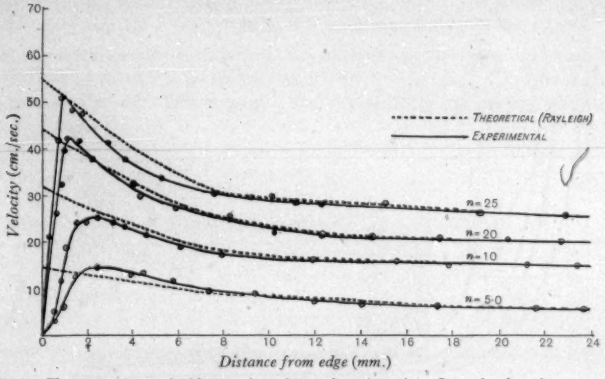

Fig. 5. Traverses 1 mm. inside opening of round section pipe. Length of stroke 0·95 cm.

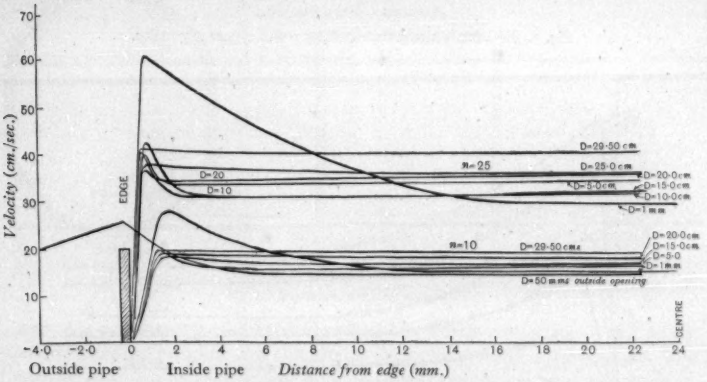

Fig. 6. Traverses across square section pipe at different distances D cm. from opening. Piston 31·0 cm. from opening.

a number of whirling speeds the apparent velocity from the ordinary calibration curve is read off. This has to be correlated with the true velocity, i.e. with the known velocity of the wire through the air round the cylinder. Fig. 3 shows such a correction curve of apparent to true velocity for a 0·002 in. wire taking 0·20 amp.

* Piercy and Richardson, *Aero. Res. Comm. R. and M.* No. 1229, 1929.

This was used on the velocities close to the walls of the tube in the present research, on the assumption that the average velocity in alternating flow would be affected similarly to a steady velocity of the same amount. A justification for this procedure is obtained in the passage of the velocity through zero at the boundary.

Figs. 4 and 5, corresponding to Fig. 9 for the round tube in the former paper, show traverses in the plane of the opening of the square and round tubes at a constant stroke of 0·95 cm., but at various frequencies n.

Fig. 6 shows traverses at frequencies of $25\sim$ and $10\sim$ respectively, at various sections down the tube from just outside the open end; it shows how the annular effect diminishes as the distance from the open end increases, the velocity being uniform (apart from the sudden drop in the boundary layer) just in front of the piston.

In one set of traverses a large flange was placed flush with the end of the tube, to reduce the possible solid angle of spreading by nearly one half. In confirmation of our suggestion that the inertia is thereby affected, the annular effect was much less pronounced, and at a distance 0·5 cm. outside the plane of opening the annular effect was completely eliminated.

§ 3. THEORY OF ALTERNATING FLOW

Rayleigh* has given the equation for the potential due to a flexible membrane with the condition that the velocity potential over the membrane shall be uniform. This equation is, however, insoluble unless the frequency term be neglected, in which case a solution corresponding to the theoretical steady flow through an orifice is obtained†. In order to retain the frequency in the solution the analogy of the membrane may be retained, but the membrane must be imagined to have a variable resistance over its surface, due to inertia. This resistance will be in phase with the velocity, but will decrease from the centre outwards, owing to the radial leakage of air from the confines of the orifice.

Sexl‡ has examined a number of cases of flow in tubes having axial symmetry, taking the general equations of flow of Stokes, with the stream function Ψ defined by

$$u = r^{-1}\,\delta\Psi/\delta l,\quad v = -\,r^{-1}\,\delta\Psi/\delta r$$

(where u is the radial, v the axial velocity, r the distance from the axis, l the length of the tube), in cylindrical co-ordinates, thus:

$$\frac{\delta}{\delta t}D.\Psi + \frac{1}{r}\frac{\delta\Psi}{\delta l}\cdot\frac{\delta}{\delta r}D\Psi - \frac{1}{r}\frac{\delta\Psi}{\delta r}\cdot\frac{\delta}{\delta l}D\Psi - \frac{2}{r^2}\frac{\delta\Psi}{\delta l}.D\Psi = \nu DD\Psi \qquad \text{.....(1)}$$

where ν is the kinematic viscosity and D is Stokes's operator, so that

$$D = \frac{\delta^2}{\delta r^2} - \frac{1}{r}\frac{\delta}{\delta r} + \frac{\delta^2}{\delta l^2}.$$

If we first consider the motion to be unchanged along the tube, i.e. independent of l, then (1) becomes

$$\left(D - \frac{1}{\nu}\frac{\delta}{\delta t}\right).D\Psi = 0,$$

* *Sound*, p. 176; quoted by Warren, *loc. cit.*

† Cf. Lamb, *Hydrodynamics*, § 102.

‡ *Ann. d. Physik*, **87**, 570 (1928).

which, if Ψ is simple harmonic and $= e^{i\beta t}$, leads to two equations:

$$\frac{\delta^2}{\delta r^2}\Psi - \frac{1}{r}\frac{\delta}{\delta r}\Psi = 0$$

$$\frac{\delta^2}{\delta r^2}\Psi - \frac{1}{r}\frac{\delta}{\delta r}\Psi - \frac{i\beta}{\nu}\Psi = 0,$$

of which the solution (since v must be finite when $r = 0$, but be zero when $r = a$, where a is the radius of the pipe) is

$$v = -\frac{1}{r}\frac{\delta\Psi}{\delta r} = c\left\{J_0\left(\sqrt{\frac{-i\beta}{\nu}}\,a\right) - J_0\left(\sqrt{\frac{-i\beta}{\nu}}\,r\right)\right\} e^{i\beta t}.$$

In terms of Kelvin's "ber" and "bei" functions, this is equivalent to

$$v = c\,(\text{ber}\,\alpha a + i\,\text{bei}\,\alpha a - \text{ber}\,\alpha r - i\,\text{bei}\,\alpha r)\,e^{i\beta t},$$

where α is put for $\beta^{\frac{1}{2}}\nu^{-\frac{1}{2}}$.

Finally, the maximum velocity in the simple harmonic motion at any radius is given by

$$\bar{v} = c_1\{(1 - \text{ber}\,\alpha r.\text{ber}^{-1}\,\alpha a).\text{ber}\,\alpha a.\sin\beta t + i\,(1 - \text{bei}\,\alpha r.\text{bei}^{-1}\,\alpha a)\,\text{bei}\,\alpha a.\cos\beta t\} = f(r) \quad \ldots\ldots(2).$$

When α is small this gives nearly the Poiseuille distribution of velocity, but if α is given an increasing value corresponding to increasing frequency and to predomination of inertia over viscosity, substitution of the ber and bei functions from tables gives more and more pronounced peaks with fall to zero velocity at the walls, in correspondence with our curves.

The above treatment neglects the change in the distribution across a section hich takes place as one recedes to the piston. Assuming an exponential change, some such equation as

$$\bar{v} = f(r)\,\{(\kappa f(r)^{-1} - 1)\,e^{\gamma z} + 1\} \quad \ldots\ldots(3)^*$$

would give the necessary uniform velocity at the piston, and exhibit the change due to distance from the surface of the piston. If, to get the mean square velocity at any radius, we add the squares of the coefficients of $\sin\beta t$ and $\cos\beta t$ in (2) and substitute the known expansions of the ber and bei functions, i.e.

$$\text{ber}\,x = 1 - \frac{x^2}{2^2.4^2} + \frac{x^8}{2^2.4^2.6^2.8^2} - \cdots,$$

$$\text{bei}\,x = \frac{x^2}{2^2} - \frac{x^4}{2^2.4^2.6^2} + \cdots,$$

we shall obviously get for $\bar{v}$ a series of ascending even powers of r/a, when the latter is small. This is interesting, for Rayleigh, in an appendix to his *Sound*, tested an arbitrary distribution of velocity across the mouth of the orifice of a resonator of the form $\bar{v} = \bar{v}_0\,(1 + \mu r^2/a^2 + \mu' r^4/a^4)$, where v_0 is the central velocity, r is the distance of the point considered, and a is the radius of the edge of the flanged orifice, and he then determined μ and μ' so as to make the whole energy minimum. The velocity at the mouth being given by the above expression, that at infinity

* We are indebted to Mr A. M. Cassie, M.A., B.Sc., for suggesting this equation.

outside the tube being zero, and that at the piston ($x = -l$) being constant across the tube, then the resistance of the orifice is determined as the ratio of the difference of hydrodynamic potential across it to the current or velocity produced. For minimum energy ($\propto \Sigma v^2$) this resistance must be a maximum, a condition which

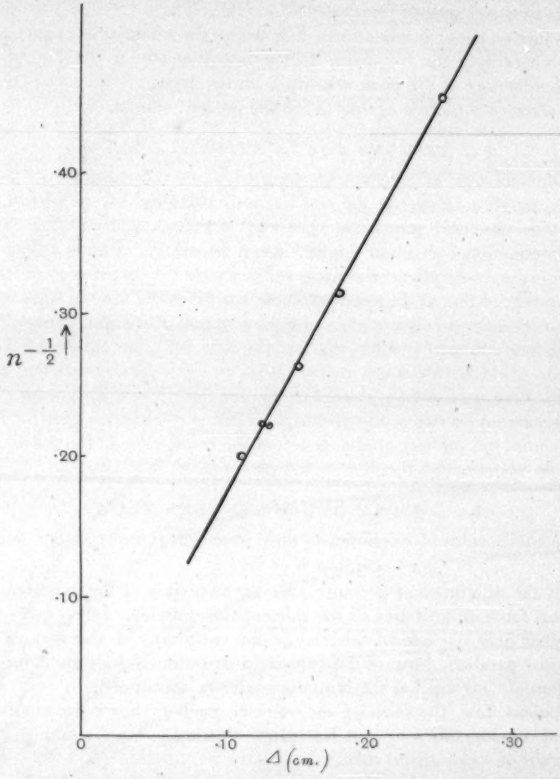

Fig. 7.

allows one to determine μ and μ'. This resistance Rayleigh co-ordinated with the "end correction," i.e. the distance from the end of the tube to the centre of the radial motion in the surroundings, and found that the best values of μ and μ' to fit the known practical end correction were 0 and 1·103 respectively.

Curiously enough, the expression

$$\bar{v} = v_0 (1 + 1{\cdot}1 r^4/a^4)$$

fits our experimental curves for the unflanged tube at the orifice very well (cf. Figs. 4 and 5, dotted lines). For a flanged tube μ' is considerably less. Also μ' diminishes to zero as one approaches the piston.

At these low frequencies a change in frequency merely increases every ordinate in the ratio of the change of frequency.

In the former paper it was shown that we might reasonably expect $\Delta n^{\frac{1}{2}}$ to be constant, where n is the frequency and Δ is the width of the boundary layer, taken as the distance of the peak velocity from the edge.

Fig. 7 shows $n^{-\frac{1}{2}}$ plotted against Δ for the present results.

§ 4. RESULTS FOR CONTINUOUS FLOW

A continuous flow of air through the two tubes was produced by a blower operating through a diffuser at the end formerly occupied by the piston. The hot wire was then traversed across the open end as before, and velocities calculated with the conduction correction applied when necessary. Fig. 8 shows a set of curves so obtained for different average velocities in the square-section tube.

It was expected that at the lowest of these velocities the flow in such wide tubes would be turbulent for Reynolds' number = 1000, so to get comparison with stream-line flow a tube of smaller (oval) section, Fig. 2 (*c*), was constructed, in which the hot wire could be traversed, by means of an external micrometer, across the centre of the tube, and remain parallel to the two flats. These flats were 1 cm. long and were separated by two semi-cylindrical surfaces of radius 0·4 cm.

Fig. 9 shows the velocity gradients across the small tube at Reynolds' numbers ranging from 15, at which the flow would presumably be stream-line, to 500.

§ 5. THEORY OF CONTINUOUS FLOW

For the equilibrium of a cylinder of fluid of length δl and radius r, we have

$$\pi r^2 . \delta p / \delta l = 2\pi r l . \tau,$$

where δp is the difference of pressure over the two ends of the cylinder, and τ is the frictional force on unit area of the sides of the cylinder.

In laminar flow $\tau = \eta dv/\delta r$, where η is the coefficient of viscosity. This leads to the familiar parabolic form of the velocity distribution across the cylinder when the condition of "no slip" at the boundary sides is introduced.

In turbulent flow, the form of the velocity gradient has been obtained from dimensional considerations by von Kármán*. Putting $y = r - a$, the distance out from the walls of a cylindrical tube,

$$v = f(\eta, \rho, \tau, y).$$

Imagine this to be expanded in powers of y, of which the first term is

$$v = f_1(\eta, \rho, \tau)\, y^q,$$

or, since $(\tau/\rho)^{\frac{1}{2}}$ and ν/y have the dimensions of velocity, we can put

$$v = B \left(\frac{\tau}{\rho}\right)^{\frac{1+q}{2}} . \left(\frac{\nu}{y}\right)^q \qquad \ldots\ldots(4),$$

* *Abhand. Aero. Inst. Aachen*, 1, 6 (1921).

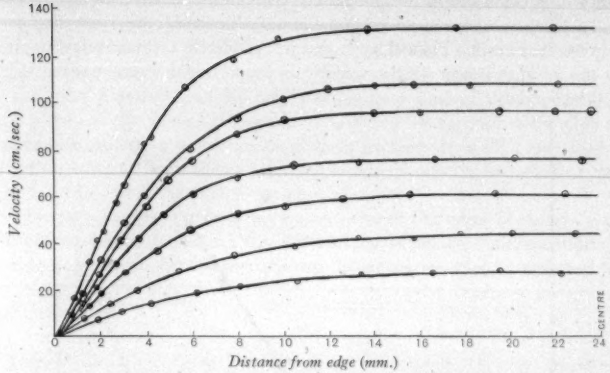

Fig. 8. One-way flow through square section pipe. Traverses 1 mm. inside opening.

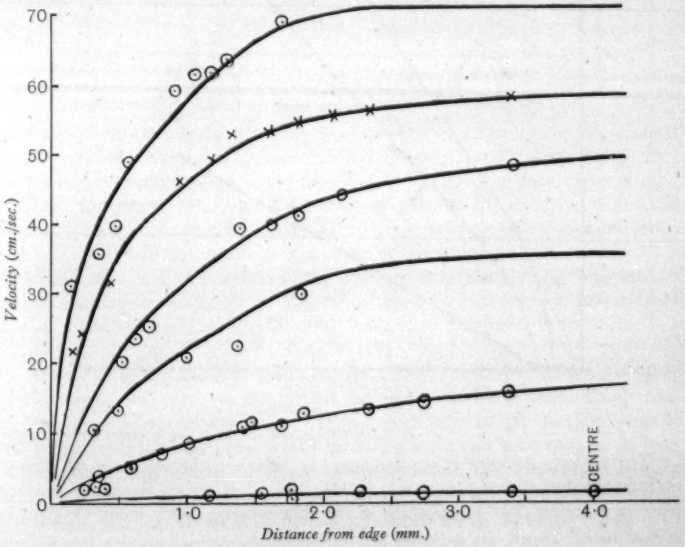

Fig. 9. Results for small oval pipe.

where B is a non-dimensional constant. To proceed further, we have to introduce the known variation of τ with v, or rather with outflow from a circular pipe. For this, von Kármán takes τ as proportional to $v^{7/4}$, whence $\frac{1}{2}(1+q)=4/7$, and $q=1/7$. This gives the so-called Prandtl law*, that in completely turbulent flow in circular pipes the seventh power of the velocity is proportional to the normal distance from the boundary.

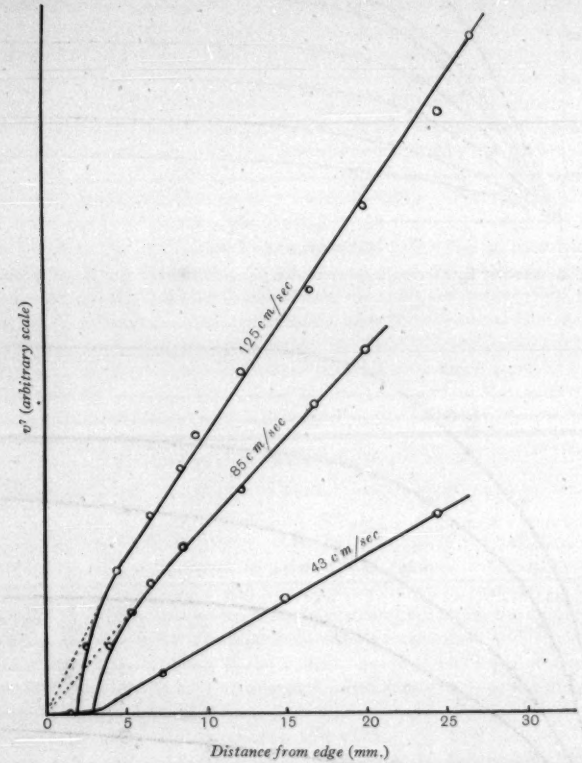

Fig. 10. The Prandtl law for three different central velocities (large round pipe).

The velocity gradient at the boundary is, however, infinite from (4), giving an infinite value of $\tau=\eta.dv/dy$. It is therefore assumed that laminar flow takes place in a region close to the walls of the tube wherein the velocity gradient is

* Apparently Prandtl had prevjously deduced this law but had not published it when von Kármán developed it further.

linear, merging into a gradient of the v^7 type in the body of the pipe. At high values of Reynolds' number the flow will more nearly approach the type $v = ay^{1/7}$ over the whole of the tube, or, alternatively, the laminar layer will get thinner, vanishing in the limit.

The first experiments on the velocity distribution in continuous flow in pipes were made by Stanton* and his collaborators by means of "disappearing Pitot tubes" of which one side of the orifice was formed by the wall of the pipe itself, the other side projecting to various small distances from the wall. These, calibrated in a known laminar flow, allowed measurements of velocity in turbulent flow to within 0·25 mm. from the wall of the pipe.

Nikuradse† traversed a Pitot tube across the end of pipes of various sections and about 2·5 cm. in diameter. Von Kármán has examined Stanton's results, and Nikuradse† his own results from the standpoint of the Prandtl law, and found

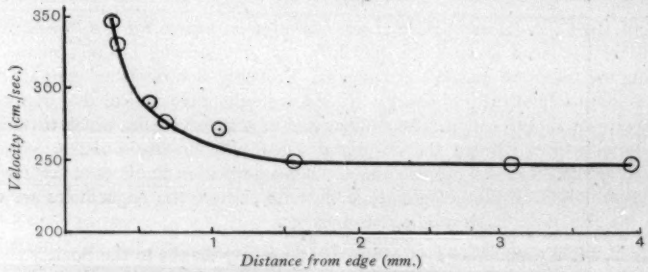

Fig. 11. Velocities at which stream became unsteady: small oval pipe.

good agreement outside the boundary layer (which Nikuradse's Pitot tube scarcely penetrated). We have tested our own results in the same way, by plotting v^7 against r for the higher average speeds in the pipe, Fig. 10. The best of these give a straight line from the centre of the pipe to within about 4 mm. of the walls, but within this boundary layer v is directly proportional to y. At intermediate speeds the actual distribution follows the fourth or fifth power of the velocity.

Incidentally, a determination of the pressure drop along a long pipe enables one to calculate τ and hence to find the numeric B, characteristic of turbulent flow, if the v^7 law be assumed. This was obtained by Nikuradse in the form

$$B^{-7/4} = 0{\cdot}022.$$

By way of confirmation of the change over from stream-line to turbulent flow in the narrow oval pipe, we determined the velocity at which unsteadiness commenced by connecting the hot wire through a transformer to a string galvanometer, and taking records of the deflection of the string at various wind speeds. As long as the velocity was steady the string was undisturbed, but at a critical velocity oscillation of the string pointed to incipient turbulence. Fig. 11 shows the incidence

* Stanton, *Proc. R. S.* **85**, 366 (1911). Stanton, Marshall and Bryant, *ibid.* **97**, 413 (1920).

† *Forschungsarbeiten Verein. Deut. Ing.* H. 281. See also Döuch, *ibid.* H. 282, and Schiller and Kirsten, *Zeits. tech. Physik*, **10**, 268 (1929).

of turbulence at various positions across the tube. It brings out well the fact that turbulence sets in over the whole of the tube at about the same velocity (250 cm./sec.), except within a millimetre or so of the walls, but that, as the mean speed goes up, this protective layer is reduced in thickness (to 0·3 mm. at 350 cm./sec.).

§ 6. ACKNOWLEDGMENTS

We wish to thank the Senate of the University of London for a grant from the Dixon Fund for the purchase of the Einthoven string galvanometer and the camera. Most of the practical work was carried out during 1928 at the College of Technology, Leicester, and we wish to acknowledge with gratitude the facilities which the authorities and Mr E. Langton, B.Sc., placed at our disposal in the Physics Department of that College.

DISCUSSION

Prof. Sir C. V. RAMAN said that when writing an article for the *Handbuch der Physik* he had been appalled by the difficulty of obtaining a direct method of treating the theory of musical instruments. Probably electrical analogies afforded the best approach to the problem, provided the results were subjected to correction by experimental observation. The annular region of high velocity which the author had found was so marked that it might conceivably be made visible by some suitable device. Had the author done any similar work on the flow of air through transverse holes? In the saxophone, for instance, where the finger holes are very large, the size of the holes might be important.

Mr C. M. WHITE: May I as a visitor express my thanks to the Society and to the authors of the paper for their account of a very interesting piece of work? My criticism of the paper—I feel that the authors have carried the desire for conciseness to a point at which it becomes almost a fault. I personally should have appreciated a more comprehensive account of the origin and aims of the work. I am in doubt also as to the extent to which the authors' aims have been realised. The apparatus too—experimental apparatus doesn't just happen—it is usually the result of careful design—a compromise between conflicting considerations—in their paper the authors give but little indication of the factors which led to the design of their apparatus. I for one should have found an account not only of interest but also of real value. I feel that there is this tendency towards undue briefness throughout the scientific writings of to-day, with the result that the reader is forced to narrow his field of interest. With regard to the results. The confirmation of the Rayleigh curves is most convincing and particularly valuable in view of the relative scarcity of sound theoretical treatments of the subject. Do the authors see any way to solve the problem of an alternating flow superimposed on continuous flow in long pipes? Data on this question would be of immediate value to engineers.

I am not yet convinced that the hot-wire method of measurement is sound quantitatively. In a sense, Fig. 4 is a proof of its unsoundness, for here the average velocity is zero, yet the wire shows definite cooling. I should expect a hot wire to

read high in a position where the flow is highly turbulent, and the upper curve of Fig. 8 seems to show evidence of this. I do not think the approximation to the seventh-power law shown in Fig. 10 is very convincing as a proof that the hot wire is reliable. This law is an approximation based on experiments with long smooth pipes. If the surface were rough, then the index would be 4, while a short streamlined orifice gives almost infinity as the index—the possible range therefore is very wide.

The phenomena described in connexion with Fig. 11 may be explained, I think, by the action of disturbances originally in the air as it enters the pipe—possibly these act in conjunction with the eddy-producing capabilities of the wire itself—but whether this is so or not the disturbance will only reach the wire at fairly high Reynolds' numbers—with small flows they will die out first. I think the term "critical velocity" has come to imply instability and is perhaps misleading in connexion with Fig. 11. Unsteadiness is present apparently long before the velocities of Fig. 11 are reached, as Fig. 9 shows considerable deviation from the Poiseuille distribution for all except the lowest two curves.

Dr W. N. Bond: The experiments by Carrière, mentioned in the second footnote of this paper, would be interesting if carried out for the space just outside the open end of the pipe, as the direction as well as the amplitude of the oscillations might be found. Departure from the v^7 law (Fig. 10), even at a distance from the pipe wall, was attributed by Mr White to the possible roughness of the pipe wall. I feel that the law is in any case not to be expected to hold accurately, as it may be affected by the distance from the entrance to the pipe, and also because it is based on the approximate assumption that $\tau \propto v^{7/4}$. I should like to ask the authors what were the characteristics of the beginning of turbulence as indicated by the string galvanometer. In some experiments in which I was able to hear the beginning of turbulence (for instance, using a stethoscope) it appeared to be quite random, and like a series of pistol shots. Finally, may I express my appreciation of the work the authors have brought before us?

Mr W. A. Benton said that, while the authors had clearly established the existence of the annular region of high velocity, it was difficult to picture the reason for this, from the point of view of common sense. Could the authors give any summary explanation of it, and show where the energy in the region came from?

Prof. W. Wilson: A simple illustration may perhaps assist in removing the difficulty mentioned by the last speaker. Imagine a wide cylindrical vessel filled with a liquid and having a small orifice on one side near its base. I will suppose for the moment that the viscosity of the liquid is zero or negligibly small. I believe it is easy to show, with the aid of Bernoulli's theorem, that the velocity in the emerging jet at points in the plane of the orifice is greatest at the edge and has a minimum value somewhere near the middle. A little outside the orifice, in the *vena contracta*, the velocity will not vary throughout the cross-section of the jet but (assuming no viscosity) will have the Torricellian value $v = (2gh)^{\frac{1}{2}}$. Actually

the liquid has viscosity; but if this is not too great it will not seriously affect the general character of the velocity gradient in the plane of the orifice except near the edge where one would expect a steep gradient like those shown in the authors' curves.

Mr A. M. Cassie: The peaks are not really difficult to explain. They are clearly analogous to the skin effect in an electrical conductor with the additional condition that the velocity must be zero at the walls of the tube. The skin effect in a conductor of circular cross-section, you will remember, can be shown to be the result of the variation of the mutual inductance between the elementary current cylinders with their radii. In the same way in the problem under discussion the force due to viscosity on an elementary cylinder varies with its radius. I find the two problems lead to the same Bessel equation, and making the velocity zero at the walls leads to the same shape of curve as that observed by Richardson and Tyler. The solution also shows that the sharpness of the peaks increases with the frequency and with the density of the gas while it decreases with the viscosity.

The President asked whether the authors had estimated the impedance of an aperture in an infinite plane wall or of the end of a pipe in an infinitely extended medium, utilising their results? The possible difference between the calibration of the hot-wire apparatus in turbulent and in stream-line flow had probably some bearing on the calculation of the impedance.

Authors' reply: We have no data for the flow through side holes to put before Sir C. V. Raman, but some qualitative measurements at a side hole in a sounding flute have shown the same effect. The theory of wind instruments having side holes has been considered by one of us as an exercise in summed impedances in an appendix to a recent book, but it has not yet been possible to work out the impedance of an orifice from theory, taking into account the annular effect, as suggested by the President.

We are inclined to agree with Mr White that the paper is somewhat compressed—at least as regards data—two of our graphs having been rejected as superfluous! As regards exposition, he must remember that it is a second paper on the subject, and we hope he will find the *raison d'être* of the second paper in the first. The points raised with regard to alternating plus continuous flow (which we hope to find time to investigate) and the use of a hot wire in a turbulent stream are very relevant. To the extent that turbulent motion can be regarded as continuous flow on which an irregular ripple has been superposed, the hot wire will indicate a slightly higher average (towards the peaks of the ripples) than in a steady draught. However, calibrations in a wind channel and on a whirling arm tally to an extent which seems to indicate that the difference is a small one. Vorticity would not be produced by the wire itself at speeds below 2000 cm./sec. Mr White views Fig. 10 in a novel light. We were not trying to test our hot-wire calibration by the v^7 law, but boldly assumed that our calibration was correct, and subjected

the v^7 law to the test in Fig. 10. We agree with Dr Bond and other speakers that the agreement may be deceptive; a v^6 or a v^5 law would look almost equally well on the graph, but we wanted to test the Prandtl-Kármán law, which, as Dr Bond remarks, is based on an assumption as to the resistance of pipes which has been only approximately verified in practice. Some analogies between a.c. electricity and ideal orifice flow were considered in the first paper and the discussion thereon. Mr Cassie's mathematical solution, which we have seen, is a neat alternative to that given in the paper. Finally, we would thank all speakers for their kind interest.

RESOLVING POWER TESTS ON MICROSCOPE OBJECTIVES USED WITH ULTRA-VIOLET RADIATION

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Read and discussed October 25, 1929.

ABSTRACT. The work described was conducted with the view of determining the extent to which the increased resolving power which should be obtainable by the use of ultra-violet radiation instead of visible light is realisable with modern microscope object-glasses, computed for use with ultra-violet radiation. The method, which provides a quantitative rather than a qualitative test on an objective, was first developed for visual lenses and has now been adapted for use with ultra-violet objectives. An object of known regular structure and of variable interval is produced by projecting in the object-plane of the lens under test a reduced image of a grating, the apparent line separation of which is varied by rotation of the latter; thus the line interval can be determined when resolution just ceases. The results show that the fused quartz monochromatic object-glass of numerical aperture 0·35, computed for and used with radiation of wave-length 0·275μ, gives nearly twice the resolving power of a lens of similar aperture computed for and used with light of wave-length 0·51μ; while the fused quartz monochromatic lens of numerical aperture 1·2 has a resolving power 70 per cent. higher than that of a well-corrected object-glass of the same numerical aperture when used with light of wave-length 0·51μ.

§ 1. PURPOSE OF THE WORK

In a paper* read before the Royal Microscopical Society in April 1928, the writer described a method whereby a numerical value for the optical resolving power of microscope objectives could be obtained. Full details of the method will be found in that paper, but briefly it consisted in forming a reduced image of a relatively coarse grating either by an auxiliary microscope objective or by the lens itself (i.e. with a vertical illuminator), and using this image as the object for the lens under test. By rotation of the grating the apparent distance between the lines of the object is varied and a position found when resolution just ceases, from which the line separation is obtained.

As it was possible by this means to obtain a quantitative rather than a qualitative test (such as is given by observations on natural objects) on the performance of a lens, the method was further developed for use with the quartz monochromat series of lenses computed for a wave-length in the ultra-violet region of the spectrum ($\lambda = 0{\cdot}275\mu$), the aim being to determine how much increased resolution really was being obtained by the use of a shorter wave-length.

* *Journ. Roy. Micro. Soc.* **48**, 144–158 (1928).

§ 2. THEORY OF THE METHOD EMPLOYED

The formation of a reduced image produced by a lens system when the object is rotated will be readily remembered by reference to Fig. 1. Lateral and axial magnifications being proportional to M and M^2 respectively, it follows that the axial length l' of the reduced image $x'y'$ will be equal to l/M^2, and therefore will be distinctly small in comparison with l. Provided l' be less than the depth of focus of the lens to be tested, the image $x'y'$ acts as a sensibly flat object, and the spacing of the lines of this imaged object will be proportional to the cosine of the angular swing of the grating xy.

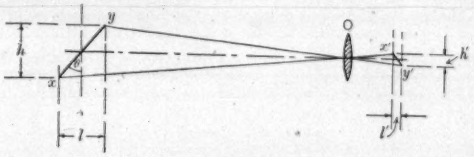


Fig. 1. Image of inclined object.

The depth of focus in the object space of an objective being equal to

$$\lambda/4n \sin^2 (U/2)^*$$

(where U is the angular semi-aperture of the cone of rays entering the lens), it will be seen that the requirement that l' shall not exceed this depth of focus imposes a necessary limit on the maximum permissible rotation of the grating; therefore it is desirable to choose a grating such that resolution will cease (for the lens under test) before the grating reaches this maximum angular position. Also it is necessary to limit the field in which the imaged grating lines are formed: this is done by means of a line ruled on a silvered cover glass or alternatively by limitation of the width of the grating proper.

Example. In the case of the two ultra-violet lenses used in these tests, where the imaged object is formed by a 2 mm. immersion monochromat; if the reduction is 90 times, the length xy of the portion of the grating used is 1·7 mm. and if it is rotated through 60° for example, then l' will be found to be equal to 0·00018 mm. or 0·18μ.

If a 6 mm. monochromat were to be used to view this image, then its depth of focus $= \lambda/4n \sin^2 (U/2) = 2{\cdot}2\mu$. Thus l' is well within the permissible value for producing a flat object for the 6 mm. lens under test. If the lens under test is a 2 mm. monochromat, its depth of focus is 0·2μ and therefore the imaged object as before will still serve for testing the last named lens.

On this principle, therefore, from a knowledge of the size of the grating space in the imaged object when the grating is normal to the optical axis, and by determination of the angle at which resolution of the lines just ceases, it is possible to obtain a numerical value for the resolving power of a given objective, for if d is

* See section on "Optics of the Microscope" (Prof. A. E. Conrady) in *Dict. Applied Physics*, **4**, 218–223.

the size of a grating space of the reduced image when the grating is normal, and if θ is the angular turn of the grating when resolution ceases, then

$$d_\theta = d \cos \theta.$$

A verification of the validity of this simple formula was obtained when the tests were made with objectives used in visual light; a full discussion of the theory, together with experimental confirmation, was given in the paper first referred to above, and need not be repeated here. As a matter of interest, however, a summary of the resolving-power values, as determined by the method for three visual objectives is given in the table.

Table 1. Resolving powers of three visual objectives.

Objective (nominal)	Numerical aperture	λ	Observed resolution, mm.	Theoretical resolution mm.
$\frac{2}{3}$ in.	0·28	0·51 μ	0·00094	0·00091
$\frac{1}{6}$ in.	0·71	0·51 μ	0·00039	0·00036
$\frac{1}{12}$ in. oil	1·2	0·51 μ	0·00024	0·00021

§ 3. EXPERIMENTAL WORK

Owing to the exacting character of the practical work with the ultra-violet lenses it is thought that some details of this may be worthy of description. The apparatus took the form previously described by Dr L. C. Martin and the author*, the microscope stand being that designed by Mr J. E. Barnard. Modifications

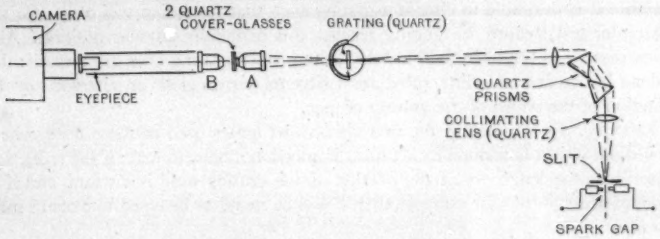


Fig. 2. Schematic diagram of the optical system.

to meet the requirements of the tests had to be made, and Fig. 2 shows diagrammatically the arrangement of the components. All the optical parts were of quartz. The fittings shown on the right of the figure constitute a monochromator by means of which the light from a high potential spark, of the order of 10,000 volts, formed between cadmium electrodes is dispersed sufficiently to bring one line (namely the 0·275μ line) of the cadmium spectrum on to the quartz grating. An image of the latter† is formed by the objective *A* and this image is in turn observed by the lens *B* under test.

* "Ultra-violet Microscopy," *Journ. Sci. Inst.* 5, 337–344, 380–386 (1928).

† It will be noted that this is an *enlarged* image.

§ 4. TESTING OF 6 MM. MONOCHROMAT

For the test on the low power quartz lens, the 2 mm. Beck immersion monochromat was used as the object-forming lens, and the microscope stand was reversed, see Fig. 3; the purpose of this arrangement was to make use of the specially constructed fine adjustment (reading to 0·1 μ) for the high power lens, and of the somewhat coarser adjustment (provided for the condenser) as the focussing movement for the 6 mm. objective, for which the depth-of-focus requirements are not so exacting as for the former lens. The figure illustrates the way in which the sub-stage condenser mount has been converted into the microscope proper, the tube *M* (carrying the quartz eyepiece) fitting conveniently into the

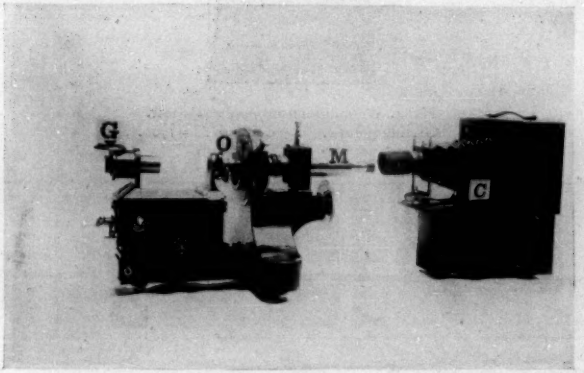


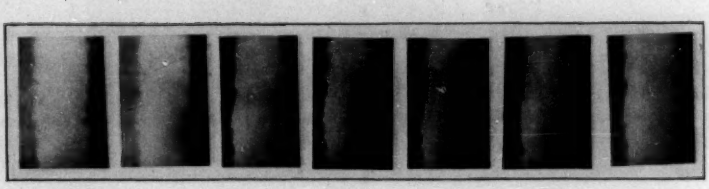
Fig. 3. Reversal of microscope for 6 mm. object glass test.

mount. The immersion objective *O* is also seen, whilst the grating *G* with its divided circle is held in a suitable fitting in the position usually occupied by the eyepiece. A quartz slide was specially prepared: this consisted of two fused quartz cover glasses (one of which was silvered and had a ruled line 0·008 mm. wide) cemented together with a thin film of glycerin between them; the line served as the object on which the 6 mm. lens was focussed. Focussing was facilitated by illumination of the object in the way already described; the line was focussed roughly with the fluorescent eyepiece and a series of photographs was then taken to obtain the exact focus. Without further alteration to this end of the microscope stand, the grating was placed in position (*G*, Fig. 3) at the correct tube-length for the 2 mm. monochromat, the latter being already in place and acting as a condenser for the first part of the experiment. By observation through the fluorescent eyepiece, the 2 mm. lens could now be moved by means of its own fine adjustment until a rough outline of the grating was seen approximately in focus with the line

object, and then a further series of photographs was taken, the focus of this lens being varied by 0·2 μ each time until the precise focus of the grating lines was found.

The line on the silver film and the imaged lines of the grating having been obtained simultaneously in focus on the plate*, the grating was then rotated through successive angular amounts and a series of some twenty exposures were

Fig. 4. 6 mm. monochromat under test.
Grating normal. $\lambda = 0{\cdot}275\mu$. $M = 1700$.



44° 43° 42° 41° 40° 39° 38°

Fig. 5. Successive rotations of grating. 6 mm. monochromat.
Resolution ceases at 42°. $M = 1250$.

taken on a film as before. On development of this the angular position of the grating at which resolution ceased could be found, and the object-interval determined. Fig. 5 shows seven successive exposures of the series thus taken. It is feared that the photomicrographs are not very suitable for reproduction. The originals will be gladly shown to anyone who is interested.

Details of the actual figures obtained with this lens are given below:

6 *mm. quartz monochromat.*

Numerical aperture: 0·35.
Wave-length: 0·275μ (Cd spark).
Object-forming lens: 2 mm. quartz monochromat.
Grating employed: 20 lines to the mm. ruled on a silvered quartz plate.

* Fig. 4 shows the actual photomicrograph of this.

With the grating normal to the optical axis, the line interval of the imaged object, determined photographically, was 0·00053 mm.

Photographs were taken at 32°, 33°, 34°, 35°, 36°, 37°, 38°, 39°, 40°, 41°, 42°, 43°, 44°, 45°, 46°, 47°, 48° and 49°. Resolution ceased at 42°.

$$\therefore \text{ line interval } d = 0{\cdot}00053 \times \cos 42^\circ = 0{\cdot}00039_3 \text{ mm.}$$

By check method.

Width of line on silver film: 0·008 mm.

Number of lines immediately before resolution ceases: 20.

$$\therefore d \text{ (by check)} = 0{\cdot}00040 \text{ mm.}$$

Theoretical resolution: $0{\cdot}00039_1$ mm.

The result indicates clearly the advantage of using the shorter wave-length, as the resolving power of this objective is the same as that which it is possible to obtain with an objective of double the numerical aperture (namely a $\frac{1}{8}$ in. object-glass of numerical aperture 0·71), used with light of wave-length 0·51μ.

§ 5. SOME EXPERIMENTAL DETAILS

Mention must here be made of certain details, only briefly alluded to in the foregoing description of the method, but such that without them it would not have been possible to carry out the tests.

First of these, was the use of the stable immersion fluid for the 2 mm. monochromat. A full description of this glycerin and cane-sugar solution is given in the *Journal of Scientific Instruments*, in the article already referred to, and the test was made possible by the fact of being able to use this fluid for long periods—several hours—without the occurrence of any change in its refractive index. Obviously, considerable time is taken both in focussing with each objective in turn and also in taking a series of grating rotations, all three operations being photographic ones; and to have used the originally intended immersion fluid, namely glycerin and water, would have added to the difficulties very greatly, its hygroscopic nature (affecting its refractive index) produces a continual change in focus of the image.

Secondly, the preparation and making of the gratings and object-slide may be of interest. The former were ruled on silvered quartz slides by an attachment fitted to the geometric slide of a measuring microscope illustrated in Fig. 5 of the paper referred to in the footnote on page 16. With this three gratings were made, namely: (*a*) 20 lines per mm., grating space 0·05 mm.; (*b*) 40 lines per mm., grating space 0·025 mm.; (*c*) 67 lines per mm., grating space 0·015 mm. The first was ruled with a ground steel tool, whilst a carborundum chip, suitably mounted, was employed for the ruling of the latter two. A splinter from a carborundum crystal can be made to produce extremely fine and clean lines on silvered surfaces, and it was with such a splinter that the line 0·008 mm. wide was ruled for the object-slide. The use of a platinised surface aids in the obtaining of a clean cut, and it was found possible to rule lines with a carborundum splinter on such a surface of 0·002 mm. in width; further a platinised surface, produced by the cathode process, is more opaque

than silver to wave-length 0·275μ and is therefore better from this point of view also. Grating (*c*) was ruled on such a surface.

Thirdly, the best tube-length for each objective was determined by means of ultra-violet photomicrographs of the minute holes present in a chemically deposited silver film. If a series of photographs for a number of separate tube-lengths be taken, one will be found where the extra-focal images are similar in appearance, and this represents the best tube-length.

§ 6. TESTING OF 2 MM. MONOCHROMAT

The general outline of the method for testing the immersion quartz objective is similar. In this case, however, another immersion monochromat had to be used

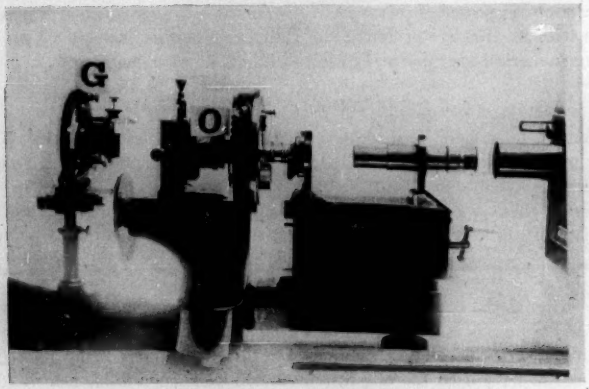


Fig. 6. Apparatus when testing 2 mm. quartz monochromat.

for the object-forming lens and therefore it was necessary to obtain a lens which was at least as good in performance as the one under test. Consequently, a qualitative test was first carried out by the taking of comparison photomicrographs of a silver-film object with each objective* in turn; the results obtained showed that both the definition and resolution of the silver granules produced by each of the lenses were practically identical.

The arrangement of the apparatus for the resolving power test is seen in Fig. 6. As the objectives were both immersion lenses it was not necessary to have an object slide on the microscope stage; the space between the two front lenses of each objective was filled with immersion fluid. This was effected by means of a suitable bath formed by a flexible casing of thin rubber attached to the metal work

* One lens was the 2 mm. Beck monochromat and the other a Zeiss 2 mm. monochromat.

of each lens (see Fig. 7). Further, as no object slide was employed, it was necessary only to focus with one lens, namely that which was attached to the special fine adjustment movement of the microscope, whilst the other lens remained stationary in the sub-stage condenser mount.

The grating was set up as before (see Fig. 2) normal to the optical axis and at the correct tube-length distance for the object-forming lens; it was then illuminated with the 0·275 Cd line and by use of the fluorescent eyepiece it was possible to focus the objective under test sufficiently well for the image of the outline of the grating to be seen, although no lines were visible. The camera was placed in position and a series of photographs was taken, the focus being varied by 0·2μ each time: the best focus was thus determined and the lines were clearly resolved on the plate. The grating was then rotated and a further series of photographs was taken

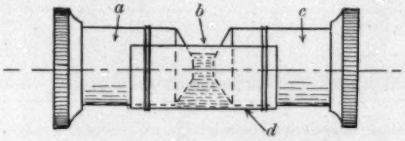


Fig. 7. Method of supporting immersion liquid between the two monochromats.

a 2 mm. monochromat. *b* Immersion liquid bath.
c 2 mm. monochromat. *d* Thin rubber casing.

for successive increases in its angular position, the point being found when resolution ceased. When the grating was near this position it was felt desirable to make perfectly sure that the lines were still precisely in focus, on account of the author's previous experience with such a test when using a $\frac{1}{12}$ in. oil immersion lens in visual light, and therefore for each orientation of the grating from then onwards a series of five photographs was taken, the focus being altered each time in order to secure the best setting. An illustration of this is given in Figs. 8, 8 (*a*), 8 (*b*), where it will be seen that the centre photograph in each case is in focus, whilst the two outer ones are not. By taking this precaution one felt more confident that a wrong resolution value due to incorrect focussing of the grating lines would be obviated.

Two entirely independent measurements by the method were made, one with a grating of 0·025 mm. interval, and the other with one of 0·015 mm. line space, the limit of resolution in each case lying between 0·00012 mm. and 0·00013 mm.

2 mm. quartz monochromat.

Numerical aperture: 1·2.

Wave-length: 0·275μ (Cd spark).

Grating—interval: 0·025 mm.

Magnification of microscope proper, determined photographically: 840.

With the grating normal to the axis the line interval of the imaged object was $0{\cdot}00027_2$ mm.

When the grating was rotated to 60° the line interval d was $0{\cdot}00027 \times \cos 60°$ or $0{\cdot}00013_5$ mm. The lines were still resolved.

With the grating at 63°, $d = 0{\cdot}00027 \cos 63° = 0{\cdot}00012_2$ mm., the lines being no longer resolved.

By check method.

Grating—interval: 0·015 mm.

Magnification: 840.

With the grating normal to the axis, the line interval of the imaged object was $0{\cdot}00017_1$ mm.

Fig. 8 (*a*). Object interval $0{\cdot}00013_5$ mm. Clearly resolved. 2 mm. monochromat. $\lambda = 0{\cdot}275\,\mu$. $M = 3260$.

Fig. 8 (*b*). Object interval $0{\cdot}00012_4$ mm. Not resolved. 2 mm. monochromat. $\lambda = 0{\cdot}275\,\mu$. $M = 2600$.

With the grating rotated to 38°, the line interval was $0{\cdot}00017 \times \cos 38°$ or $0{\cdot}00013_4$ mm. This was still resolved.

With the grating rotated to 43°, $d = 0{\cdot}00017 \times \cos 43° = 0{\cdot}00012_4$ mm., the lines being no longer resolved.

Theoretical resolution: $d = 0{\cdot}00011_4$ mm.

This test, however, where the two lenses are of almost the same numerical aperture, may be looked upon as a test of the two lenses combined, for the limit of resolution as measured will be that given by the poorer objective, and thus a possible objection to the method might be raised. If, for instance, the image-forming lens had errors which were just compensated by errors in the lens under test, or conversely were not corrected by corresponding errors of the latter, in either case an incorrect value for the resolving power of the lens under test would be obtained. This objection can be met when it is possible to use a good objective of higher numerical aperture as the object-forming lens. For this reason it was hoped to employ the vertical illuminator method, depicted in Fig. 9, which was

used so successfully with visual objectives. For although, on account of the light's travelling twice through the lens, all aberrations would be doubled, with a consequent lowering effect on the resolving power, nevertheless the vertical illuminator method has the merit of providing a relative test for objectives of the highest possible numerical aperture. Incidentally it has the additional advantage of using only the one object-glass instead of two.

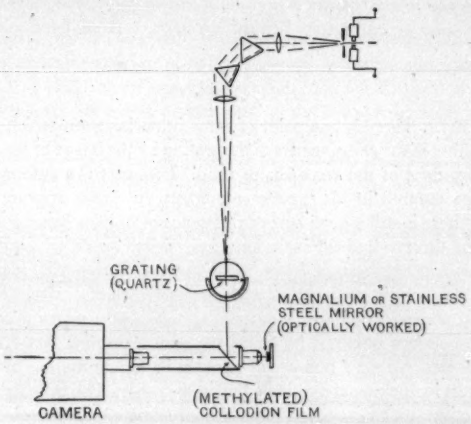


Fig. 9. Diagram of vertical illuminator method.

Although this form of the test was carried out quite satisfactorily for a 2 mm. immersion lens in visible light, it was not found possible to do this for either of the two available high-power ultra-violet objectives, the reason being that the design of the component lenses of their systems is such that an unusually large percentage of the incident radiation is reflected back along the axis of the microscope, thus making it almost impossible to obtain satisfactory photographs.

Hopes had also been entertained of carrying out a further test, by the use of finely ruled gratings on quartz and by direct observation of these with the 2 mm. monochromat, but it has not yet been possible to obtain a satisfactory ruling of the necessary fineness.

In any case, the test as it was made reveals the fact that the two lenses together will resolve two objects separated by a distance of $0{\cdot}13_8\,\mu$, and shows therefore that a distinct advantage is being obtained over a $\frac{1}{12}$ in. objective, used in visible light, whose resolving power is $0{\cdot}21\,\mu$.

§ 7. CONCLUSIONS

The work carried out indicates that the method can be applied for determining a numerical value for the resolving power of objectives used with ultra-violet radiation. It further illustrates that the use of shorter wave-lengths with micro-

scope objectives is justified, inasmuch as it increases the resolution obtainable, although the difficulties involved both in the manufacture and in the manipulation of the apparatus are at present admittedly great. It will be noticed that the 6 mm. monochromat gives almost its theoretical resolution, but the 2 mm. does not go so near to that limit; nevertheless the fact remains that something of the order of a 70 per cent. increase in resolving power has been obtained over what is physically possible with the highest-power objective in visible light, and therefore it is felt that in this respect at least a step forward has most certainly been made.

DISCUSSION

Mr T. Smith. There is one point of some importance on which this paper is silent—the filling of the effective part of the grating by the image of the spectroscope slit formed by light of the wave-length used. I suggest the author should state the conditions secured in his experiments when the paper appears in the *Proceedings*. As there is still a good deal of difference of opinion among microscopists on microscope theory—and the inclusion of physicists would widen the field—the results Mr Johnson has obtained for the ultra-violet resolution with quartz objectives should be considered in conjunction with the corresponding visual tests given in Table 1. The observed values in both cases are reasonably near the corresponding theoretical resolutions obtained by using the same empirical factor. Exact comparison is not possible for a number of reasons, including the use of a wide band of wave-lengths in one case and a single line in the other. The conclusion, however, that the objectives are good enough to yield approximately the increase in resolving power that might be hoped for by the use of ultra-violet light is justified, and this result should encourage the makers in any further developments of such objectives. Mr Johnson is to be congratulated on his success in making such very difficult measurements.

Mr W. W. Barkas. I should like to ask whether the increased resolving power obtained by the use of ultra-violet light would make it possible to obtain real images of colloidal particles with a view to measuring their radii directly. It is really a question of the time of exposure required since in liquid media the Brownian motion is fairly rapid.

Dr L. C. Martin (communicated). A quantitative test on the resolving power of ultra-violet microscope objectives of high numerical aperture has hitherto offered the greatest difficulties. Efforts have been made to produce test rulings of the order of 200,000 lines to the inch, but we have not been able, so far, to obtain a satisfactory object of this kind to test the resolving power. Moreover it is somewhat difficult with a grating of the ordinary type to determine the optimum performance of the objective, which may have to be stopped down in order to secure sufficient contrast in the image. Mr Johnson's "grating" with its transparent and opaque spaces lends itself more readily to experiments at full aperture. The deficiency in the actual as compared with the theoretical performance of the

objectives is almost certainly due, in my opinion, to the residual lack of homogeneity of the fused quartz. It is an open question whether, in view of all the increasing difficulties of manipulation, and this lack of homogeneity, it would pay, at present, to try to construct objectives computed for use with wave-lengths shorter than $\lambda = 0{\cdot}275\,\mu$, but if it is tried then Mr Johnson's test will give ready information as to the result.

Author's reply. In reply to the point raised by Mr Smith as to the method of filling the effective part of the grating with the desired wave-length, I fear that I did not originally make it sufficiently clear—although the monochromator in Fig. 2 partly suggests it—that an *enlarged* image of the 275 Cd line was formed in the plane of the grating. Its size is proportional to the ratio of the focal length of the telescope lens to that of the collimator lens used in the monochromator, which was in this case about 3. As the actual width of the enlarged image was approximately 6 mm. and the necessary width of the grating 2 mm., the total effective area of the grating was always filled with the required wave-length. I would like to thank Mr Smith for his appreciative remarks concerning the difficulties of the work. Colloidal particles of the size referred to by Mr Barkas are far below the capabilities of resolution of even the ultra-violet microscope. As is well known, the light scattered by such particles may be rendered visible by the use of the modern high-power dark-ground illuminator employing visible light, but it is nevertheless practically impossible to take satisfactory photomicrographs of these owing to the Brownian movement. In the case of the ultra-violet microscope the only way of recording a precise image is by photography, and as the length of exposure required for dark-ground ultra-violet illumination is at present far from "instantaneous", I am afraid there is little hope of obtaining the information (such as the diameter of these particles) in which Mr Barkas is interested, even with this microscope.

SOME ADDITIONAL LINES IN THE SECONDARY SPECTRUM OF HYDROGEN

F. C. CONNELLY, A.R.C.S., B.Sc., D.I.C.

Communicated by Prof A. Fowler, F.R.S., June 22, 1929.
Taken as read, October 25, 1929.

ABSTRACT. The author had measured about 1000 lines in the hydrogen spectrum between λ 4735 and λ 4018 when the similar work of Gale, Monk and Lee was published in 1928. The author's list included, however, about 280 lines not given in the prior list, and these are published in the present paper.

§ 1. INTRODUCTION

Before the publication in March 1928 of Gale, Monk and Lee's "Wavelengths in the Secondary Spectrum of Hydrogen*," the usual standards of reference used in attempts to analyse the spectrum had been the tables published by Merton and Barratt† some seven years before. These tables, however, were not complete, as a large number of fainter lines were not measured and numerous brighter ones were not completely resolved into their components. Tanaka‡ and Deodhar§ filled in some of these gaps, but do not appear to have used standard iron lines for calibration purposes, having based their measurements on some of the stronger hydrogen lines measured by Merton. These additional tables are therefore less accurate than the original ones. The dispersion used by Merton, 10 Å. per mm., was quite moderate so that the limit of resolution appears to be 0·3 Å. From attempts which have been made to select bands in the spectrum, this lack of resolution seemed a serious defect, and it was with the object of clearing up the difficulties in the blue and violet that a remeasurement of these regions was commenced early in 1927.

In all about 1000 lines were measured between 4735 Å. and 4018 Å. Many of those given by Merton as single were found to be double or even triple and numerous new lines were measured. The publication of these values was delayed pending further experiments, and in the meantime the tables by Gale, Monk and Lee, mentioned above, have appeared and have been accepted by most workers as satisfactory standards. These values have been compared carefully by the present author with his own values and in nearly all cases the agreement has been

* *Astrophys. Journal*, **67**, 89 (1928).
† *Phil. Trans.* A, **222**, 69 (1921).
‡ *Proc. R. S.* A, **113**, 420 (1926).
§ *Proc. R. S.* A, **108**, 592 (1925).

very good. No useful purpose would therefore be served by publishing the complete tables. As, however, a number of faint lines are not given in Gale, Monk and Lee's tables, it has been felt that these additional lines may be found valuable in extending bands already discovered and they are accordingly given below in Table 1.

A few lines measured by Gale, Monk and Lee, and given in their tables, have not been found by the author. These lines are printed below in Table 2.

§ 2. EXPERIMENTAL METHOD

The source of the spectrum in these determinations has been an H-tube of the usual type having a capillary 8 cm. long and 1 mm. in diameter. The type of tube used by Wood*, and designed by him to give the secondary spectrum at its strongest, has also been used, but the extra complication involved in water-cooling the electrodes, a precaution which was necessary when 2 or 3 kw. were to be dissipated in the tube, greatly offset the advantages of shorter exposure and freedom from the Balmer series attainable with the special tubes. Indeed the presence of the line spectrum was not disadvantageous, nor was the exposure unduly long when the H-tubes were used, so that the latter were generally adopted.

The effect of pressure on the spectrum is appreciable, the optimum pressure with these tubes being 2 or 3 mm. of mercury. This appears to be higher than the pressure used by Gale, working with a larger tube similar to that of Wood.

With a 10 ft. concave grating a dispersion of 2·5 Å. per mm. was obtained in the second order, and an exposure of 2 hours was sufficient to give a strong photograph on Imperial ordinary plates (H and D 90). The measurements were not, however, solely dependent on the grating plates as a two-prism Littrow instrument was used for checking many of the lines. This instrument was specially arranged for the purpose and gave a dispersion of 1 Å. per mm. at 4000 Å. and 2 Å. per mm. at 4400 Å. It was slower than the grating, and owing to slight optical defects of the prisms a slight blurring of the stronger lines could not be prevented from becoming apparent under high dispersion. This defect prevented the resolution from being any better than that yielded by the grating.

§ 3. DISCUSSION OF RESULTS

In the original tables drawn up by the author, wave-lengths were given to 2 or 3 decimal figures according to their probable accuracy. As the additional lines given below are the faintest ones, capable of least accurate measurement, most of the values are given to 2 decimal places only.

The line given as 4450·346 Å. by Gale, Monk and Lee is presumably a misprint for 4451·346 Å., this being in good agreement with the author's value 4451·35. Where a line has previously been measured by Deodhar or Tanaka, the fact is indicated by D or T against it.

* *Proc. R. S.* A, **97**, 455 (1920); *Phil. Mag.* **6**, **251**, 729 (1921).

§ 4. NOTATION

The system of notation used in this work is that adopted in the "Revision of Rowland's *Preliminary Table of Solar Spectrum Wavelengths*" (1928), the faint lines being denoted by 0, − 1, − 2, − 3.

§ 5. ACKNOWLEDGMENT

In conclusion the author wishes to express his thanks to Prof. A. Fowler for his continued interest and assistance throughout these measurements.

NOTE ADDED OCTOBER 1929

During the preparation of this paper further wave-lengths were published by Finkelnburg*. As, however, many lines remained unmeasured, it was decided not to recast the paper.

* *Zeits. für Physik*, **52**, 27 (1928).

Table 1. Lines measured by the author and not given by Gale, Monk and Lee.

Wave-length	Wave-number *in vacuo*	Wave-length	Wave-number *in vacuo*
4725·23 (− 3)	21157·08	4540·66 (− 2)	22017·08
4703·88 (− 3) D	21253·11	4539·60 (− 2)	22022·22
4696·31 (− 3)	21287·38	4538·67 (− 1)	22026·73
4695·70 (− 3)	21290·14	4536·87 (− 3)	22035·46
4695·12 (− 3)	21292·77	4536·40 (− 2)	22037·75
4694·78 (− 3)	21294·31	4535·12 (− 1)	22043·98
4682·59 (− 2)	21349·95	4532·18 (− 3)	22058·27
4676·73 (− 1)	21376·50	4531·64 (− 2)	22060·90
4668·573 (2)	21413·84	4530·53 (− 1)	22066·30
4664·82 (0)	21431·07	4528·75 (− 2)	22074·98
4654·76 (− 1)	21477·39	4524·72 (− 3)	22094·63
4646·94 (− 3)	21513·53	4524·50 (− 3)	22095·71
4646·40 (− 2)	21516·04	4522·09 (− 3)	22107·48
4644·19 (− 1)	21526·27	4521·80 (− 3)	22108·91
4637·68 (− 1) D	21556·49	4521·10 (− 3)	22112·33
4635·98 (− 3)	21564·40	4498·95 (− 3)	22221·19
4630·95 (− 3)	21587·81	4494·49 (− 3)	22243·25
4623·26 (− 1) T	21623·71	4494·19 (− 2)	22244·73
4622·73 (− 2)	21626·20	4492·21 (− 3)	22254·53
4621·15 (− 3)	21633·59	4491·29 (− 2)	22259·09
4619·09 (− 3)	21643·24	4491·12 (− 2)	22259·93
4615·64 (− 3)	21659·43	4490·62 (− 1)	22262·42
4615·21 (− 3)	21661·44	4489·17 (− 1)	22269·50
4610·76 (− 3) D	21682·35	4488·96 (− 1)	22270·64
4610·07 (− 3)	21685·59	4480·40 (− 1)	22313·19
4606·53 (− 3) D	21702·25	4478·38 (− 1) *d*	22323·26
4601·37 (− 2)	21726·59	4477·62 (− 3)	22327·05
4595·54 (− 3)	21754·16	4476·72 (− 3)	22331·54
4592·21 (− 1)	21769·94	4473·42 (− 3)	22348·01
4591·47 (− 1)	21773·43	4472·86 (− 2)	22350·80
4590·21 (− 1)	21779·42	4468·91 (− 2)	22370·56
4588·08 (− 3)	21789·53	4468·55 (− 3)	22372·37
4586·96 (− 1)	21794·84	4468·11 (− 2)	22374·57
4586·23 (− 3) T	21798·31	4466·23 (− 2)	22383·98
4586·00 (− 2)	21799·40	4462·93 (− 2)	22400·54
4585·60 (− 2)	21801·30	4462·53 (− 3)	22402·55
4580·47 (− 1)	21825·73	4458·01 (− 2)	22425·29
4576·94 (0)	21842·56	4454·34 (− 1) D	22443·74
4573·34 (− 3)	21859·75	4449·38 (− 2) T	22468·75
4570·40 (− 2)	21873·81	4442·07 (− 2)	22505·73
4569·18 (− 3)	21879·65	4441·11 (− 1)	22510·58
4565·10 (− 2)	21899·21	4440·12 (− 3)	22515·61
4563·14 (− 3)	21908·61	4436·81 (− 3)	22532·40
4561·01 (− 2)	21918·85	4434·09 (− 1)	22546·23
4559·13 (− 1)	21927·88	4433·72 (− 3)	22548·10
4556·74 (− 2)	21939·39	4433·41 (− 1)	22549·68
4556·49 (− 1)	21940·59	4433·05 (− 2) D	22551·52
4555·32 (− 3)	21946·23	4432·03 (− 3)	22556·71
4552·72 (− 1)	21958·76	4431·82 (− 3)	22557·78
4552·65 (0)	21959·09	4429·90 (− 3)	22567·56
4548·93 (− 2)	21977·05	4429·56 (0)	22569·29
4545·48 (− 2) D	21993·75	4428·23 (− 3)	22576·06
4545·19 (− 2)	21995·13	4425·35 (− 3)	22590·76
4544·85 (− 2)	21996·78	4424·93 (− 2)	22592·90
4544·163 (1)	22000·11	4423·68 (− 2)	22599·29
4542·17 (− 2)	22009·77	4421·57 (− 1)	22610·07
4541·62 (− 2)	22012·44	4420·71 (− 3)	22614·47

D, T: previously measured by Deodhar and Tanaka respectively. *d* Diffuse.

Table 1.—*Contd.*

Wave-length	Wave-number *in vacuo*	Wave-length	Wave-number *in vacuo*
4415·82 (− 2)	22639·51	4309·25 (0)	23199·39
4413·75 (− 3)	22650·13	4301·98 (− 1)	23238·60
4413·30 (− 3)	22652·44	4301·35 (− 3)	23242·00
4412·07 (− 2)	22658·75	4299·34 (− 3)	23252·86
4411·29 (− 3)	22662·76	4298·41 (− 3)	23257·90
4409·31 (− 3)	22672·95	4296·26 (− 3)	23269·54
4405·85 (− 3)	22690·71	4296·656 (1)	23272·81
4405·16 (− 3)	22694·30	4295·13 (− 3)	23275·66
4404·483 (0)	22697·15	4294·47 (− 3)	23279·23
4401·95 (0)	22710·84	4288·69 (− 1)	23310·61
4397·51 (− 3)	22733·77	4285·85 (− 1)	23326·06
4396·37 (− 3)	22739·67	4283·40 (− 3)	23339·40
4395·17 (− 2)	22745·88	4278·09 (− 1)	23368·35
4390·07 (− 3)	22772·30	4277·95 (− 3)	23369·31
4388·46 (− 3)	22780·65	4277·70 (− 2)	23370·49
4387·53 (− 3)	22785·49	4277·19 (− 2)	23373·28
4384·07 (− 2)	22803·47	4276·79 (− 3)	23375·47
4383·72 (− 3)	22805·29	4274·61 (− 2)	23387·39
4377·77 (− 3)	22836·29	4274·08 (− 2)	23390·28
4377·68 (− 2)	22836·76	4272·08 (− 2) T	23401·24
4376·00 (− 3)	22845·52	4270·50 (− 3) T	23409·89
4374·81 (− 2)	22851·73	4269·57 (0) D	23415·00
4374·42 (− 1)	22853·77	4269·06 (− 3)	23417·79
4374·29 (− 2) T	22854·45	4268·76 (− 3)	23419·43
4372·46 (− 1)	22864·02	4262·18 (− 3)	23455·59
4369·54 (− 3)	22879·30	4259·65 (− 1)	23469·52
4369·31 (− 3)	22880·50	4259·22 (− 3)	23471·88
4369·03 (− 1) T	22881·97	4258·76 (− 1)	23474·42
4367·16 (− 2)	22891·76	4254·74 (− 3)	23496·60
4366·94 (0)	22892·92	4252·62 (0) *d*	23508·31
4366·58 (− 2)	22900·04	4252·13 (− 2)	23511·58
4364·05 (− 2) T	22908·08	4251·45 (− 3)	23514·79
4362·87 (0) T	22914·28	4250·48 (− 2)	23520·16
4361·65 (− 1)	22920·68	4250·28 (− 1)	23521·26
4360·89 (− 2)	22924·68	4244·11 (− 3)	23555·45
4360·47 (0)	22926·88	4243·66 (− 3)	23557·95
4356·05 (0)	22950·15	4239·58 (− 3)	23580·62
4352·19 (0) *d*	22970·50	4238·87 (− 3) T	23584·62
4352·07 (− 1) *d*	22971·14	4236·33 (− 2)	23598·71
4351·28 (− 2)	22975·30	4235·54 (− 3)	23603·16
4350·838 (1) T	22977·63	4234·01 (− 3)	23611·64
4347·85 (− 2)	22993·43	4225·99 (− 3)	23656·46
4347·52 (0)	22995·18	4221·14 (− 3)	23683·63
4346·36 (− 1)	23001·32	4215·32 (− 3)	23716·23
4345·73 (− 1) T	23004·65	4215·08 (− 3)	23717·68
4341·40 (− 2)	23027·60	4206·84 (− 3)	23764·14
4336·76 (− 2)	23052·23	4201·23 (− 3)	23795·87
4332·44 (− 2)	23075·21	4191·23 (− 2)	23852·65
4328·95 (− 2)	23093·81	4190·19 (− 3)	23858·57
4327·08 (− 2)	23103·79	4185·65 (− 3)	23884·45
4325·77 (− 3)	23110·80	4185·04 (− 2)	23887·92
4325·43 (− 3)	23112·62	4183·55 (− 2)	23896·44
4322·86 (− 3)	23126·35	4183·20 (− 3)	23898·43
4321·00 (− 1)	23136·31	4181·88 (− 3)	23905·98
4319·89 (− 1)	23142·25	4178·38 (− 3)	23926·00
4319·63 (− 2)	23143·63	4176·82 (0)	23934·94*
4311·47 (− 2)	23187·45	4175·93 (− 3)	23940·03

* Possibly a ghost.

Table 1.—*Contd.*

Wave-length	Wave-number *in vacuo*	Wave-length	Wave-number *in vacuo*
4172·34 (− 3)	23960·63	4117·15 (− 1)	24281·81
4171·02 (0)	23968·22†	4113·96 (− 2)	24300·65
4167·98 (− 2)	23985·69	4113·71 (− 1)	24302·13
4166·14 (− 3) D	23996·29	4113·23 (− 2)	24304·96
4164·79 (− 3)	24004·07	4110·58 (− 1)	24320·63
4164·00 (− 3)	24008·62	4105·45 (− 2) *d*	24351·02
4160·18 (− 3)	24030·67	4104·27 (− 2)	24358·02
4159·58 (− 3)	24034·14	4102·40 (0)	24369·11
4157·55 (− 3)	24045·87	4100·87 (− 2)	24378·21
4153·16 (− 2)	24071·28	4097·98 (0)	24395·41
4148·91 (− 3)	24095·94	4087·46 (− 2)	24458·19
4147·11 (− 2)	24106·40	4086·73 (− 1)	24462·56
4144·71 (− 3)	24120·36	4086·51 (− 1)	24463·88
4140·74 (− 3)	24143·49	4082·91 (− 2)	24485·45
4140·18 (− 3)	24146·76	4077·72 (− 1)	24516·61
4139·46 (− 1)	24150·90	4055·68 (− 3)	24649·84
4136·20 (− 2)	24169·98	4051·61 (− 3)	24674·60
4134·65 (− 3)	24179·05	4047·19 (− 1)	24701·55
4128·70 (− 3)	24213·89	4046·18 (− 3)	24707·72
4125·66 (− 3)	24231·73	4045·99 (− 3)	24708·87
4124·40 (− 3)	24239·14	4044·114 (1)	24720·34
4123·48 (− 3)	24244·55	4042·67 (− 3)	24729·17
4120·46 (− 2)	24262·31	4042·45 (− 3)	24730·51
4119·72 (− 3)	24266·67	4041·97 (− 3)	24733·45
4119·49 (− 3)	24268·02		

† Possibly a ghost.

Table 2. Lines given by Gale, Monk and Lee but not found by the author.

Wave-length	Wave-number *in vacuo*	Wave-length	Wave-number *in vacuo*
4712·805 (0)	21212·87	4287·613 (0)	23316·46
4665·770 (00)	21426·71	4224·105 (0)	23667·01
4644·905 (0)	21522·96	4216·176 (0)	23711·52
4640·192 (0)	21544·82	4211·133 (00)	23739·91
4638·012 (0)	21554·94	4210·680 (00)	23742·47
4625·457 (0)	21613·45	4209·690 (0)	23748·06
4624·731 (0)	21616·85	4205·279 (1)	23772·96
4621·806 (0)	21630·52	4200·893 (0)	23798·35
4604·928 (0)	21709·81	4198·455 (00)	23811·59
4591·925 (0)	21771·28	4189·091 (00)	23864·83
4588·833 (0)	21785·95	4182·674 (0)	23901·43
4586·120 (0)	21798·83	4179·879 (0)	23917·32
4579·579 (1)	21829·97	4152·164 (00)	24077·06
4575·981 (0)	21847·14	4101·768 —	24372·88
4553·954 (1)	21953·14	4101·690 —	24373·34
4526·996 (0)	22083·53	4091·068 (00)	24436·62
4518·128 (0)	22126·88	4086·284 (0)	24465·23
4517·722 (1)	22128·86	4079·432 (0)	24506·33
4508·793 (00)	22172·68	4076·806 (0)	24522·08
4503·542 (00)	22198·54	4076·110 (00)	24526·29
4466·928 (0)	22380·49	4075·010 (00)	24532·91
4464·352 (1)	22393·40	4072·341 (0)	24549·00
4461·704 (0)	22406·69	4065·182 (00)	24592·24
4455·023 (0)	22440·29	4064·173 (0)	24598·35
4427·472 (00)	22579·93	4060·903 (0)	24618·15
4385·704 (00)	22794·99	4058·993 (0)	24629·74
4354·181 (0)	22960·00	4040·247 (00)	24743·97
4316·956 (00)	23157·98		

REVIEWS OF BOOKS

Handbuch der Experimentalphysik, Herausgegeben von W. WIEN und F. HARMS. Unter Mitarbeit von H. Lenz. Band XXII: "Zeemaneffekt," von E. Back; "Ergebnisse und Anwendungen der Spektroskopie, Ramaneffekt," von G. Joos. Pp. x + 436. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1929.) Unbound 41 gold marks, bound 42·80 gold marks.

This valuable volume on modern optical spectroscopy, while ostensibly concerned with results and applications, has the very desirable feature that enough of the theoretical groundwork is presented and kept constantly in view to enable the physicist to understand the significance of observational data, even if he be not actively engaged in spectroscopic research. It comprises two separate works, each by an author of high repute and unimpeachable authority.

The part devoted to the Zeeman effect occupies the first 189 pages, and contains six chapters dealing with (1) groundwork and empirical rules, (2) experimental methods and apparatus (electromagnets, spectrographs and light-sources), (3) theory of the Zeeman splitting in relation to the term analysis of atomic spectra, (4) the Paschen-Bac effect, which Prof. Back modestly calls the magneto-optic transformation effect, (5) by Prof. Joos, the Zeeman effect in band-spectra of diatomic molecules, and (6) the influence of the magnetic field on the absorption spectra of crystals.

The second part of the book, by Prof. Joos, occupies 232 pages, and consists of eight chapters on atomic and molecular spectra, with an appended chapter on the Raman effect. Chapter 1 deals with the analysis of atomic (line) spectra into multiplets and series, and, briefly, with that of band-spectra of diatomic molecules. More detailed accounts of the structure of line spectra appear in Chapters 2 and 3, and of band spectra in Chapter 4. Intensity relations in both line and band spectra are considered in Chapter 5. Chapter 6 deals with spectroscopic determinations of fundamental quantities such as e/m and h, fine structure of spectra lines, and relativity and gravitation effects. Chapter 7, under the title of Applications of Spectroscopy in Chemistry, deals with several unrelated parts of the subject such as quantitative analysis, the isotrope effect, absorption spectra of liquids and solids, and the determination of heats of dissociation of non-polar molecules from their band spectra. Modern astrophysical applications of spectroscopy form the subject of Chapter 8.

With so much that is excellent, it seems ungracious to criticise anything, but the order of treatment outlined above perhaps calls for some comment. The discussion of band spectra of diatomic gaseous molecules, for example, is not connected and self-contained, but is scattered amongst several chapters in the second part, namely Chapter 1 § 4, Chapter 4 §§ 1–4, Chapter 5 § 5, Chapter 7 §§ 2 and 4 (*b*), and also Chapter 5 of the first part. In the reviewer's opinion a distinct advantage to the reader would have accrued from a blending of these sections into, say, two chapters following those dealing solely with atomic line spectra and preceding those on the band spectra of polyatomic molecules (including liquids and solids) and the Raman effect.

In some instances the notation employed is inferior to that now used by most (including many German) investigators: e.g. the designation of the upper and lower electronic states of an atom or molecule by the subscripts a and e instead of the usual $'$ and $''$ has the disadvantage that logically the a should refer to the upper ($'$) state in emission spectra and to the lower ($''$) state in absorption; and such descriptions as *Anfangs-* and *Endzustand*

and their English renderings, initial and final states, therefore often leads to confusion. Nor is the notation consistent throughout one part of the book; for while on pp. 208 and 304 ν_0 and x appear for the nuclear vibration frequency (for $n = 0$ and for infinitesimal amplitude) and anharmonicity factor respectively, yet in the table on pp. 297–303 these quantities are denoted by ν_a and β_a for the upper state and ν_e and β_e for the lower state, in both emission and absorption systems; the more usual notation for these is ω_0' and x', and ω_0'' and x'' respectively. On the other hand, the appearance of the symbols Li, Be^+, B^{++}, etc., instead of Li I, Be II, B III, etc., is welcome.

In the table of molecular data on pp. 297–303 the information given for some molecules is very recent (1929), while for others, e.g., CO and C_2, it does not represent the state of knowledge early in 1928; and in Fig. 17, p. 294, the omission of a ^1D level of Mg makes the general resemblance between CO and Mg electronic levels appear slightly more complete than it really was, even without the inclusion of the CO systems discovered in and since 1927.

To the list of corrections of errors, given on p. 424, the reviewer is able to add the following: the term $n(\nu_0 - nx + \ldots)$ in equations (15) and (16) on p. 208 should be $n\nu_0(1 - nx + \ldots)$ and the corresponding term in equation (17) should be $(n + \frac{1}{2})\nu_0 - (n + \frac{1}{2})^2 \nu_0 x$; and the names of Selwyn and Ruedy are misspelt (pp. 231, 233, 234, 236). All these are, however, at most quite minor blemishes in a very admirable addition to the volumes of this *Handbuch* which have already appeared.

W. J.

Wave-Lengths and Atomic Levels in the Spectrum of the Vacuum Iron Arc, by Keivin Burns and Francis M. Walters Jr. Pp. 159–211. (Publications of the Allegheny Observatory of the University of Pittsburg, **6**, no. 11, 1929.)

Whether or not vacuum sources will ever replace the air-iron arc for general use as standards of wave-length in the ordinary photographic region, they are necessarily so used in the further ultra-violet; and for the latter region new standards may be computed with great accuracy by applying the combination principle to the atomic levels obtained from the precise measurement and classification of lines in the former region. In the first part of this publication quartz interferometer measurements are given for over 700 lines in the vacuum-iron arc from λ 8825–2760 Å., the observed wave-lengths (reduced to 15° C. and 760 mm.) and vacuum wave-numbers being recorded in a table which also includes intensity estimates, classifications (temperature, pressure, and energy level), solar data and the best available wave-lengths for the air-iron arc. The second part deals with the construction of the system of energy levels and the computation of data for further lines; one table shows the values and designations of nearly 300 levels, of which 180 are classified, and another contains the computed data for nearly 1500 lines between λ 9653 and 2015 Å. together with their level notations and comparisons with observed wave-lengths both in the vacuum arc and in the solar spectrum. Using the shorter computed wave-lengths as standards, it will be possible to make exact measurements of the Fe^+ spectrum, and a similarly computed extension of that spectrum would, it is noted, furnish many good standards in the Schumann region. The authors suggest that the record of any general analysis of an atomic spectrum should be accompanied by an intensity diagram, which may be useful in the understanding not only of the spectrum in question but also of the spectra of allied elements; they give a series of intensity diagrams for neutral Fe and neutral Ti, since those for Ti may be helpful in the classification of the unassigned levels of Fe. The reviewer, in common with many spectroscopists, would like to see the designations M I, M II, M III,... abandoned in favour of M, M^+, M^{++}... for the neutral, singly ionised, double ionised,... atom. Even in this beautifully printed publication Fe I and Ti I exactly resemble chemical formulae for iodides. Single ionisation is conveyed to mind

less easily by Fe II than by Fe^{+}; and, to take an extreme case, sextuple ionisation can be denoted much better by a contraction such as Cl^{6+} than by the notation Cl VII now commonly used.

W. J.

Infra-red Analysis of Molecular Structure, by F. I. G. RAWLINS and A. M. TAYLOR. Pp. xv + 176. (Cambridge Series of Physical Chemistry: Cambridge University Press, 1929.) Price 10*s*. 6*d*. net.

The authors remark that no book in English exists which deals with the application of infra-red spectroscopy to the analysis of molecular structure and they offer their work to those undertaking investigations in molecular physics who approach the subject from the point of view of the mathematician or of the physical chemist. They discuss, naturally rather incompletely, both the theory and the experimental side of the subject, in both cases with a great measure of success. The book deals with gases, liquids (including solutions), solids, experimental apparatus, and mathematical theory. In the latter the newer investigations of wave-mechanics find a place. In a few cases some irregularities which might well have been removed by the editor occur. The curves of Fig. 8 do not appear to be described in the text in sufficient detail; on p. 57 "W. Mc. C. Lewis" appears instead of W. C. McC. Lewis," and the style occasionally leaves something to be desired, as for example on p. 71, where we are told that "a weapon will be forged which will not only be powerful in revealing the secrets of atomic and molecular linkages, but which may enable a check to be imposed upon the accuracy of deductions made from X-ray analysis." In several cases, in fact, the text shows some signs of haste and lack of adequate revision. The symbol *R* in the equation on p. 97 is not defined. The use of the German words *Eigenwerte* and *Eigenfunktionen* on p. 152 is not very satisfactory since it is hardly likely that, even at Cambridge, the mathematical conceptions they convey are unknown. The subject has moved so rapidly that some parts of the work need to be supplemented by newer accounts, such as that of Mecke and the Faraday Society's Discussion at Bristol, to which both the present authors contributed interesting papers. The book is a very useful introduction to the subject and above all is intelligible to the average physical chemist.

J. R. P.

Speech and Hearing, by HARVEY FLETCHER, Ph.D. Pp. xv + 331, with 151 figures, and an introduction by H. D. ARNOLD, Ph.D. (London: Macmillan and Co., Ltd.) 21*s*. net.

During the last decade the fundamental research carried out at the Bell Telephone Laboratories into all phases of the hearing of speech and music has led to the publication of a number of valuable papers. The information obtained is of interest in itself, but it is specially important in connexion with the requirements and limitations of various types of equipment for transmitting, recording and reproducing speech and music. The research has also produced indirect results in that the apparatus designed for the research has had far-reaching applications to distortionless gramophone, talking film and radio reproduction. The compilation by Dr Fletcher, the Acoustical Research Director of the Laboratories, of a book crystallising the experiments performed on speech and hearing and the conclusions reached during the investigations will therefore be widely welcomed. Although naturally, in many places, it is closely related to the original papers referred to, it co-ordinates and interprets them with insight and lucidity. It is especially valuable as an authoritative survey from one of the most successful laboratories dealing with this phase of research. Dr Fletcher's high authority for the opinions he expresses mitigates to some extent the absence at times of references to the full literature of the subject, and Dr Arnold's intro-

duction usefully informs us that in the work discussed by Dr Fletcher every effort has been made to use information found by other experimenters.

The book is divided into four distinct parts, each separately subdivided into chapters 1, 2, etc. This subdivision may arise from the fact that the book takes the general form in which it was presented in the Bell Laboratories' programme of "out-of-hours" courses, but it would be a convenience in future editions if cross references were given definitely to pages, rather than to "Part III Chapter II," etc., for there are no part or chapter numbers at the heads of the pages.

A valuable incidental feature is the adoption of the "bel," a power step of 10^1, and its subdivision the "decibel" ($10^{0\cdot1}$) as units of intensity ratio. Both names have been adopted by telephone engineers. The "decibel" is equal to the well-known "transmission unit" of telephony, and is also approximately the acoustical intensity ratio ordinarily just detectable by the ear. In consequence of this latter characteristic the ratio has been called a "sensation unit," but since it is purely a numerical ratio, and since our knowledge of sensations is not very extensive, it is better to call it a "decibel," and avoid unnecessary entanglements. With two exceptions (Fig. 71 and p. 199, line 10) Dr Fletcher has consistently adhered to this nomenclature throughout the volume.

The book is well printed and well illustrated. It opens with the mechanism of speaking, with references to Sir Richard Paget's work, and an exhaustive study of the wave form and power of speech sounds and of their frequency of occurrence in the English language. It is presumably due to an American influence in pronunciation that the sounds in the words "part, not, and father" are taken to be similar enough to be designated phonetically by a single sign. Acoustic analyses of typical singing voices and musical instruments are given also, together with estimates of the power output—factors which are of importance in the design of faithful reproducing systems.

In a chapter on the increasingly important subject of noise, spectral analyses of certain noises are supplemented by physiological estimates of their loudness. In these estimates various notes of standard pitch were separately adjusted in volume until they were either just drowned by the noise or judged to be equally loud. The recent work of Barkhausen and his collaborators on similar lines is not referred to. It appears from page 244 that attempts to develop universal methods for calculating loudness from a knowledge of the acoustic spectrum of complex tones have so far been unsuccessful, and it follows that a formula put forward in 1925 by Steinberg has not proved to be applicable outside the range of the type of experiments upon which it was based.

A very interesting account is given of the ear and its behaviour, and of the theory of the action of the ear which Dr Fletcher considers to be most in accord with the varied data now available. Key references are given to other theories which have been the subject of well-known controversy for some time but are not described. Dr Fletcher does not discuss the history of the theory set out, but it is largely his own extension, on the lines of Gray and of Roaf, of the Helmholtz theory.

A chapter on various simple methods of testing acuity of hearing, and their interpretation in numerics, affords highly interesting reading.

The section of the book on the perception of speech and music deals with questions of loudness, pitch and intelligibility, and then discusses the adverse effect of distortion by resonances, by reverberation, or by, say, alteration of the speed of gramophone discs, and of the adverse influences of noise and of deafness.

A. H. D.

The Acoustics of Orchestral Instruments and of the Organ, by E. G. RICHARDSON. Pp. 158. (London: Edward Arnold and Co.) 10*s*. 6*d*. net.

This book is based on a series of lectures by the author on the organ and other wind instruments, additional chapters having been inserted to cover strings and the orchestra in

general. The types of instruments described are the flute, reed instruments, brass, percussion and strings, while a concluding chapter deals with the *ensemble*. The subject is treated in a popular style which will appeal to musicians having only an elementary knowledge of physics. Nevertheless the book will be appreciated also by physicists interested in musical instruments, for the principles involved in their operation are clearly described and a considerable amount of new information, derived from recent research, has been included.

The author states that "the organ being in itself an orchestra does not receive a special chapter." The omission of such a chapter is in some respects unfortunate notwithstanding Dr Richardson's contention. Perhaps a little more might have been said about the organ as a musical instrument and a little less about the church bells which, despite their interesting characteristics, are seldom regarded as orchestral instruments.

Reference is made on page 25 to the end correction at an open end of a pipe, amounting to 0·58 of the radius, but the author has omitted to point out that a much larger end correction, at the blown end, must be applied in determining the pitch of an actual organ pipe.

The final chapter dealing with the orchestra as a whole includes a general survey of the important subject of architectural acoustics, and interesting references are made to certain buildings, e.g. the Pleyel Hall in Paris, having noteworthy acoustic properties. The book is written in an interesting style and is well produced. The illustrations are particularly good and well chosen.

Molecular Magnetism, by Prof. W. PEDDIE, D.Sc., F.R.S.E. Pp. viii + 140, with 38 diagrams. (London: Edward Arnold and Co., 1929.) 10*s*. 6*d*.

At a time when the attention of physicists is focussed so much on recent advances, and even text-books may have hectic references to the latest papers added in proof, it is a pleasant change to read a book in which due consideration is given to those parts of a subject in which a classical outlook can still be of service. The hero of Prof. Peddie's book is Weber, whose pioneer work on the theory of dia- and paramagnetism well deserves the prominence given to it. Weber's conception of molecular magnets arising from steady or induced molecular currents forms the basis of the whole treatment. With only slight modifications (such as the substitution of electronic currents for Weber's continuous currents) the consequences of Weber's views are developed as far as possible without the introduction of any of those "unexplained arbitrary hypotheses" which, it is suggested, are characteristic of the quantum theory. From a strictly classical standpoint, the trouble is that the physical world itself is very arbitrary. The value of the quantum theory lies in the fact that it is making that world seem less arbitrary. Even without the aid of the quantum theory, however, Prof. Peddie shows that a great deal can be done.

An account is given of paramagnetism, in which the treatment of Langevin is followed, and the effect of the Weiss molecular field is considered, but the main subject-matter of the book is molecular aggregates, crystals, particularly those of ferromagnetics. A straightforward and admirable treatment is given of the complicated problem of calculating the mutual magnetic effects of a number of molecular magnets arranged on a crystal lattice. It is shown that a cubic crystal will not be magnetically isotropic, and the values of the internal fields arising from the magnetic interaction are worked out. The results are in remarkable agreement with those found experimentally for crystals of iron, nickel and other ferromagnetics. Certain conclusions as to the "magnetic lattice," however, may require modification, as the work of Mahajani shows that changes of sign are introduced in some of the expressions if the magnetic elements are regarded as electron orbits rather than as small but finite magnets.

Stress is laid on the small values of the internal fields (of the order of a hundred gauss) which are found theoretically and apparently confirmed by a number of the experimental facts considered. These are contrasted with the enormous values required by the Weiss molecular field hypothesis. The difficulties seem to arise from the fact that there are two distinct types of internal fields to be considered. One of them—that arising from the mutual purely magnetic energy—is comparatively small, but is adequate to account for such effects as the differences in the properties of an iron or nickel crystal in different directions. To account for the distinctive characteristics of ferromagnetics, however, it seems necessary to postulate some interaction, of a non-magnetic origin, which may be very large. As an alternative, Prof. Peddie makes the unorthodox suggestion that the thermal energy associated with the magnetic elements may be very small—that kT may be small compared with the kinetic theory value—but this is equally arbitrary and leads to difficulties in other directions. There is now, moreover, some indication as to the origin of the mysterious Weiss molecular field in Heisenberg's theory of ferromagnetism. Where Prof. Peddie's conclusions are unorthodox, however, an adequate proof or disproof of them would be highly desirable.

The atomic side of magnetism is now fairly well developed, although many details are still obscure. The next step is to attack the problems of atomic and molecular aggregates in crystals. To those who wish to investigate these problems, Prof. Peddie's book will be of great value, for it is on what may be called the complex side of magnetism, the crystal side, that he has concentrated. Students, to whom the book is mainly addressed, will, no doubt, like others, find some parts difficult. It is by no means a purely introductory book, for although some aspects of the subject are touched on but lightly, others are dealt with very thoroughly. These are just those whose treatment in some other books on magnetism has been least adequate. For the special account of crystalline media, and for the systematic treatment of the subject-matter as a whole, with as few assumptions as possible, the book can be strongly recommended to all who are interested in magnetism.

E. C. S.

Geophysics Memoir 47 of the Meteorological Office. Report on Thames Floods and on the Meteorological Conditions associated with them, by A. T. Doodson and J. S. Dines. Pp. 39. (London: H.M. Stationery Office.) 2*s*. 6*d*.

These floods are found to be due to the simultaneous occurrence of tidal high water and a storm in the North Sea which produces a "storm surge" of the water to the west. The amount of water flowing down the Thames is not of primary importance. The probable occurrence of floods such as that of January 6–7, 1928, is about 1 in 60 years and would be reduced to about 1 in 150 years if the training walls were raised 1 ft. It does not appear possible to issue useful warnings of such floods with our present limited knowledge of their causes.

C. H. L.

Handbuch der Experimentalphysik, Wien and Harms, Band 8, Theil 1: *Energie und Wärmeinhalt*, by Prof. A. Eucken (Breslau). Pp. xvi + 736. Royal 8vo. (Leipzig: Akademische Verlagsgesellschaft m.b.H.) 65 marks.

This volume covers the calorimetry of solids, liquids and gases, latent heats, heats of solution, dilution and chemical action. It is certainly the most complete account of the experimental methods in use which is available in book form. Sufficient theory is given to make the object of the experimental work apparent and it is set down in a clear

and concise way. Good subject and name indexes and a list of the symbols used, with references to the pages on which they are defined, add materially to the ease with which the volume may be consulted. The print, illustrations and general appearance of the volume are excellent, but the high price must have a serious effect on its circulation.

C. H. L.

Materiewellen und Quantenmechanik, by Prof. A. Haas (Vienna). Pp. 189. 8vo. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 2nd edition, 1929.) 8 marks.

Although it is little more than a year since the first edition of this introduction to wave-mechanics appeared, the rapid advance of the subject has rendered the addition of four new chapters necessary. The treatment is such as to give a general idea of the lines on which the subject has developed and the results which have been attained rather than a summary of the memoirs of de Broglie, Schrödinger, Heisenberg and Dirac. Non-mathematical readers, for whom the preface states that the book is intended, may find five of the chapters heavy and are advised to omit them.

C. H. L.

Tables Annuelles de Constantes et Données Numériques. (Paris: Gauthier-Villars et Cie.)

(1) Données Numériques d'Électricité, Magnétisme et Électrochimie, pp. xii + 488; (2) Données Numériques de Spectroscopie, pp. viii + 363; (3) Données Numériques de Photographie, pp. x + 36; (4) Données Numériques de Radioactivité, pp. vii + 29; (5) Données Numériques de Radioélectricité, pp. vii + 26; (6) Données Numériques sur les Colloïdes, pp. viii + 58; (7) Données Numériques de Cristallographie, pp. viii + 52; Données Numériques de Physique du Globe, pp. vii + 59; (9) Art de l'Ingénieur et Métallurgie, pp. xiv + 169.

Dr Marie and his international Committee continue with praiseworthy regularity the good work of keeping active researchers in the physical and allied sciences in close touch with recently published numerical data in almost every branch of physics. These volumes form an almost indispensable supplement to the tables of Landolt and Bornstein and to the International Critical Tables, and they have the advantage that they may be obtained in handy sections of some fifty or a hundred pages each, so that workers in a particular subject need not be put to the expense of buying a volume containing data in which they take but little interest.

The volumes constitute parts of Volume 6, published in 1928 but containing data for the years 1923–1924.

The contents of the volumes are briefly as follows: (1) Electric conductivity, insulation, magnetism, atomic moments, electrolytes, etc. (2) This is a large volume which includes emission spectra, the Zeeman effect, X-ray spectra and band spectra. Much difficulty and confusion may arise from ever-changing notations and it is therefore interesting to note that in the record of Fowler's work on carbon, a Bohr notation is employed in the tables with the corresponding Rydberg numbers in parentheses. (3) Is a volume very useful to those physicists who rely on the photographic plate, and contains many data upon blackening of plates, grain distribution and the properties of photographic materials. (4) Includes, in addition to radioactivity, information concerning the ionisation of gases, thermionics, X-ray diffraction and isotopes. (5) Radioelectricity is the elegant name for "wireless," and the volume includes such topics as valves, attenuation coefficients, atmospherics, and finally a table for the calculation of inductances. (6) Includes the properties of gels and the adsorption of gases and dissolved substances. (7) Includes data upon X-ray determinations of crystal structure. (8) Includes radioactivity of the rocks, solar radiation

and meteorology. (9) Includes the properties of building materials, glass, enamels, rubber, lubricants and fuels. Under metallurgy are given the conductivities and mechanical properties of metals and their alloys.

There is no indication of the subscription price.

J. E. C.

Grundriss der Physiologie für Studierende und Ärzte, von CARL OPPENHEIMER und OTTO WEISS. Erster Teil: *Biochemie*, by Prof. CARL OPPENHEIMER. Pp. 420 + viii, with 8 figures in text, name and subject indexes, but no bibliography. (Leipzig: Georg Thieme, 6th edition, 1929.) 17 marks, unbound.

Prof. Oppenheimer has divided this edition of his introduction to the study of biochemistry into two parts. He first deals systematically with the chemical composition of the animal body, and the chemical and physical properties of the substances found therein. In the same part he then considers enzymes, antigens and antibodies—biological units whose chemical structures have not yet been elucidated—in terms of their activities. Of interest to the physicist will be the sections on the colloidal state of proteins, and on enzymes as colloids. In the second part, the chemical functions of tissues and organs are discussed. Accounts are given of changes in the chemical composition and physical states of matter and of the energy transformations associated with physiological activities such as muscular work, secretion, excretion, absorption and the transport of materials.

For those who wish to obtain a brief account of the biochemical and biophysical approach to physiology in the German language, this book can be recommended. It is as up-to-date as can be expected of any general text-book on a subject which grows as rapidly as biochemistry.

M. T.

Lehrbuch der Enzyme: Chemie, Physikalische Chemie, und Biologie, by Prof. CARL OPPENHEIMER. Pp. ix + 660 with 18 figures, subject index, and a few scattered references. (Leipzig: Georg Thieme, 1927.) 33 marks, unbound.

One of the objectives of the biochemist, in his analysis of the chemical processes occurring in living organisms, is the isolation from living tissues of non-living catalytic systems which will show the same activity outside the organism as they did when forming part of the living machinery. These biochemical catalysts are called enzymes. Most of the chemical changes occurring in organisms do so under the influence of specific enzymes. Every month sees an addition to the number of enzymes which have been separated in an active form from living tissues.

Experiments on the separated enzyme systems can take many forms, and these are indicated by the subject matter of the book under review. In Part I, Prof. Oppenheimer records what is known about the chemical nature of enzymes: but very little is known, for direct analysis of extracted systems has not, so far, been successful in isolating pure chemical compounds which show the same activity as the systems. But there is little doubt that most enzymes are colloidal catalysts, and as such, if their chemical nature be disregarded, they may be investigated by the physico-chemical methods used in experiments on colloidal dispersoids, and on catalyses in heterogeneous systems. A useful account of the physical chemistry and kinetics of enzymes has been written by Dr Richard Kuhn in a special chaper of about 90 pages: he first briefly considers electrical charges on, surface activity and adsorption relations and stability of, enzyme systems; then he treats reaction velocities under different conditions for e.g. varying temperatures—enzymes are thermolabile—and varying [H$^+$] to which most enzymes are sensitive; finally the kinetics of eleven

enzyme entities, when in activity, are shortly reviewed. In Part 2, Prof. Oppenheimer analyses systematically the chemical changes which occur under the influence of enzymes outside and inside living organisms. A very great deal of valuable biochemical information is concentrated in this part.

This work may be described as a good critical summary of our knowledge of enzymes up to 1927.

M. T.

Examples in Elementary Physics, by S. R. HUMBY and F. W. GODDARD. Pp. viii + 166. (London: Longmans, Green and Co., Ltd.) 2*s*. 6*d*.

This small volume is arranged to cover the syllabuses of the various school certificate and matriculation examinations, and the greater part of the First M.B. and Army Entrance examinations.

The examples are well chosen, and are representative of all the great branches of physical science. They are some hundreds in number, and for the most part possess the merit that the arithmetic involved may be simplified by cancelling. The book is well produced, is modest in price and can be thoroughly recommended.

A. F.

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THE DYNAMICAL THEORY OF RESONATORS

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ABSTRACT. It is well known that the theory of the small resonator provided with a neck communicating with the open air depends upon the assumption that the air within the neck may be treated as an incompressible fluid. In § 2 of the present paper the theory is extended to include generally necks of variable cross-section. Again, Rayleigh develops three theories depending upon the assumptions made as to the velocity-distribution of the air at the open end of a resonator. It is shown in § 3 that his theory, which assumes a variable velocity-distribution over the open end, is erroneous, and a theory which appears to be correct is put forward. The theory of the neck of variable cross-section is applied to the Helmholtz resonator in § 4. In § 5 the theory of an open end of rectangular cross-section is considered. In § 6 the general theory of resonators is dealt with. It is shown that impedance principles require modification for certain types of resonator, although they hold for most of the common types.

§ 1. INTRODUCTION

My attention was attracted to the theory of resonators in connexion with another subject, and from the theoretical point of view it seemed to be of great interest. It soon appeared to me that the theory was imperfect and defective in several respects, and in the present paper I have endeavoured to the best of my knowledge to revise it briefly. I have not touched upon the experimental side, but I think it will be found that some of the results lend themselves readily to experimental verification. Briefly stated, the problems considered in this paper are concerned with the dependence of the characteristics of a resonator

(1) upon the distribution of normal velocity over the end of the channel which communicates with the outside air,

(2) upon the form of the channel which, in an important class of resonators, is a neck of variable cross-section, and

(3) upon the shape of the orifice, or open end, for which the impedances can be calculated in certain cases.

Rayleigh's work, fundamental though it is, requires some reconsideration. Reference may be made to the theory of the pipe of uniform circular section with

an open end, which is discussed in Appendix A at the end of his *Theory of Sound*, Vol. 2. An arbitrary approximate expression for the distribution of normal velocity over the open end is there assumed; but in the present paper it is shown that that portion of the fluid, throughout which Rayleigh calculates the kinetic energy, contains a plane, coincident with the open end, over which a discontinuity of pressure must exist, which Rayleigh has not considered. A full discussion of this question will be found in § 3, where reasons are given for doubting the validity of Rayleigh's conclusions as to the normal velocity distribution over an open end. For some experimental investigations of the velocity distribution over a circular aperture in a plane wall reference may be made to a paper by E. G. Richardson*.

The nature of the velocity near the edge is remarkable, but it is important to note that the experiments determine the resultant velocity at any point and not the normal velocity, which is required by the theory. It must also be noted that the values of impedance and end correction vary little for almost any assumption which may be made respecting velocity distribution.

Accordingly, in the absence of more precise information, the discussion in § 4 of the Helmholtz resonator with respect to the effect of the form of the channel is undertaken along with Rayleigh's classical assumptions†. These are (1) that the velocity at the open end is uniform, and (2) that the pressure at the open end is uniform. They are clearly the best postulates for the theory, for they provide reasonably close limits between which the true theory lies. An extended theory of resonators is considered in § 6, and leads to a discussion of the difficulties which arise in connexion with the nature of the transition from the theory of the pipe to the theory of the Helmholtz resonator, and with the theory of impedance. For the approximate theory of sound waves in horns of variable section, which is not considered in the present paper, reference may be made to Rayleigh‡ and to Webster§.

§ 2. GENERAL THEORY

Consider a pipe (Fig. 1), which may constitute the neck of a resonator, whose longitudinal section lies in the plane of the paper, and take as axis of x a line X_1X_0 which lies in this section, X_1 being the origin of x. Consider also two planes at X_1 and X_0 respectively which are both perpendicular to the axis of x. These planes intersect the longitudinal section in the points $A_1B_1A_0B_0$; they are distant L apart; and the cross-section between them is variable.

The assumptions made are (1) that the pipe is provided with an infinite flange aA_0B_0b, the space to the right of this flange and the section A_0B_0 being occupied by the open air, and (2) that the fluid contained within the pipe between the cross-sections A_1B_1 and A_0B_0 may be treated as incompressible. The latter assumption, as Rayleigh points out, is valid if the dimensions of the neck are small compared

* "The Amplitude of Sound Waves in Resonators," *Proc. Phys. Soc.* **40**, p. 206, (1928).
† *Theory of Sound*, **2**, §§ 306, 307.
‡ *Ibid.* § 265.
§ *Proc. Nat. Acad. Sc.* **5**, 275 (1919).

with the wave-length of the oscillations. A velocity potential ϕ is postulated for the flow within the pipe, ϕ_1 and ϕ_0 denoting respectively the values which ϕ assumes on the planes A_1B_1 and A_0B_0.

Let AB be any plane cross-section parallel to the end sections, and let C be a point upon AB. Consider an elementary tube of flow C_1C_0 passing through C and let the sectional area of this tube by the plane AB be dS. If dS_n denote the normal cross-section of the tube at C, and n the normal to this section, then it follows from the requirement of continuity that

$$\frac{d\phi}{dx}.dS = \frac{d\phi}{dn}.dS_n.$$

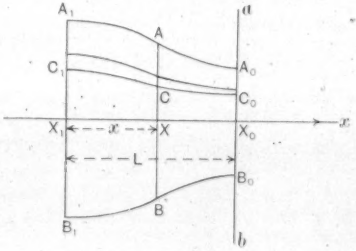


Fig. 1. Longitudinal section of neck of resonator.

Hence $d\phi/dx.dS$ is a constant for the elementary tube C_1C_0, and therefore, if ϕ_m be the mean value of ϕ over the section AB,

$$\frac{d}{dx}\left(S\frac{d\phi_m}{dx}\right) = 0 \qquad \text{.....(1),}$$

where S is the area of the cross-section of the pipe by the plane AB.

From (1) it follows that

$$\phi_m = C\int_0^x \frac{1}{S}dx + D,$$

where C and D are constants.

Denoting by ϕ_{m1} and ϕ_{m0} the mean values of ϕ over the sections A_1B_1 and A_0B_0 respectively, we must have $D = \phi_{m1}$. Denoting also by N the integral

$$\int_0^L \frac{1}{S}dx,$$

we have

$$\frac{1}{N}(\phi_{m0} - \phi_{m1}) = S\frac{d\phi_m}{dx} \qquad \text{.....(2).}$$

From continuity it follows that

$$S\,(d\phi_m/dx) = S_1\,(d\phi_{m1}/dx) = S_0\,(d\phi_{m0}/dx).$$

The "neck integral," expressed by N, is the resistance of the channel.

For the kinetic energy of the fluid, of density ρ, contained between the planes A_1B_1 and A_0B_0, we have, denoting it by T,

$$\frac{T}{\rho} = \tfrac{1}{2}\int \frac{d\phi_0}{dx}.\phi_0 dS_0 - \tfrac{1}{2}\int \frac{d\phi_1}{dx}.\phi_1 dS_1.$$

If it be assumed that ϕ_1 is uniform over A_1B_1, two cases of interest arise; (i) $d\phi_0/dx$ is assumed uniform over A_0B_0; (ii) ϕ_0 is assumed uniform over A_0B_0. In both cases

$$\frac{T}{\rho} = \tfrac{1}{2}(\phi_{m0} - \phi_{m1})\frac{d\phi_{m1}}{dx} S_1 = \tfrac{1}{2}N\left(S_1 \frac{d\phi_{m1}}{dx}\right)^2 \qquad \text{.....(3).}$$

Again, at the open end A_0B_0, owing to the presence of the infinite flange, ϕ_0 and $d\phi_0/dx$ are connected by the important relation

$$\phi_0 = -\frac{1}{2\pi}\int \frac{d\phi_0}{dx}.\frac{e^{-i\kappa r}}{r} dS_0.$$

Since κr is in the present paper assumed to be small, it is sufficient to write

$$\phi_0 = -\frac{1}{2\pi}\int \frac{d\phi_0}{dx}.\frac{1}{r} dS_0 + \frac{i\kappa}{2\pi}\int \frac{d\phi_0}{dx} dS_0,$$

and then

$$\phi_{m0} = -\frac{1}{2\pi S_0}\int dS_0 \int \frac{d\phi_0}{dx}.\frac{1}{r} dS_0 + \frac{i\kappa}{2\pi}\frac{d\phi_{m1}}{dx} S_1 \qquad \text{.....(4).}$$

The resistance N. Three examples of N, the reciprocal of the conductivity, associated with common forms of the neck, are of interest. In the first case the neck is uniform so that

$$N = L/S_1.$$

For a neck of uniform circular section of radius R_1, $N = L/\pi R_1^2$, an expression obtained by Rayleigh.

In the second case

$$S = a + b\cos(\pi x/L).$$

This makes dS/dx zero when $x = 0$ and $x = L$. When $a > b$ it is easy to show that

$$N = L/(S_1 S_0)^{\frac{1}{2}}.$$

If the neck be of circular section, of radii at the two ends respectively R_1 and R_0,

$$N = L/\pi R_1 R_0.$$

In the third case

$$S = a - bx,$$

and it follows that

$$N = L\left\{\frac{\log S_1 - \log S_0}{S_1 - S_0}\right\}.$$

§ 3. THE THEORY OF THE PIPE WITH AN OPEN END

In addition to the assumptions (1) and (2) of § 2 it is assumed (3) that the orifice is circular; (4) that from the section A_1B_1 (Fig. 1) to the left there is a uniform pipe, so that at A_1B_1, and to the left of A_1B_1, the waves in the pipe may be considered to be plane; and (5) that the velocity distribution over the circular end A_0B_0 is expressed by

$$d\phi_0/dx = \beta\{1 + (\sigma/R_0^2)\, s^2\} \qquad \text{.....(5),}$$

where β and σ are constants, R_0 is the radius of the end, and s is the distance of a representative point from the centre of the end.

It follows readily from (5), by integration over the open end, that

$$d\phi_{m0}/dx = \beta\,(1 + \sigma/2).$$

Hence
$$\beta = \frac{1}{1+\sigma/2}\cdot\frac{1}{S_0}.S_1\frac{d\phi_{m1}}{dx} \qquad \text{.....(6).}$$

The method of calculating ϕ_{m0}, which is the mean value of the velocity potential induced over the surface A_0B_0 of the open end by the reaction of the external air, follows that of Rayleigh and Maxwell*.

The general result for all wave-lengths is somewhat complicated, but in the present case the problem reduces to the consideration of the first part of the right-hand side of (4). Since the calculation is an obvious extension of that of Rayleigh, it is not given here in full. The open end A_0B_0 is a circle with centre at X_0, and of radius R_0. Consider a concentric circle of radius c and let ϕ_0' be the potential at a point situated on the circumference of this circle due to that portion of the open end bounded by it.

It can be shown from (4) that

$$\phi_{m0} = \frac{4\pi}{S_0}\int_0^{R_0}\phi_0'.cdc + \frac{i\kappa}{2\pi}\frac{d\phi_{m1}}{dx}.S_1.$$

It can, further, be shown that

$$-\pi\phi_0' = \beta\left(2c + \frac{10}{9}\cdot\frac{\sigma}{R_0^2}c^3\right).$$

Hence it follows, from (6), that

$$\phi_{m0} = \frac{d\phi_{m1}}{dx}\left\{-\frac{8R_0}{9\pi}\cdot\frac{3+\sigma}{1+\sigma/2}\cdot\frac{S_1}{S_0} + \frac{i\kappa}{2\pi}S_1\right\} \qquad \text{.....(7).}$$

But, from (2),

$$\phi_{m0} = \phi_{m1} + NS_1\,(d\phi_{m1}/dx).$$

Hence
$$\phi_{m1} = \frac{d\phi_{m1}}{dx}\left\{-\frac{8R_0}{9\pi}\cdot\frac{3+\sigma}{1+\sigma/2}\cdot\frac{S_1}{S_0} - NS_1 + \frac{i\kappa}{2\pi}S_1\right\}$$

$$= \frac{d\phi_{m1}}{dx}\{-A_0 + iB_0\}, \text{ for brevity.} \qquad \text{.....(8).}$$

It is assumed for the velocity potential ψ_m within the pipe to the left of A_1B_1, the origin being at X_1 (Fig. 1), that

$$\psi_m = (A\cos\kappa x + B\sin\kappa x)\,e^{i\kappa ct},$$

corresponding to $\phi_{m0}e^{i\kappa ct}$ at the open end. It follows that

$$A = \phi_{m1} \quad \text{and} \quad \kappa B = d\phi_{m1}/dx.$$

Hence
$$\psi_m = (d\phi_{m1}/dx)\,\{(-A_0 + iB_0)\cos\kappa x + \kappa^{-1}\sin kx\}\,e^{i\kappa ct},$$

the real part of which is

$$(d\phi_{m1}/dx)\,\{(-A_0\cos\kappa x + \kappa^{-1}\sin\kappa x)\cos\kappa ct - B_0\cos\kappa x\sin\kappa ct\}.$$

* See also *Phil. Mag.* **46**, 434 (1923).

The equation for ψ_m is most conveniently written in the form

$$\psi_m = (d\phi_{m1}/dx)\{\kappa^{-1}\sec\kappa a_1 \sin\kappa(x-a_1)\cos\kappa ct - (\kappa/2\pi)S_1\cos\kappa x.\sin\kappa ct\} \quad ...(9),$$

where $\tan\kappa a_1 = \kappa A_0$.

When κA_0 is small the end correction $(a_1 - L)$ is $(A_0 - L)$, or

$$\frac{8R_0}{9\pi}\cdot\frac{3+\sigma}{1+\sigma/2}\cdot\frac{S_1}{S_0} + NS_1 - L.$$

If the pipe be of uniform section up to the end A_0B_0, this reduces to

$$\frac{8R_0}{9\pi}\cdot\frac{3+\sigma}{1+\sigma/2} \quad .....(10).$$

The problem so far is quite definite, but it is not easy to proceed to the determination of the velocity distribution σ by theoretical considerations only. It is possible, however, to compare the result directly with that obtained by Rayleigh. Let us, therefore, consider Rayleigh's theory which is given in Appendix A at the end of his *Theory of Sound*, Vol. **2**. This theory was evolved in order to determine approximately the velocity distribution over an open end. Rayleigh finds two solutions, one for the fluid inside the pipe and another for the fluid outside the pipe, the latter being fitted with an infinite plane flange; both fluids are treated as incompressible. These solutions are determined so as to give the same velocity distribution over the open end, this distribution being expressed by a function with coefficients to be evaluated. Rayleigh then proceeds to calculate the kinetic energies of each of these motions, and argues that the coefficients of the function which expresses the velocity distribution can be obtained by making the sum of the kinetic energies a minimum.

This sum represents the total kinetic energy of the motion, and it is clear that, if it contains the coefficients of the postulated velocity distribution, the pressure must be discontinuous upon crossing the plane of the open end.

In this problem it is not permissible to have recourse to diaphragms or pistons, and therefore the introduction of a pressure discontinuity at points situated in the open end A_0B_0 does not seem to be tenable.

Refer now to equation (11) on p. 490 of the *Theory of Sound*, Vol. **2**. The fraction on the right of that equation becomes, on putting $\mu' = 0$,

$$\frac{0{\cdot}0666667\mu - 0{\cdot}0122728\mu^2}{(1+\mu/2)^2},$$

in which μ is the same as our σ. Rayleigh states that the value of μ required is that which makes this expression a maximum, for then the kinetic energy will be a minimum. This leads to the equation

$$\mu = 1{\cdot}152.$$

On p. 491 the corresponding value of the end correction is given as $0{\cdot}828146R$, where R is the same as our R_0. But, if this value for the end correction be equated to the expression (10) of the present paper, the corresponding value of σ turns out to be 0·159. This result appears to be more probable than that of Rayleigh, and it supports the criticism which was raised by Bosanquet against Rayleigh's theory.

§ 4. THE HELMHOLTZ RESONATOR

In addition to the two assumptions of § 2 it is further assumed (3) that the end A_1B_1 of the neck connects with a small sound box, throughout which the pressure rises and falls uniformly. This assumption is implied in the definition of a Helmholtz resonator. It is also assumed (4) that the whole kinetic energy of the interior fluid is confined to that contained in the neck.

Case A. This involves the further assumption (5) that the velocity distribution over the orifice is uniform. For the total pressure over the open end A_0B we have

$$S_0 p_{m0} = + S_0 \rho \, (d\phi_{m0}/dt),$$

if ρ be the density of the air. We shall write

$$\phi_{m0} = C_0 \, (d\phi_{m1}/dx) = - C_0 \, (dx_{m1}/dt),$$

in which x_{m1} is the mean fluid displacement at A_1B_1 at any instant.

For the kinetic energy we have, from (3),

$$T = \tfrac{1}{2}\rho N S_1^{\,2} \, (dx_{m1}/dt)^2,$$

and for the potential energy of the air in the sound box

$$V = \tfrac{1}{2}\rho c^2 \, (S_1^{\,2} x_{m1}^{\,2}/W),$$

where c is the velocity of sound, and W the volume of the small sound box. For the equation of motion it follows that

$$(d^2x_{m1}/dt^2) \, (NS_1^{\,2} - C_0 S_0) + x_{m1} \cdot c^2 S_1^{\,2}/W = 0 \qquad \ldots\ldots(11).$$

Since there is dissipation, $i\kappa$ in (4) must be replaced by γ, where γ is complex. Also $x_{m1} \propto e^{\gamma ct}$.

It follows readily that

$$\gamma^3 - \gamma^2 \, (A + C) - B = 0, \qquad \ldots\ldots(12),$$

where

$$A = 2\pi N S_1/S_0,$$

$$B = 2\pi S_1/W S_0,$$

and

$$C = \frac{1}{S_0^{\,2}} \int dS_0 \int \frac{1}{r} \, dS_0.$$

In all cases we may put

$$\gamma = i\kappa - \epsilon,$$

where ϵ is small, for then we obtain, approximately,

$$\kappa^2 = B/(A + C),$$

and

$$\epsilon = B/2 \, (A + C)^2.$$

If n be the resonant frequency, it follows that

$$\frac{1}{\epsilon c} = \frac{4\pi}{n^4} \cdot \frac{S_1}{S_0} \cdot \frac{c^3}{W};$$

this is the time in which the amplitude is diminished in the ratio $e/1$, and, when the neck is of uniform section, it reduces to the result given by Rayleigh on p. 194 of his *Theory of Sound*, Vol. **2**.

Case B. Instead of assumption (5) in case A, it is assumed (5) that the pressure over the orifice is uniform. In case A we found a cubic in γ where

$$\gamma = i\kappa - \epsilon,$$

and a solution was obtained upon the assumption that ϵ is small in comparison with κ. In the present case we commence by writing

$$\phi_{m0} = A' c e^{\gamma c t},$$

where A' is real.

It follows that

$$S_0 p_{m0} = S_0 \rho A' c^2 \gamma e^{\gamma c t}.$$

Hence, as in case A, for the equation of motion, we obtain

$$N S_1^2 (d^2 x_{m1}/dt^2) + (c^2 S_1^2/W)\, x_{m1} + S_0 A' c^2 \gamma e^{\gamma c t} = 0.$$

Let $x_{m1} = B' e^{\gamma c t}$;

then $$(B'/A')\,(N S_1^2 \gamma^2 + S_1^2/W) + S_0 \gamma = 0 \qquad \ldots\ldots(13).$$

If $e^{\gamma c t}(\alpha + i\beta) = d\phi_0/dx$,

$$e^{\gamma c t} \frac{1}{S_0} \int (\alpha + i\beta)\, dS_0 = \frac{d\phi_{m0}}{dx} = -\frac{dx_{m0}}{dt} = -\frac{S_1}{S_0}\frac{dx_{m1}}{dt}$$

$$= -\frac{S_1}{S_0} B' \gamma c e^{\gamma c t}.$$

Hence $$\int (\alpha + i\beta)\, dS_0 = -S_1 B' \gamma c \qquad \ldots\ldots(14).$$

As before let $\gamma = i\kappa - \epsilon$; then

$$\frac{1}{2\pi} \int (\alpha + i\beta) \left(\frac{1}{r} - i\kappa + \epsilon\right) dS_0 = -A' c.$$

This requires that

$$\frac{1}{2\pi} \int \left(\frac{\alpha}{r} + \alpha\epsilon + \kappa\beta\right) dS_0 = -A' c,$$

and $$\int \left(\frac{\beta}{r} + \beta\epsilon - \kappa\alpha\right) dS_0 = 0.$$

We assume provisionally that $\beta\epsilon$ is so small that it may be neglected.

Let $$\int \frac{\alpha}{r}\, dS_0 = -2\pi\mu A' c,$$

Then $$\int \alpha\, dS_0 = -2\pi\mu A' c E,$$

where E is the electrostatic capacity of a metal disc which has the shape of the orifice and is equal to c/π, where c is the conductivity of the orifice.

Hence $$\int \frac{\beta}{r}\, dS_0 = -2\pi\kappa\mu A' c E,$$

so that $$\int \beta\, dS_0 = -2\pi\kappa\mu A' c E^2.$$

The assumption respecting $\int \beta\epsilon\, dS_0$ is therefore justified. It follows at once, on neglecting $\kappa^2 E^2$, that

$$\mu = 1/(1 + \epsilon E).$$

From (14) we have

$$\frac{B'}{A'} = \frac{2\pi\mu E}{S_1 \gamma}(1 + i\kappa E).$$

Hence, from (13),

$$(-\kappa^2 - 2i\kappa\epsilon + \epsilon^2)(A + 1/\mu E + i\kappa EA) + (1 + i\kappa E)B = 0 \quad \ldots\ldots(15),$$

where A and B have the same meanings as in case A.

Equation (15) leads, approximately, to the equation

$$\kappa^2 - \epsilon^2 = B/(A + E^{-1})$$

and

$$2\epsilon(A + E^{-1}) = \kappa^2 - \epsilon^2.$$

It follows that, since κ is small in comparison with $(A + E^{-1})$, ϵ must be small in comparison with κ. Hence we obtain, just as in case A,

$$\kappa^2 = B/(A + E^{-1})$$

and

$$\epsilon = B/2(A + E^{-1})^2.$$

If S_0 becomes small in comparison with S_1

$$\kappa^2 \to 1/WN,$$

which is independent of the shape of the orifice, and of the velocity distribution over the orifice. This, of course, applies also to case A.

§ 5. THE ORIFICE

The constants C and E have been left undetermined. The constant E is known for an elliptic aperture. The constant C was calculated by Rayleigh for a circular aperture. We shall complete the analysis by calculating C for a rectangular aperture.

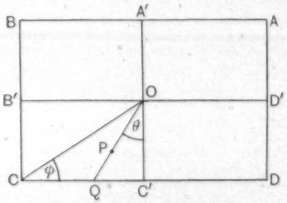


Fig. 2. Rectangular orifice.

In Fig. 2 $ABCD$ represents the rectangular open end, with sides a and b respectively. Let the angles ACD and ACB be respectively α and β. Consider a point O and through O draw the lines $A'C'$ and $B'D'$ parallel to the sides of the rectangle. From O draw a line OQ making an angle θ with OC'. Let P be a point on OQ distant r from O, and let dS_0 be an element of area at P.

Let the co-ordinates of O with respect to C be ξ, η. Let

$$I_1 = \int \frac{1}{r}\, dS_0,$$

the integration being taken over the area OCC'. It follows that

$$I_1 = \iint dr\, d\theta = \eta \int_0^{\frac{1}{2}\pi - \phi} \sec\theta\, d\theta = -\eta \log\tan(\phi/2).$$

Consider next the integral

$$H_1 = \int I_1 dS_0,$$

where dS_0 is now an element of area at O, and the integration is taken over the area ACD. Let $OC = \rho$, so that $dS_0 = \rho\, d\rho\, d\phi$.

Integrating first from O to $a \sec \phi$ with respect to ρ, we obtain

$$H_1 = \frac{a^3}{3}\int_0^{\alpha} \sec^3 \phi \log \tan(\phi/2)\, d(\cos\phi),$$

which, on substitution of

$$(1 - \cos^4\phi)/(1 + \cos\phi) \text{ for } \tan^2(\phi/2),$$

leads to the equation

$$H_1 = \frac{a^3}{6}\left| \frac{1}{\cos\phi} + \tfrac{1}{2}\tan^2\phi \,.\, \log\frac{1+\cos\phi}{1-\cos\phi}\right|_0^{\alpha}.$$

The corresponding integral H_2, due to a first integration over $OB'C$ and a second integration over ACD, is found likewise to be

$$H_2 = \frac{a^3}{6}\left| \tan\phi \log\frac{1+\sin\phi}{1-\sin\phi} - \frac{2}{\cos\phi}\right|_0^{\alpha}.$$

Hence, putting $H_3 = H_1 + H_2$, we have

$$H_3 = \frac{a^3}{6}\left\{\tfrac{1}{2}\tan^2\alpha \log\frac{1+\cos\alpha}{1-\cos\alpha} + \tan\alpha \log\frac{1+\sin\alpha}{1-\sin\alpha} - \frac{1}{\cos\alpha} + 1\right\}.$$

If H_4 is due to a first integration over the whole of $OB'CC'$ and a second over ACB, H_4 is obtained from H_3 merely by substitution of b for a and β for α.

Hence $(H_3 + H_4)$ is due to a first integration over $OB'CC'$ and a second over $ABCD$. The contributions of the remaining three rectangles must all be the same. Hence

$$S_0^2 C = 4(H_3 + H_4).$$

If $\alpha = 45°$, i.e. for a square end,

$$S_0^2 C = 8H_3 = \frac{8a^3}{6}\left\{\tfrac{3}{2}\log\frac{\sqrt{2}+1}{\sqrt{2}-1} - \sqrt{2} + 1\right\}$$
$$= (a^3/3) \times 8{\cdot}934, = S_0 a\, 8{\cdot}934/3.$$

For a circular aperture

$$S_0^2 C = S_0 (8/3)\, 2R_0.$$

Hence the ratio of C for a square aperture to its value for a circular aperture of equal area is

$$8{\cdot}934\sqrt{\pi}/16 = 0{\cdot}981.$$

§ 6. THE EXTENDED THEORY OF RESONATORS

A uniform pipe $ABCD$ of length L_n and cross-sectional area S_n is joined at the end CD to another uniform pipe $C'D'EF$ of length M_n and cross-sectional area T_n, Fig. 3. The end AB has a neck ABA_0B_0 of resistance N_n, the end correction measured from AB being α_n. The end EF communicates with a small junction box H. The whole resonator consists of any number of systems similar to this one, all communicating with the same junction box H.

This box being small, it is assumed that the air inside it moves as an incompressible fluid would. It is further assumed that the variation of the velocity potential within the box may be neglected: this is equivalent to neglecting the kinetic energy. The validity of this assumption is sufficiently obvious in all practical cases, and the whole approximate theory of resonators must be based upon it.

As we are considering resonant frequencies, no account will be taken of dissipation. Let x_n be measured from O (Fig. 3) to the right and y_n from Q to the left.

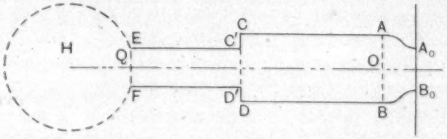


Fig. 3. Longitudinal section of element of resonator system.

Let the velocity potential for $ABCD$ be

$$\phi_n = \beta_n \sin \kappa (x_n - \alpha_n),$$

and for $C'D'EF$

$$\psi_n = \gamma_n \sin \kappa (y_n - \delta_n).$$

It follows at once that

$$\beta_n \sin \kappa (L_n + \alpha_n) = \gamma_n \sin \kappa (M_n + \delta_n),$$

and

$$S_n \beta_n \cos \kappa (L_n + \alpha_n) + T_n \gamma_n \cos \kappa (M_n + \delta_n) = 0.$$

Hence

$$S_n \cot \kappa (L_n + \alpha_n) + T_n \cot \kappa (M_n + \delta_n) = 0 \qquad \ldots\ldots(16).$$

At the junction box we have, on account of continuity and the constancy of ψ,

$$\Sigma T_n \cot \kappa \delta_n = 0.$$

Hence, from (16),

$$\Sigma \frac{T_n^2 - S_n T_n \cot \kappa (L_n + \alpha_n) \cot \kappa M_n}{S_n \cot \kappa (L_n + \alpha_n) + T_n \cot \kappa M_n} = 0. \qquad \ldots\ldots(17).$$

If α_{n0} be the end correction which would correspond to a uniform pipe of sectional area S_{n0}, which is the area of the open end A_0B_0, we have seen that

$$\tan \kappa \alpha_n = \kappa (\alpha_{n0} + N_n S_{n0}) S_n / S_{n0} \qquad \ldots\ldots(18).$$

$$= \kappa \epsilon_n, \text{ say.}$$

The formula (17) includes all resonators that are commonly met with.

The type $n = 1$.

From (17) we have at once

$$\tan \kappa M_1 \tan \kappa (L_1 + \alpha_1) = S_1 / T_1 \qquad \ldots\ldots(19).$$

This is a slightly more general form of an equation given by J. A. Aldis* and discussed by E. T. Paris†.

* *Nature*, **114**, 309 (1924). † *Nature*, **114**, 465 (1924).

Putting $L_1 = 0$ and making κM_1 small, we get the equation for the resonant frequency of a Helmholtz resonator in the form

$$\kappa^2 = S_1/M_1T_1\epsilon_1.$$

The formula for the Helmholtz resonator can also be obtained by the equating of T_1 to zero in (19). In this case

$$\tan \kappa L_1 \tan \kappa a_1 = 1,$$

which is the required result when κL_1 is small.

Dr Richardson, in his paper quoted in the introduction, assumes a cylindrical cavity of volume SL, S being the cross-sectional area of the cylinder and L its length. He then assumes that with the value of L for a pipe is incorporated an end correction. He states finally that, for an ideal pipe, the conductivity of the orifice is infinite. There seems to be confusion as to what the volume of the cavity is, and the meaning to be attached in his formula to infinite conductivity appears to be vague, unless the author has misunderstood him.

For a pipe we may put $S_1 = T_1$ and $L_1 = 0$ in (19) and we obtain

$$\tan \kappa M_1 = 1/\kappa\epsilon_1.$$

The type $n = 2$.

This type includes, among others, Rayleigh's double resonator, the Helmholtz double resonator, Boys' resonator, the dumb-bell resonator, the Kundt's tube. In formula (17) we make M_2 zero, so that we have three pipes. The formula then reduces to

$$S_1^{-1} \tan \kappa (L_1 + a_1) + T_1^{-1} \tan \kappa M_1 + S_2^{-1} \tan \kappa (L_2 + a_2) = (T_1/S_1S_2) \tan \kappa (L_1 + a_1) . \tan \kappa M_1 . \tan \kappa (L_2 + a_2) \quad \ldots\ldots (20).$$

In those cases listed above the right-hand side of (20) is small and may be neglected. For example, the formula for the Rayleigh double resonator becomes

$$\frac{1}{S_1} . \frac{L_1 + \epsilon_1}{1 - \kappa^2 L_1 \epsilon_1} + \frac{M_1}{T_1} + \frac{1}{S_2} . \frac{L_2 + \epsilon_2}{1 - \kappa^2 L_2 \epsilon_2} = 0,$$

which is a quadratic in κ^2.

By making ϵ_2 infinite we obtain the Helmholtz double resonator; and by making both ϵ_1 and ϵ_2 infinite we get the dumb-bell resonator.

In all cases there appears to be no dubiety as to the nature of the transition from "pipe" theory to "small resonator" theory.

The term impedance, undoubtedly of great use in many branches of acoustics, must be used with caution. Each term on the left-hand side of (20) is, with the omission of a constant factor, the impedance of the pipe to which it relates. In the examples given it is permissible to add the impedances in series and equate to zero for resonance. In the case of two pipes, as we have also seen, it is likewise permissible to do this. But, generally, in the case of three pipes the theory of impedance breaks down. The omission of the right-hand side of (20) in some cases leads to results that are entirely wrong.

§ 7. ACKNOWLEDGMENT

In conclusion I have to acknowledge my indebtedness to the Admiralty for permission to publish this paper.

DISCUSSION

Dr E. T. PARIS. I have listened to Mr Hanson's paper with great interest, and think that the formula which he has given for calculating the conductance of an orifice in which the cross-sectional area varies along the length is of great practical value.

In the application of Rayleigh's frequency formula to the design of a Helmholtz resonator difficulty often arises from the indefiniteness of the end-correction which must be added to the length of the orifice. In the case of wide orifices the end-correction plays a dominant part in determining the conductance, and however accurately one can calculate this correction when there is a hypothetical infinite flange, this does not assist in overcoming the practical difficulty of knowing how much to allow for end-correction when dealing with actual resonators.

In nearly all theories of resonance the only allowance made for damping is on account of the radiation of sound-waves from the orifice; but if the latter is small by far the greater part of the damping is due to viscosity. There seems to be some inconsistency in allowing the viscous forces in the orifice to enter into the exact theory of a resonator, by determining the distribution of velocity in the orifice, and at the same time ignoring altogether their dissipation effect. A complete theory of the Helmholtz resonator in which viscosity was fully allowed for would be very valuable in the practical design of resonators.

The PRESIDENT asked whether Dr Paris had measured experimentally the damping due to viscosity.

Dr PARIS said that in a cylindrical neck 1 cm. long and 0·5 cm. in diameter the total damping factor was found to be about 80 sec.$^{-1}$ whereas the calculated radiation damping was only about 2 sec.$^{-1}$

Dr E. J. IRONS. Equation (20) of the paper is readily obtained by repeated application of equation (28) of Webster's paper to which reference is made at the end of § 1: the grounds upon which Mr Hanson criticises the theory of impedance at the end of § 6 would therefore appear to be non-existent. In the *Philosophical Magazine* for May of this year I derived on impedance principles formulae for the resonators mentioned in § 6. In a paper which I have recently submitted for publication in the same journal I have extended the argument to include compound resonators having a conical horn as a component part and I have shown how "parallel" systems such as a closed pipe with two Helmholtz resonators on the side (considered by Dr Paris in 1924) may be treated. I have also considered a general "series" system which, in a manner somewhat different from Mr Hanson's equation (20), shows under what conditions Rayleigh's formula for his double resonator may be applied.

Sir RICHARD PAGET. In practice it appears that the pitch of a resonator with a tubular neck is not affected by the addition of a flange of flaring bell form; in other words, the "infinite flange" is not a right-angled extension of the neck but a curved surface which merges into a surface at right angles to the axis of the neck. It also

appears that in the design of a resonator for a given pitch the nature of the resonator wall is very important owing to variations in transparency to sound of different materials. Sound-transparency operates to raise the resonant pitch in much the same way as if the resonator walls were perforated with small holes.

Dr E. G. Richardson (communicated). I have not had time to follow Mr Hanson's interesting paper throughout, but I note that in equation (5), for the distribution of velocity across the mouth of the resonator, his theory makes $\sigma = 0{\cdot}159$ as against Rayleigh's $1{\cdot}152$. Rayleigh's value would give peaks at the edge, viscosity being neglected, of about double the central velocity; whereas on Mr Hanson's theory the peaks would scarcely be noticeable. I should like to say that the results with the hot wire showed peaks much more pronounced than those which a coefficient of $0{\cdot}159$ would indicate. With an unflanged tube the peak velocity was about double the central velocity, so that with an "infinite flange" (to which these theories apply) the ratio of the peak to the central velocity was about $3 : 2$. It is true that the hot wire measures the total velocity, but the measurements to which I refer were made just inside the pipe, where the flow is probably axial.

With regard to equation (6) of my paper, for the frequency of the pipe-resonator, Mr Hanson has given in his equation (19) a more general formula for this type. I admit that the idea of infinite conductivity is *physically* vague, but I suggest that it is a postulate necessary to an ideal pipe, i.e. one having no end-correction.

I am interested in the last paragraph of the paper, as I have been working out the theory of fingering on wood-wind instruments. Suppose you have a number of side-holes uncovered on the flute, and wish to work out the resonant frequency: does the author think that the impedance principle would break down in this case, or does he limit his stricture to the case of three or more pipes in series?

Author's reply. Dr Paris has emphasised very clearly the importance of allowing for viscosity in the complete theory of a resonator, and the figures which he gives, in his reply to the President, as to the damping which must be attributed to viscosity are very significant. An exact resonator theory is by no means easy, and in fact, in the present state of our knowledge, little more is to be gained than has already been done by Rayleigh, unless, by direct experimental investigation as to the law of resistance due to viscosity, the form and dimensions of the neck being taken into account. In the case of a Helmholtz resonator with a circular orifice of radius R, if it be assumed that there is a viscous drag proportional to $2\pi R$ and also to the mean velocity over the orifice, the theory as given in my paper or otherwise, with an obvious extension to include the drag, will readily indicate that the damping effect due to this resistance is inversely proportional to R, so that, as Dr Paris has found experimentally, for small orifices the damping effect due to viscosity is very great. It must be remembered, however, that it is easy to make assumptions upon which to build a mathematical theory and, although it seems to me that there is some evidence that the seat of the effective viscous forces is round the sides of the orifice, the assumption is too empirical to be of very great value.

In reply to Dr Irons: I agree that Webster's formula can be used as a basis from which to derive my results, but, so far as I know, Webster did not develop his theory in connexion with systems of resonators. In my study of the subject I gathered the impression that, for a system of resonators in series, the impedances of each component resonator could be added in series in order to obtain the resonant frequency, and I have been unable to find anywhere what I considered to be an adequate treatment of the subject.

My object, in § 6 of my paper, was to make clear the limits to this procedure, and I had intended to correct the impression I have given in my paper that the theory of impedance, when properly applied, breaks down. I may say that I read Dr Irons' paper in the *Philosophical Magazine* with interest, and I shall look forward to the publication of the next paper to which he refers.

Sir Richard Paget's remarks as to the pitch of a resonator with a tubular neck are very interesting, and it appears to me that they also tend to confirm that a jet motion takes place at the orifice rather than a motion, which Rayleigh favoured, resembling that of a hydrodynamically perfect fluid. For in the case of jet motion the reaction of the external air is probably little affected by the addition of a flange of flaring bell form at the end of the tube in place of an infinite plane flange.

I was particularly interested in Dr Richardson's paper, to which I have referred. The forms of the peaks which he reproduces there are noteworthy on account of their variation for frequencies varying from very slow to acoustic rapidity. These refer, I understand, to a hole in a plate. For a tube the results will be rather different, and in his reply I take it that Dr Richardson refers to a tube.

With regard to my figure of 0·159 for σ it must be remembered that this was obtained from a critical comparison with Rayleigh's theory, depending upon a precise value for the end-correction. Now, as Dr Paris has pointed out, there is considerable indefiniteness as to the end-correction which must be added to the length of a pipe. If only a slightly different value were taken for the end-correction, my value of σ would be materially altered. In fact the present state of the theory is incapable of determining the velocity distribution with a reasonable approach to accuracy. The case which Dr Richardson takes at the end of his reply in relation to fingering on wood-wind instruments is of a somewhat more general type than can be dealt with at once by my formula, but it can be treated by a method similar to mine or by an application of impedance theory. Dr Richardson admits that the idea of infinite conductivity is physically vague, and this is one of my reasons for objecting to the introduction of electrical analogies into the theory under discussion.

ESCAPEMENT ERRORS OF PENDULUM CLOCKS

By E. C. ATKINSON, M.A.

Communicated by Prof. C. V. Boys, September 16, 1929.
Read and discussed November 22, 1929.

ABSTRACT. Prof. R. A. Sampson's theory of maintenance is applied to find equations for the errors of rate caused by changes in intensity and limits of the impulse and in friction of moving parts of the impulse mechanism. As an example, the equations are worked out for the Cottingham clock and applied to find best working conditions. Rigidity of the stops limiting the impulse is found to be the most important point in designing this type. The equations are also given for the dead beat escapement and show that working conditions which are good so far as friction is concerned are bad for barometric changes. The method used for computing "remote" error in these cases must be modified when the inertia of the impulse mechanism is appreciable as in the Shortt clock. The further remote escapement errors due to changes in amplitude introduced by variation of temperature and density of the air are the same as those produced by changes in escapement but are associated with different direct errors which are excluded from this paper.

The following table of symbols will be found convenient:

πa, πb, πc	accelerations due to escapement friction.
A, B	components of maintenance vector.
$D.da/a$	remote error.
E	supply of energy to pendulum per cycle.
f	oscillation frequency.
g	acceleration due to gravity.
h	coefficient of dt in maintenance integral B (§ 9).
l	distance between centre of mass and point of support of pendulum.
M	mass of pendulum.
Mk^2	moment of inertia of pendulum about its point of support.
ω	$2\pi f$ for free pendulum.
n	$2\pi f$ for maintained pendulum.
t	time measured in seconds, generally from zero when $\theta = 0$.
x	defect from final amplitude (§ 5).
X, Y	abbreviations for τ functions (§ 8).
α	amplitude.
β	small function of τ (§ 4).
δ	decrement factor, $-2\delta\dot{\theta}$ = resistance acceleration.
η	$\frac{dE}{E}\Big/\frac{d\alpha}{\alpha}$ (§§ 3, 4, etc.).
θ	angle between pendulum and vertical.
$\lambda.d\alpha/\alpha$	amplitude error, of which circular error is a component.
$\pi\sigma$	maintenance acceleration.
τ	phase in radians ($= nt$).

The proportional change of rate is dn/n or $86400\ (dn/n)$ (s/d).

Unless otherwise stated, angles are measured in radians, while velocities and accelerations are all angular.

§ 1. INTRODUCTION

ELEVEN years ago Prof. R. A. Sampson* demonstrated a general method for calculating deviations in rate produced by escapements. I wish to show in this paper how this method may be applied to finding errors in rate resulting from changes in the working conditions. In order to be brief I have excluded variations of temperature and air density although errors due to these causes also include escapement terms. For a similar reason I have concentrated on the method and refer only briefly to the use which can be made of the results.

The equations of error exhibit the merits and defects of an escapement, they point out the essential features in design, and from them the most favourable working conditions can be calculated. All recent improvements in timekeeping have been made by securing comparative uniformity in working conditions. Little attention has been directed to the problem of minimising the effect of variations which still take place.

§ 2. VECTORS

The following sketch of well-known facts will serve to introduce the symbols to be employed. In the absence of resistance and circular equation, the motion of a pendulum would be harmonic, so that the displacement θ of the rod from the

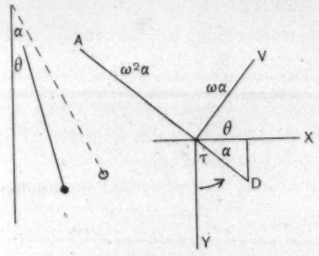


Fig. 1. Displacement, velocity and acceleration vectors.

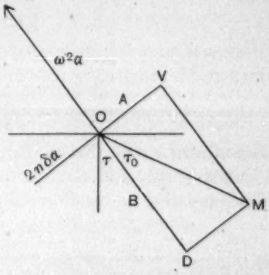


Fig. 2. Resistance and maintenance vectors.

vertical is represented by the projection on OX of a *displacement vector* OD of length representing the amplitude α, which rotates with angular velocity ω. $\theta = \alpha \sin \tau$ where $YOD = \tau = \omega t$. The angular velocity is similarly derived from a *velocity vector* OV of length $\omega\alpha$ rotating 90° ahead of OD. The *acceleration vector* $\omega^2\alpha$ is 180° ahead of OD. Thus $Mk^2\omega^2 = Mgl$, Mk^2, g and l having their usual meanings.

In order that a constant amplitude may be maintained when the pendulum encounters friction, a maintaining force must be applied. Provided the acceleration due to friction were $-2\delta\dot{\theta}$ † (i.e. always proportional to the angular velocity), motion would still be harmonic if the maintaining force were harmonic, having a period

* *Proc. R.S. E.* **38**, p. 75 (1918).

† 2δ is used for κ in order to avoid fractions.

equal to that of the pendulum, which will now be $2\pi/n$ instead of $2\pi/\omega$. Fig. 2 shows OM, the maintenance-acceleration vector with components A, B along OV, OD respectively, and the resistance-acceleration vector $2n\delta\alpha$.

It follows that central acceleration

$$-n^2\alpha = -\omega^2\alpha + B.$$

Δn, or $(n - \omega)$, is the *deviation* from natural frequency and is of the order $\omega \times 10^{-4}$. Thus the equation $-\Delta n\,(\omega + n)\,\alpha = B$ may without appreciable error be written

$$-\Delta n/n = B/2n^2\alpha \qquad \ldots\ldots(1).$$

It also follows from Fig. 2 that, for steady motion,

$$2n\delta\alpha = A \qquad \ldots\ldots(2).$$

(1) and (2) are the fundamental equations of maintenance.

If the motion be steady (but *not otherwise*) $-\Delta n/n = (\delta/n)\,(B/A)$.

In calculating errors, this form of the equation is best avoided, but results are often translated into a form involving δ/n by a final multiplication by

$$2n\delta\alpha/A\ (=1).$$

If E be the supply of energy during each cycle,

$$E = \pi\,.\,Mk^2\,.\,A\alpha \qquad \ldots\ldots(3).$$

§ 3. DECREMENT

If the resistance law assumed above were true, amplitude of the pendulum when maintenance is removed would diminish exponentially so that

$$-\frac{1}{\alpha}\frac{\Delta\alpha}{\Delta t} = \delta = \text{constant}.$$

Experiment shows that the left-hand side of this equation increases with α, and δ must be regarded, not as a constant, but as a function of α.

Differentiating (3) logarithmically after substituting $2n\delta\alpha$ for A, we find that

$$\frac{dE}{E} = \frac{2d\alpha}{\alpha} + \frac{d\delta}{\delta} = \eta_1\frac{d\alpha}{\alpha} \text{ where } \eta_1 - 2 = \frac{d\delta}{\delta}\Big/\frac{d\alpha}{\alpha} \qquad \ldots\ldots(4, 5).$$

η_1 is never less than 2 and rarely, if ever, exceeds 3.

Decrement experiments furnish no information as to the resistance at each moment, and very little is known about this. Since the pendulum is moving in air which it has already disturbed, the relative velocity cannot in general be the same as that of the pendulum. Resistance as indicated in Fig. 2 is in "A" direction and has consequently no effect on the rate*, but it is clear that there is a "B" component as well, which (although probably smaller than $Mk^2\,.\,2n\delta\alpha$) affects frequency perhaps 1000 times as much: this component is not negligible†, and changes with amplitude.

Amplitude equation. The frequency of a pendulum decreases as amplitude increases since the centre of gravity moves in a circle. The deviation from the rate which would obtain with infinitesimal amplitude is called the circular equation. We have

* i.e. when amplitude is maintained constant.

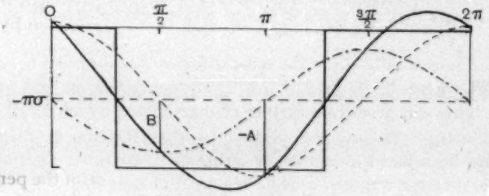
† Pointed out by Stokes, *Camb. Phil. Soc. Trans.* **9** (1850).

just seen that there is another deviation to which must be added change in the inertia effect of the air. Moreover spring suspension tends to reduce the circular equation, and finally lack of rigidity (or rather of perfect elasticity) of the pendulum bracket will also cause change of frequency with amplitude. It is logical to speak of the amplitude equation, leading to *amplitude error**, $\lambda . d\alpha/\alpha$ and to confess that, in using $(\alpha^2/8)\,(d\alpha/\alpha)$ in place of this, we are ignorant of the real value of amplitude error.

§ 4. ESCAPEMENTS. *A* AND *B* AS DEFINITE INTEGRALS

Harmonic maintenance as assumed in § 2 can be secured in electrical systems, but no escapement has yet been devised to do the same thing for the pendulum. The impulse however is always periodic, the period being $2\pi/n$. Consequently the acceleration can appropriately be expressed as a Fourier's series

$$\pi\sigma\dagger = A_0 + A\cos\tau + B\sin\tau + A_2\cos 2\tau + B_2\sin 2\tau + \text{etc.}$$



$$A = \int_0^{2\pi} \sigma\cos\tau\, d\tau \qquad B = \int_0^{2\pi} \sigma\sin\tau\, d\tau$$

Fig. 3. Cottingham impulse and equivalent sinusoidal impulse with its components.

Prof. Sampson in his paper shows that, whilst A_0, A_2, B_2, etc. affect the position of the pendulum at any moment, their effect on rate and amplitude is negligible and that, in the determination of these two quantities, A and B alone need be considered. Thus equations (1) to (5) are true for escapements when

$$A = \frac{1}{\pi}\int_0^{2\pi} \pi\sigma\cos\tau\, d\tau = \int_0^{2\pi} \sigma\cos\tau\, d\tau \qquad \text{.....(6),}$$

$$B = \int_0^{2\pi} \sigma\sin\tau\, d\tau \qquad \text{.....(7).}$$

In order that A and B may be computed, σ must be expressed as a function of τ. As a rule σ is (primarily at least) a function of θ and, owing to the escapement, it is no longer true that $\theta = \alpha\sin\tau$. σ should be expressed as another series and the coefficients of neither series can be computed without a knowledge of those of the other, except by successive approximation. Is the first approximation obtained by using $\alpha\sin\tau$ for θ good enough for A and B? On the assumption that

$$\theta = a + b\sin(\tau + \beta)$$

for a second approximation (β being a small function of τ) the first approximation

* Coined by J. M. Bloxham, who states that it is much smaller than the circular error. (*R.A.S. Memoirs*, **22**, 1853).

† $\pi\sigma$ is used to avoid the fraction $\frac{1}{\pi}$ in integrals.

can be checked. I find that it suffices for any reasonable value of δ provided σ does not change rapidly near the extremities of the swing. It will be noticed that in this exceptional case a small change in $\sin\tau$ (i.e. θ/α) may be accompanied by a large change in $\cos\tau$ and would appreciably affect $\int\sigma d(\cos\tau)$ if σ is changing rapidly at the same time.

This exceptional case does not occur in practice (except when the clock is on the point of stopping!).

§ 5. ERRORS; DIRECT AND REMOTE

(*a*) An error or change of deviation $-dn/n$ results from a change dB in B. From equation (1)

$$-d_d n/n = dB/2n^2\alpha \qquad \ldots\ldots(8).$$

This is called the *direct error* because it follows immediately on the change dB. Equation (8) means that the clock is losing $86400\,(dB/2n^2\alpha)$ (s/d) on its previous rate.

(*b*) When A increases by dA at the same time, energy supplied to the pendulum increases by dE_1 where $dE_1/E = dA/A$, α will commence to grow and will eventually reach $\alpha + d\alpha$. This will produce a further change in rate which I call *remote error*. E is the nett energy reaching the pendulum after the energy released by the escapement has been taxed at source by escapement friction. The tax usually increases with α, giving a remote (usually negative) change dE_2

where
$$\frac{dE_2}{E} = \frac{d(A\alpha)}{d\alpha}\cdot\frac{d\alpha}{A\alpha}.$$

Equations (3) to (5) show that

$$\eta_1\frac{d\alpha}{\alpha} = \frac{dA}{A} + \frac{d(A\alpha)}{Ad\alpha}\cdot\frac{d\alpha}{\alpha} = \frac{dA}{A} - \eta_2\frac{d\alpha}{\alpha}$$

or
$$\eta\frac{d\alpha}{\alpha} = \frac{dA}{A}, \text{ where } \eta = \eta_1 + \eta_2 \qquad \ldots\ldots(9).$$

After the initial change dA the energy supplied by the escapement remains unchanged except for increasing frictional losses, so that η_2 represents the ratio of dE/E to $d\alpha/\alpha$ for escapement friction, just as η_1 represents this ratio for pendulum friction. Whilst η_1 ranges from 2 to 3, η_2 should be a small fraction.

The remote error consists of amplitude error (§ 3) and a further escapement error
$$-\frac{d_r n}{n} = \left[\frac{1}{2n^2}\frac{d}{d\alpha}\left(\frac{B}{\alpha}\right) + \frac{\lambda}{\alpha}\right]d\alpha = \left[\frac{1}{2n^2}\left(\frac{dB}{d\alpha} - \frac{B}{\alpha}\right) + \lambda\right]\frac{d\alpha}{\alpha} = D\frac{d\alpha}{\alpha} \quad \ldots(10).$$

By (9) and (2) the total error $-d_d n/n - d_r n/n$ may be written

$$-\frac{dn}{n} = \frac{\delta}{n}\frac{dB}{A} + \frac{1}{\eta}\left[\frac{\delta}{n}\left(\frac{dB}{d\alpha} - \frac{B}{\alpha}\right) + \lambda\right]\frac{dA}{A} \qquad \ldots\ldots(11).$$

This equation is the general equation of error. A and B depend on intensity of impulse and its range and on friction in various parts of the escapement. To change in any one of these quantities, there is a corresponding error.

Transitional state. Having made some change in working conditions, we should

reckon time by making allowance for the new error. The remote part of this error, however, is not present at first: when the amplitude is short by x of its final value $\alpha + d\alpha$ the remote error is only $D\,(d\alpha - x)/\alpha$, so that the clock is gaining $D\,(x/\alpha)$ seconds per second on its final rate. At this time the escapement supplies energy $\pi Mk^2\{(A + dA)\,\alpha + (d\alpha - x).d\,(A\alpha)/d\alpha\}$ per cycle whilst the pendulum loses $\pi Mk^2.2n\delta\alpha^2\{1 + \eta_1\,(d\alpha - x)/\alpha\}$. The nett gain of energy is

$$\pi Mk^2.2n\delta\alpha^2\{\eta_1\,(x/\alpha) - (x/\alpha)\,d\,(A\alpha)/Ad\alpha\}$$

per cycle or $\frac{1}{2}n\,Mk^2.2n\delta\alpha.\eta x$ per second, which increases the amplitude by $-dx/dt$ and the pendulum energy by $-Mgl\alpha\,(dx/dt)$ or $-n^2Mk^2\alpha\,(dx/dt)$.

Hence $$\eta\delta x = -dx/dt \text{ and } x = d\alpha e^{-\eta\delta t} \qquad \text{.....(12).}$$

The gain in time during the transition as reckoned above

$$= \frac{D}{\alpha}d\alpha\int_0^\infty e^{-\eta\delta t}dt = \frac{D}{\eta\delta}\cdot\frac{d\alpha}{\alpha} \text{ seconds} \qquad \text{.....(13).}$$

§ 6. COTTINGHAM CLOCK

It will be seen that the following routine should be followed for calculating an error:

(1) B and $A\alpha$ are found from impulse and frictions by integration (6), (7).

(2) D is calculated, the principal terms of B only being retained (10).

(3) $\eta\,(d\alpha/\alpha)$ is computed from $A\alpha$ variations in all elements being assumed (9). For variation of each element the corresponding error equation can then be written down.

As an example, I select the Cottingham escapement in which a gravity lever-rests against a stop S connected to the armature of an electromagnet. Fig. 4 represents the state of affairs when the pendulum is about to pick up the lever. The pick up completes a circuit and S is withdrawn to S' to be returned again to S when the circuit is broken during the return swing of the pendulum.

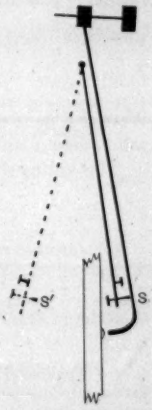


Fig. 4. The Cottingham clock.

Pivot friction increases the uniform escapement acceleration $-\pi\sigma$ to $-\pi\,(\sigma + a)$ in the range θ_1 to α. Similarly the acceleration is $-\pi\,(\sigma - a)$ in the range α to θ_2. Impulse acceleration may be briefly expressed as

$$[-\pi\,(\sigma + a)]_{\tau_1}^{\pi/2} + [-\pi\,(\sigma - a)]_{\pi/2}^{\tau_2}.$$

$$\text{(1)}\quad B = -(\sigma + a)\int_{\tau_1}^{\pi/2}\sin\tau\,d\tau - (\sigma - a)\int_{\pi/2}^{\tau_2}\sin\tau\,d\tau$$

$$= -\sigma\,(\cos\tau_1 + \cos\tau_2) - a\,(\cos\tau_1 - \cos\tau_2) \qquad \text{.....(14).}$$

Similarly $$A = \sigma\,(\sin\tau_1 - \sin\tau_2) - a\,(2 - \sin\tau_1 - \sin\tau_2)$$

or $$2n\delta\alpha^2 = A\alpha = \sigma\,(\theta_1 - \theta_2) - a\,(2\alpha - \theta_1 - \theta_2) \qquad \text{.....(15).}$$

(2) Since $a \sin \tau_1 = \theta_1 = \text{const.}$

$$d\tau_1 = -\tan \tau_1 \frac{da}{a}$$

$$dB/da = -(\sigma/a)(\sin \tau_1 \tan \tau_1 + \sin \tau_2 \tan \tau_2)$$

$$\frac{1}{2n^2}\left(\frac{dB}{da} - \frac{B}{a}\right) = -\frac{2n\delta a}{2n^2\sigma(\sin\tau_1 - \sin\tau_2)} \cdot \frac{\sigma}{a}\left(\frac{\sin^2\tau_1}{\cos\tau_1} - \cos\tau_1 + \frac{\sin^2\tau_2}{\cos\tau_2} - \cos\tau_2\right)$$

$$= (\delta/n)(2 - \sec\tau_1 \sec\tau_2)\cot(\tau_1 - \tau_2)/2 \qquad \text{.....(16).}$$

(3) $$\eta_1 \frac{d\alpha}{a} = \frac{dA}{A} + \frac{d}{Ada}(A\alpha) = \frac{d\sigma}{\sigma} - \frac{da}{\sigma}\frac{2\alpha - \theta_1 - \theta_2}{\theta_1 - \theta_2} + \frac{d\theta_1 - d\theta_2}{\theta_1 - \theta_2} - \frac{2a}{\sigma(\theta_1 - \theta_2)}\frac{da}{a}$$

or $$\eta \frac{d\alpha}{a} = \frac{d\sigma}{\sigma} - \frac{da}{\sigma}\cdot\frac{2\alpha - \theta_1 - \theta_2}{\theta_1 - \theta_2} + \frac{d\theta_1 - d\theta_2}{\theta_1 - \theta_2} \text{ where } \eta = \eta_1 + \frac{2a}{\sigma(\theta_1 - \theta_2)} \text{...(17).}$$

(*a*) *Limits errors.* If θ_1 increases by $d\theta_1$, $d\tau_1 = d\theta_1/a\cos\tau_1$

$$-\frac{d_a n}{n} = \frac{\sigma \sin\tau_1}{2n^2 a}\cdot\frac{d\theta_1}{a\cos\tau} = \frac{2n\delta a^2 d\theta_1}{\sigma(\theta_1 - \theta_2)}\cdot\frac{\sigma}{2n^2a^2}\tan\tau_1 = \frac{\delta}{n}\tan\tau_1 \cdot \frac{d\theta_1}{\theta_1 - \theta_2},$$

$$-\frac{d_r n}{n} = \frac{1}{\eta}\left[\frac{\delta}{n}(2 - \sec\tau_1\sec\tau_2)\cot\frac{\tau_1 - \tau_2}{2} + \lambda\right]\frac{d\theta_1}{\theta_1 - \theta_2}.$$

Including also change $d\theta_2$, the total error is

$$-\frac{dn}{n} = \frac{\delta}{n}(\tan\tau_1 d\theta_1 + \tan\tau_2 d\theta_2)/\overline{\theta_1 - \theta_2}$$

$$+\left[\frac{\delta}{n}(2 - \sec\tau_1\sec\tau_2)\cot\frac{\tau_1 - \tau_2}{2} + \lambda\right]\frac{d\theta_1 - d\theta_2}{\eta(\theta_1 - \theta_2)}.$$

Wear at S or at the pendulum contact gives $d\theta_1 = d\theta_2$ so that the resulting error will be zero if $\tan\tau_2 = -\tan\tau_1 (= -\tan\tau)$ or $\theta_2 = -\theta_1 (= -\theta)$.

This setting should therefore be adopted and the equation then takes the form

$$-\frac{dn}{n} = \left(\frac{\delta}{n}\tan\tau + \frac{2}{\eta}\cdot\frac{\delta}{n}\cot 2\tau + \frac{\lambda}{\eta}\right)\frac{d\theta}{2\theta} \qquad \text{.....(18).}$$

(*b*) *Intensity error.* For $d\sigma$, when $\theta_2 = -\theta_1$

$$\frac{dB}{2n^2a} = -\frac{2n\delta a}{2\sigma\sin\tau}\cdot\frac{2d\sigma\cos\tau}{2n^2a} = -\frac{\delta}{n}\cot\tau\frac{d\sigma}{\sigma} - \frac{dn}{n}$$

$$= \left(-\frac{\delta}{n}\cot\tau + \frac{2}{\eta}\frac{\delta}{n}\cot 2\tau + \frac{\lambda}{\eta}\right)\frac{d\sigma}{\sigma} \qquad \text{.....(19).}$$

(*c*) *Friction error.* The direct error is zero when $\theta_2 = -\theta_1$,

and $$-\frac{dn}{n} = -\left(\frac{2}{\eta}\cdot\frac{\delta}{n}\cot 2\tau + \frac{\lambda}{\eta}\right)\frac{da}{a}.$$

§ 7. INTERPRETATION OF THE EQUATIONS

(*a*) Showed that it was desirable that the make and break positions should be symmetrical with regard to the vertical, this also makes the clock tick evenly.

$\tan\tau + (2/\eta)\cot 2\tau$ is always positive and has a minimum value $2(\eta - 1)^{\frac{1}{2}}/\eta$ when $\tan\tau = (\eta - 1)^{-\frac{1}{2}}$. Thus the limits error is never less than

$$\{2(\eta - 1)^{\frac{1}{2}}\cdot\delta/n + \lambda\}\, d\theta/2\eta\theta.$$

(*b*) $\cot\tau - (2/\eta)\cot 2\tau$ is always positive and has minimum value $2(\eta-1)^{\frac{1}{2}}/\eta$ when $\tan\tau = (\eta-1)^{\frac{1}{2}}$. Intensity error is therefore zero for this value when $\lambda = 2(\eta-1)^{\frac{1}{2}}\delta/\eta$. This gives a minimum value of α for balancing out the error. For larger values of α suitable values of θ can be found for this purpose.

(*c*) The friction error can always be removed with a value of τ between $\pi/4$ and $\pi/2$.

For each error there is an optimum value of τ differing from that for the others. A numerical example will be useful. I designed an escapement of this type $2\frac{1}{2}$ years ago, and sought, of course, to make a, $d\sigma/\sigma$ and $d\theta$ as small as possible. To make $d\sigma/\sigma$ very small needs only moderate workmanship. I hoped that I had made $d\theta$ small with sturdy stops and stiff armature lever, and finally adopted the values $\alpha = 44'$, $\theta = 35'$ for balancing the friction error. For this amplitude $\delta/n \fallingdotseq \cdot 33 \times 10^{-4}$, $\eta_1 \fallingdotseq 2{\cdot}1$. Recent decrement experiments show that the decay of amplitude, when the impulse lever is in action, is not more than 2 per cent. in excess of decay when the pendulum is free so that $a = 0{\cdot}016\sigma$ or less. Since there is no lubrication of the steel-on-glass bearing, 0·05 seems a liberal estimate for da/a, so that da/σ is not likely to exceed 0·001. I have assumed that

$$\lambda = \alpha^2/8 = \cdot 205 \times 10^{-4},$$

in ignorance of its real value. Suppose λ were really twice as large the maximum friction error would be $-\cdot 2 \times 10^{-7}$ or 0·002 s/d.

This change in a would produce an amplitude change of $1\frac{1}{2}''$ whereas (even for constant temperature) larger variations occur. If I am right in supposing that $d\sigma/\sigma$ is really small, there is still room for improvement, to make $d\theta$ smaller.

In this clock the limits error seems to be the chief trouble.

On the assumption that $\lambda = \alpha^2/8$, the value of this error, when $\alpha = 44$, and the other values are as given above, is

$$(2 \times 0{\cdot}33 \times 1{\cdot}05 \times 10^{-4} + 0{\cdot}205 \times 10^{-4})\, d\theta/(70 \times 2{\cdot}1)$$

or $(0{\cdot}69 + 0{\cdot}205) \times 0{\cdot}7 \times 10^{-6}$ per minute of arc. Thus the error is more than four times the circular error.

τ may be changed considerably from its optimum value without appreciable increase of this error. For the above value, which $= 0{\cdot}63 \times 10^{-6}$ i.e. 0·05 s/d, the amplitude change would be $19''$. Closer working than this is possible.

Since $d\theta$ does not necessarily increase with θ, the clock should work best with small amplitude of swing and a value for θ a good deal larger than the optimum one.

To secure zero intensity error with my clock an amplitude of about 100′ would be necessary (allowing for increased values of δ/n and η). It would not pay to balance this error, since it is not troublesome, and the larger amplitude would be bad for the limits error.

This discussion may be illustrated by a table of errors in milliseconds per day corresponding to an increase of one second of arc in amplitude for my clock working under conditions ($\alpha = 44'$, $\theta = 35'$) calculated to make the frictional error small. The circular error is given as a standard with which the escapement

errors may be compared. I am now satisfied that changes in friction are very small. θ should be smaller if change in range is the main offender.

Cottingham clock E. α increases 1″

Change in impulse	Range	Intensity	Friction	Circular error
Loss of rate, ms/d	2·82	− 1·46	0·11	0·67

§ 8. GRAHAM'S ESCAPEMENT

As a further example of errors, I will give the results of an analysis of Graham's escapement, on the assumption that the live faces have been shaped so that the impulse is uniform. In addition to the main impulse the following frictional accelerations are included: $-\pi a$ on live faces, $\pm \pi b$ on dead, and $-\pi c$ due to the anchor pivot.

Thus the impulse formula is

$$2\pi\,[\sigma - a]_{\tau_1}^{\tau_2} - 2\pi\,[b]_{\tau_2}^{\pi/2} + 2\pi\,[b]_{\pi/2}^{\pi+\tau_1} - 2\pi\,[c]_{-\pi/2}^{\pi/2}$$

since the escapement operates twice per cycle.

$$B = 2\,(\sigma - a + b)\,(\cos\tau_1 - \cos\tau_2).$$

$$A\alpha = 2\,(\sigma - a)\,(\theta_2 - \theta_1) - 2b\,(2\alpha - \theta_1 - \theta_2) - 4c\alpha.$$

$$\eta\frac{d\alpha}{\alpha} = \frac{d\sigma - da}{\sigma} - \frac{2\alpha - \theta_1 - \theta_2}{\theta_2 - \theta_1}\frac{db}{\sigma} - \frac{2\alpha}{\theta_2 - \theta_1}\frac{dc}{\sigma},$$

$$\eta_2 = \frac{2\,(b + c)\,\alpha}{\sigma\,(\theta_2 - \theta_1)}.$$

Direct errors involve a term $X = (\delta/n)\tan(\tau_1 + \tau_2)/2$ and remote errors an expression $Y = (\delta/n\eta)\,(2 + \sec\tau_1 \sec\tau_2)\tan(\tau_1 + \tau_2)/2 - \lambda/\eta$, then

$$-\frac{dn}{n} = \frac{d\sigma - da}{\sigma}(X - Y) + \frac{db}{\sigma}\left(X + Y\frac{2\alpha - \theta_1 - \theta_2}{\theta_2 - \theta_1}\right) + \frac{dc}{\sigma}\cdot Y\frac{2\alpha}{\theta_2 - \theta_1}$$

with the orthodox zero settting $X = 0$, $Y = -\lambda/\eta$. By delaying the impulse, the coefficient of $(d\sigma - da)/\sigma$ is reduced, since $Y + \lambda/\eta > X$. Without entering into details it may be said that it is not practicable to reduce this coefficient to zero. This first error arises from train friction and live pallet friction. The error is reduced by delaying the impulse. Still more is error due to anchor pivot friction reduced in this way, whilst for dead pallet friction it would be possible to retard too much.

It would appear that the orthodox setting is not the best, so far as escapement errors are concerned. It should be said, however, that by delaying the impulse the barometric error is increased. This error would be reduced by application of the impulse before the zero position has been reached, but this is not practicable.

Limits errors have not been given. Such errors, due to change of level, balance out with alternate swings. Play in the anchor lever slot which embraces the pendulum can produce pronounced changes in action when oil is present, for

sometimes surface tension may maintain one-side contact throughout, whilst at other times normal rocking takes place. I advocate that the anchor should be counterpoised so that one-side contact may always be secured.

§ 9. METHODS OF CALCULATING D

In the calculation of the remote error, $(d/d\alpha)(B/\alpha)\,d\alpha$ has been calculated above as $(dB/d\alpha - B/\alpha)\,d\alpha/\alpha$. Other methods are sometimes preferable or even necessary.

(1) If an impulse lever reacts on a smooth cylindrical surface fixed to the pendulum (having its axis parallel to the axis of swing), the impulse with zero setting is $[\pi\sigma\theta]_{\theta_1}^{\theta_2}$. Here

$$B = \int_{\tau_1}^{\tau_2} \sigma\alpha \sin^2\tau\, d\tau = \frac{\sigma\alpha}{4}[2\tau - \sin 2\tau]_{\tau_1}^{\tau_2}$$

In this case it is simpler to calculate $d/d\alpha\,(B/\alpha)$.

(2) $\dfrac{d}{d\alpha}\displaystyle\int_{\tau_1}^{\tau_2} h\,d\tau$ can be calculated as $\displaystyle\int_{\tau_1}^{\tau_2} \frac{d\,(h\,d\tau)}{d\alpha}$. We have to compare an element for the enhanced value of α with one for the original value of α. If we choose pairs having the *same values of* τ, these will be identical, provided that h is independent of α. In this case the only change is at the limits and

$$\frac{d}{d\alpha}\int_{\tau_1}^{\tau_2} h\,d\tau = [h\,d\tau]_{\tau_1}^{\tau_2} = -\frac{1}{\alpha}[h\tan\tau]_{\tau_1}^{\tau_2}.$$

Thus in the above case we have $-\dfrac{\sigma\,d\alpha}{\alpha}[\sin^2\tau\tan\tau]_{\tau_1}^{\tau_2}$ at once.

(3) In the Shortt clock, inertia of the impulse lever plays an important part, so that the impulse is a somewhat complicated function, not only of pendulum position but also of its velocity, and the only method I have found practicable is to calculate the impulse for a series of values of θ.

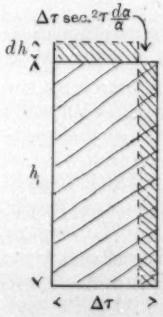


Fig. 5. Showing $d\,(h\Delta\tau)$.

In such a case it is necessary to compare two elements $h\Delta\tau$ for the *same values of* θ. Here $\Delta\tau$ changes as well as h (Fig. 5). Thus, τ changes to $\tau - \tan\tau\,(d\alpha/\alpha)$: hence $\Delta\tau$ becomes $\Delta\tau - \Delta\tau \sec^2\tau\,(d\alpha/\alpha)$. Then

$$\frac{d}{d\alpha}\int_{\tau_1}^{\tau_2} h\,d\tau = \int_{\tau_1}^{\tau_2}\left(\frac{dh}{d\alpha} - \frac{h\sec^2\tau}{\alpha}\right)d\tau.$$

It will be noticed that, if $h = f(\tau)$,

$$\frac{dh}{d\alpha} = -\frac{dh}{d\tau}\frac{\tan\tau}{\alpha},$$

and then the integral becomes

$$-\frac{d\alpha}{\alpha}\int_{\tau_1}^{\tau_2} d\,(h\tan\tau) = -\,[h\tan\tau]_{\tau_1}^{\tau_2}\frac{d\alpha}{\alpha}$$

as before.

§ 10. SHORTT CLOCK

Of escapements which I have attempted to analyse, the Shortt is the most interesting and baffling. I sought to account for errors by assuming that they were due alone to friction in the lever and wheel pivots, but found that no reasonable change in these would account for the variations in amplitude observed by Sampson* in the famous "SH_0." The average moment at which the impulse starts is slightly affected by the degree of control called for by the slave pendulum, but this again fails to account for results. Possibly there may have been small changes in the position of the lever pivot in relation to the pendulum bracket.

The foregoing method for ascertaining errors is applicable to all types of drive, but results are reliable, of course, only in so far as data are adequate. Some clocks have scape wheels so massive that the neglect of inertia seems scarcely permissible. In the Cottingham clock, inertia of the impulse lever is most significant at the moment of collision with the pendulum, but even here the effect appears to be unimportant.

In all escapements reacting on the pendulum near the top, the suspension spring is deflected. No account of this deflection has been taken in this paper, but I am sure that its effect is not negligible.

DISCUSSION

Mr Rollo Appleyard referred to an early device due to Sir Francis Ronald for overcoming coherence in the electric pendulums employed at the beginning of the nineteenth century. The cure was to stretch a steel wire across the path of the flexible pendulum rod. This produced a flick which overcame the sticktion at contact.

* *Proc. R.S. E.* **44**, 71 (1924).

Mr F. Hope-Jones. I am surprised that Mr Atkinson has had the temerity to invite me to hear his paper on escapement errors and to take part in the discussion upon it when only a few years ago I said that—so far as clocks of precision were concerned—escapements were dead, killed by the free pendulum, just as the art of steel engraving had been killed by photogravure. Until a few years ago, clocks of precision were defined as those attempting to measure time with an accuracy of one part in a million: they were then lifted to an altogether higher plane, with an accuracy nearer to one part in 100 millions.

Mr Atkinson's equations will give the layman a good idea of the difficulty of time measurement of that order. It has now been proved that it cannot be done unless all these complex considerations—barometric errors, escapement errors, and all the ills that clocks are heirs to—are eliminated by putting the pendulum *in vacuo*, and leaving it alone, except for its impulse once every half-minute. If this impulse, beginning from zero when it is assisting gravity, could finish equally after zero when it diminishes gravity, then there would be no escapement error.

The Cottingham clock which Prof. Sampson found so useful for his mathematical analysis of escapement errors is Froment, 1854. Its fault is common to all gravity escapements, that of using the pendulum to raise a lever at the end of its swing and allowing the lever to return with it to a lower position, the difference being the impulse. Why trouble to make a mathematical analysis of an escapement which is so fundamentally wrong as to subject its pendulum to constant interference at the end of each swing, when it is doing most of its job of time measurement and where it is most sensitive to disturbances of any kind?

With regard to the pivot friction in the Shortt clock, I understood Mr Atkinson to suggest, as many have suggested, that since the impulse must be 30 times normal when delivered only once every half-minute instead of every second, the pivot friction of the gravity lever and variations thereof must also be 30 times normal and that you are no better off. But surely the factor that matters is the time during which such disturbances are free to act on the pendulum. The total period during which a pendulum is subject to interference should always be compared with time itself, that is to say, the total time measured. A pendulum is only safe from interference when it is absolutely free. It would be instructive to review all known escapements from that standpoint and to place them in an order of merit based upon the degree of their freedom. Such a list would begin with Harrison's Grasshopper, in which the escapement is at work all the time and the pendulum is never free, and would end with the Shortt free pendulum in which the interference is confined to one part in one hundred of the time measured. But there is a practical consideration of overwhelming importance. The total energy which has to be imparted to a free pendulum *in vacuo* is that absorbed by the flexure of the spring, and it is only 0·08 gm. cm. per half-minute, or 0·4 gm. falling 2 mm. If you attempt to divide that into 30 impulses your gravity lever must not weigh more than one fifteen-hundredth part of an ounce, and must not fall more than one-eighth of an inch. That is impracticable; but even if you could do it the ratio of pivot friction (and of course variation of pivot friction) would be enormously increased. In reducing the size of the impulse,

you cannot cut down the friction in the same proportion. And it would be useless as a switch; even in its half-minute form it was insufficient to serve my purpose, hence the origin of its use to release the heavier lever of a Synchronome switch. This dissociation of the two functions and the provision of a separate lever for each was designed by Mr Shortt, and contained merits beyond those contemplated at the time.

AUTHOR'S REPLY. Although the errors of the Shortt clock are theoretically the same as if the impulse were subdivided, I admit that it would be inexpedient to do this in practice. I agree that the friction would thereby be increased. When Mr Hope-Jones says that "escapements" are dead he uses the word in a more restricted sense than I intend. I believe that I have conformed to custom, but this is perhaps misleading and the word seems to suggest toothed wheels.

I think that the Cottingham clock is a good illustration of the application of the theory, just because, as Mr Hope-Jones says, the impulse acts at the end of the swing and so fails to conform to Airy's criterion. Analysis shows that it is not necessarily bad on this account. Prof. David Robertson* would explain this by saying that the lever and pendulum form one piece during the impulse, whilst interference is confined to the moments when the lever is picked up and dropped. The lever friction must, however, not be omitted in computing the errors.

* *Horological Journal*, 71, 272 (1929).

APPARATUS FOR DETERMINING THE SPECIFIC HEAT OF A MATERIAL IN POWDER FORM

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ABSTRACT. A calorimeter suitable for heavy powdered materials, such as dry clay, is described. It utilises the electrical method, and embodies a special form of stirrer suitable for these materials.

§ 1. INTRODUCTION

Unless a material is either liquid or a good conductor of heat, the determination of its specific heat presents considerable difficulty. The methods ordinarily employed fall into two classes, the method of mixtures and the electrical method. In the former, the body is heated and transferred to a calorimeter containing a liquid of known specific heat. The quantity of energy involved in changing its temperature is estimated from the rise in temperature of the calorimetric fluid, when equilibrium is attained. In the electrical method, energy is generated electrically, its quantity being measured, and the apparatus is so arranged that the energy all passes into the body under examination; the specific heat is deduced from the rise of temperature.

In either method it is essential that initially and finally the body shall be in a state of uniform temperature, and when the body is a poor heat conductor the equalisation of temperature may take a considerable time. During this period, of course, the body or the calorimeter is losing heat at a rate depending on its temperature excess above its surroundings. It is usual to correct for the error thus introduced, either by raising the temperature of the surroundings so that this excess is always zero, or else by estimating the heat loss as a function of temperature excess, by separate experiments. The importance of this correction increases rapidly when the correction itself becomes an appreciable fraction of the total heat interchange, as it necessarily does when the time for equalisation of temperature is prolonged.

§ 2. DESCRIPTION OF APPARATUS

The apparatus to be described is suitable for powdered materials. It utilises the electrical method, and is designed so that uniformity of temperature in the material may be obtained by thorough mixing with a special form of stirrer capable of dealing with powders.

An axle passes down the centre of the calorimeter, and blades of helicoidal form are attached to this axis by a stirrup free to swing about a horizontal axis. Hence,

as the stirrer is rotated it picks up the material and throws it over, somewhat after the manner of a ploughshare, see Fig. 1. The stirrer is carried on ball-bearings so as to minimise heat production by friction, and by means of a wooden platform attached to the axis is slowly rotated. This platform is utilised also as a stand for a vacuum flask. The mean temperature of the material is obtained by means of a number of thermo-couples in series, of which the junctions are attached to the blades of the stirrer, but insulated from it by means of very small glass tubes. The

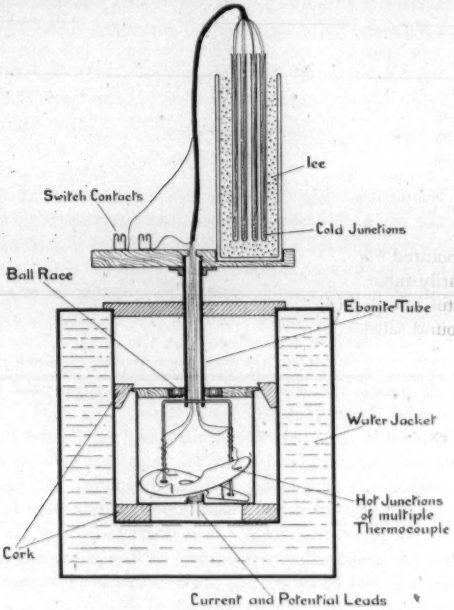

Fig. 1.

thermo-couples thus rotate with the stirrer; by the bringing of the cold junctions to the vacuum flask mentioned above, the need for elaborate contacts between rotating and stationary parts is avoided. When temperature readings are required, contact is made to the instrument (e.g. a potentiometer) used for measuring the e.m.f. by the slipping of two knife-blade contacts into the switch parts attached to the platform as shown.

Heat is generated in the interior of the mass of material by means of the heater seen just below the stirrer in Fig. 1. It is located in a re-entrant tube so that all the heat generated reaches the material under examination. Potential leads are

taken from the coil at the point of emergence from the tube, so that heat generated in the leads is not included in the quantity measured.

To minimise the heat loss from the calorimeter, the latter is closed by a wooden lid, with metal foil on the under surface to act as a radiation shield. The calorimeter is placed upon a slab of cork inside a larger double-walled vessel used as a constant-temperature water jacket. The calorimeter is prevented from rotating by two wedge-shaped pieces of cork bearing against the projecting edge of the calorimeter lid; the water jacket is itself closed by an outer cork lid. The thermal capacity of the apparatus is determined by means of a blank experiment, in which a measured amount of heat is generated with water as calorimetric fluid, and the correction for radiation losses is obtained by observation of the rate of cooling with the energy switched off. In the apparatus as we have constructed it, the water equivalent of the calorimeter and accessories is 74 gm. and it holds 800 gm. of material of density about 2 and specific heat 0·2. Successive tests with different weights of a given material give results in excellent agreement, and it has also been verified that the temperature-rise, after correction for losses, is unaltered if the energy supply be cut down so as to occupy three times as long in importing the same amount of energy.

It may be pointed out that the kindred apparatus previously described by one of us* is primarily intended for light powdered materials, such as cork dust, and that the apparatus now described is not intended to take its place, but was designed, and has been found satisfactory, for heavier powdered materials, such as dry clay.

§ 3. ACKNOWLEDGMENT

We wish to express our thanks to Mr A. R. Challoner, observer in the Physics Department, for assistance both in the design and in the construction of the apparatus.

DISCUSSION

Dr Allan Ferguson. It would be useful if Mr Awbery would give us some information concerning the accuracy aimed at in these interesting experiments. Principal E. H. Griffiths, in his specific heat work, advocated such vigorous stirring that the stirring itself caused a measurable rise of temperature which was duly allowed for. Does Mr Awbery advocate agitation of this vigorous type in dealing with powders, or is he able to obtain consistent and accurate results with more gentle stirring?

Prof. F. L. Hopwood enquired how fine were the powders investigated. Very fine powder, like flour, would be difficult to stir.

The President enquired whether the readings of the thermo-couples employed differed to an extent which might indicate inadequate stirring.

* *Proc. Phys. Soc.* **33**, 355 (1921).

AUTHOR'S REPLY. The apparatus is only designed for an accuracy of about 2%, speed of operation being an element of the design. The procedure advocated by Principal Griffiths is, however, desirable when liquids are to be used and the highest accuracy is desired. In reply to Prof. Hopwood: the apparatus would be capable of dealing with flour in a dry condition, although actually it was used for rather coarser materials. The point raised by the President is an important one, which we had not overlooked; a special examination showed that the agreement between the individual thermo-couples was very satisfactory.

HEAVISIDE'S OPERATIONAL METHOD

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ABSTRACT. The author describes a method of treatment of Heaviside's operational calculus which involves unrestricted use of complex integration as employed by Bromwich, and concludes that the theory of integration provides a complete explanation of all the aspects of Heaviside's method of solving differential equations.

§ 1. INTRODUCTION

It is proposed to show that it is possible to obtain a complete explanation of Heaviside's method of solving differential equations, by a method which incorporates the views of the late T. J. I'A. Bromwich with those of J. R. Carson. The author's method is, to the best of his belief, identical with that advocated by Van der Pol*. In contrast with the procedure recently recommended by W. E. Sumpner† at a meeting of the Physical Society, it avoids the uncertainties that arise from the use of 'operators.' For compactness of exposition, the fundamental definitions and theorems will first be stated.

§ 2. DEFINITIONS AND THEOREMS

When the function $F(p)$ of the complex variable p is derived from a function $f(t)$ of the real positive variable t (where $f(t)$ is zero for negative values of t) by the transformation

$$F(p) = \int_0^\infty e^{-pt} f(t)\, dt \qquad \ldots\ldots(1),$$

the function $F(p)$ is called the *convert* of $f(t)$, and the process whereby $F(p)$ is obtained from $f(t)$, *conversion*. In general, conversion will be indicated by changing the small letter denoting a function of t to the corresponding capital letter, which then denotes a function of p. An important property of converts is enunciated by Borel's‡ theorem. Let $f(t)$ be connected with $f_1(t)$ and $f_2(t)$ by the relation

$$f(t) = \int_0^t f_1(t-\tau) f_2(\tau)\, d\tau \qquad \ldots\ldots(2).$$

* "A Simple Proof and an Extension of Heaviside's Operational Calculus for Invariable Systems," *Phil. Mag.*, **7**, 1153, (1929).

† "Heaviside's Fractional Differentiator," *Proc. Phys. Soc.*, **41**, 404 (1929).

‡ Borel, *Leçons sur les Séries Divergentes*, p. 104 (1901).

Then if $F(p)$, $F_1(p)$ and $F_2(p)$ be the converts respectively of $f(t)$, $f_1(t)$ and $f_2(t)$, Borel's theorem states that

$$F(p) = F_1(p)\,F_2(p) \qquad \ldots\ldots(3).$$

When a function $f(t)$ of the real positive variable t is derived from a function $F(p)$ of the complex variable p by the transformation

$$f(t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} F(p)\,e^{pt}\,dp \qquad \ldots\ldots(4),$$

the function $f(t)$ is called the *revert* of $F(p)$, and the process whereby $f(t)$ is obtained from $F(p)$, *reversion*. Reversion is thus defined by an integral of the type used by Bromwich. The integration is along a straight line, the constant c is positive, and arbitrary so long as all the singularities of $F(p)$ are to the left of the line of integration.

With this nomenclature, the statement of the celebrated Mellin-Fourier theorem* (already implied by the notation) is that, with suitable restrictions on the function concerned, the revert of the convert of $f(t)$ is the function $f(t)$ itself, or in terms of integrals, if

$$F(p) = \int_0^\infty e^{-p\tau} f(\tau)\,d\tau,$$

then

$$f(t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} F(p)\,e^{pt}\,dp \qquad \ldots\ldots(5).$$

In the application of the theorems to the solution of differential equations, it has further to be noticed that the convert $F_n(p)$, of the nth differential coefficient, $f_n(t)$, of $f(t)$ is obtained by integration by parts, thus

$$F_n(p) = p^n F(p) - p^{n-1} f(0) - p^{n-2} f_1(0) - \ldots - p f_{n-2}(0) - f_{n-1}(0) \ldots\ldots(6).$$

Finally, when $f(t)$ involves other variables than t the convert of a differential coefficient with respect to one of these other variables is to be obtained by the rule of differentiation under the sign of integration. Thus the convert of df/dx is, in general, dF/dx.

§ 3. COMPARISON WITH HEAVISIDE'S CONVENTIONS

Heaviside's operational method for the solution of differential equations consists, essentially, of three steps: (i) transformation to the operational form; (ii) solution of the operational equation; (iii) "algebrisation" of the operational form of the solution. In the present method, these three steps are represented by (i) conversion; (ii) solution of the converted equation; (iii) reversion.

The definitions used in the present method involve a notation slightly different from that of Heaviside, who considers, for instance, p^{-n} as operating on the unit function of t (which is equal to unity when $t > 0$, and zero when $t < 0$). Thus he writes

$$p^{-n} = t^n/\Gamma(n+1).$$

In the notation proposed here, p^{-n} is the convert of $t^{n-1}/\Gamma(n)$, and such an expression as $p^{-n}t^m$ is meaningless.

* Bromwich, "Normal Coordinates in Dynamical Systems," *Proc. L.M.S.* **2-15**, 412 (1916).

The key to the correctness of Heaviside's method appears to be undoubtedly the Mellin-Fourier theorem, equation (5), which shows that if the required solution of a differential equation be sufficiently regular in its behaviour, then the Bromwich-Heaviside method will, certainly, find this solution, and no other.

To illustrate this comparison by a well-known particular case, consider the equation of diffusion

$$\frac{d^2y}{dx^2} = \frac{dy}{dt}$$

where $y = 0$ when $t = 0$, $y = y_0$ when $x = 0$, and $y \to 0$ when $x \to \infty$. Here y_0 may, of course, be a function of t.

Heaviside's method consists in writing p for $\frac{d}{dt}$ so that

$$\frac{d^2y}{dx^2} = py$$

and accordingly $$y = y_0 \exp(-p^{\frac{1}{2}}x).$$

This equation is then interpreted as defining y as a function of the two variables x and t by Heaviside's rules of "algebrization."

In the present method Y and Y_0 are supposed to be the converts respectively of y and y_0 so that, by (6) the equation for Y is

$$\frac{d^2Y}{dx^2} = pY$$

with the condition $Y = Y_0$ when $x = 0$.

Thus $$Y = Y_0 \exp(-p^{\frac{1}{2}}x).$$

Then, since by definition Y is the convert of y, the solution of the equation is given by an integral of Bromwich's type.

$$y = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} Y_0 \exp(-p^{\frac{1}{2}}x + pt)\, dp.$$

To interpret Heaviside's method it is only necessary to consider the function dealt with in the operational form of the equation to be the convert of that in the original equation. When the operational form of the equation has been solved, a final reversion completes the explicit solution of the original equation.

§ 4. EXAMPLES

On p. 156 of Carson's book* two integral equations are quoted, Laplace's equation

$$F(p) = \int_0^\infty e^{-pt} f(t)\, dt,$$

where $f(t)$ is the function to be found, and Poisson's equation

$$\phi(t) = f(t) + \int_0^t \phi(\tau)\, k(t-\tau)\, d\tau,$$

where $\phi(t)$ is the function to be found.

* *Electric Circuit Theory and the Operational Calculus* (1926).

Concerning Laplace's equation, Carson says, "Little work has been done on Laplace's equation from the standpoint of pure analysis." The general solution of this equation is, however, known, for the function $f(t)$ is found by reversion (4) according to the Mellin-Fourier theorem (5). When, owing to peculiarities in the structure of $F(p)$, this solution is inapplicable, integrals of $f(t)$ may be found by reverting $p^{-1}F(p)$ or $G(p)F(p)$, where $G(p)$ is suitably chosen. The discussion of these cases would follow the orthodox lines. From the physical point of view, the theory of this equation may be regarded as complete; the difficulties are connected only with numerical work and analytical abnormalities.

Poisson's equation is solved by conversion, which gives, by Borel's theorem (3), and in accordance with our conventions

$$\Phi(p) = F(p) + \Phi(p)\,K(p),$$

whence

$$\Phi(p) = F(p)/\{1 - K(p)\},$$

and the solution is, accordingly

$$\phi(t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} \frac{F(p)\,e^{pt}\,dp}{1 - K(p)}$$

when the integrand behaves suitably.

The solutions of these equations as complex integrals may be regarded as the most satisfactory theoretical ones, as complex integrals can generally be more easily and variously manipulated than any other mathematical form. Poisson's equation, when modified by the introduction of differentiations is, of course, amenable to solution by the same methods. These remarks are only supplementary to the general subject of Heaviside's method; for equations of Poisson's type are frequently solved incidentally when the effects of terminal conditions (such as networks attached to the sending or receiving end of a cable) are found without the corresponding integral equations being explicitly stated.

As a final example, consider the equation

$$\frac{d^2y}{dx^2} = \frac{d^2y}{dt^2} + \frac{2}{\Gamma(a)}\frac{d}{dt}\int_0^t (t-\tau)^{a-1}\,y(\tau)\,d\tau + \frac{1}{\Gamma(2a)}\int_0^t (t-\tau)^{2a-1}\,y(\tau)\,d\tau,$$

where $y = 0$ and $dy/dt = 0$ when $t = 0$ and a is positive.

Then, by (3) and (6), conversion gives

$$d^2Y/dx^2 = (p + p^{-a})^2 Y.$$

If also $y = f(t)$ when $x = 0$, and $y \to 0$ when $x \to 0$

$$Y = F(p)\exp\{-(p + p^{-a})\,x\},$$

and the solution of the equation under the prescribed conditions is by (5)

$$y = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} \exp\{-p^{-a}\,x + (t-x)\,p\}\,F(p)\,dp$$

$$= \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} F(p)\exp\{(t-x)\,p\}\,dp$$

$$+ \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} [\exp\{-p^{-a}\,x\} - 1]\,F(p)\exp\{p\,(t-x)\}\,dp.$$

Now write $$\xi(x, t) = \frac{1}{2\pi i}\int_{c-i\infty}^{c+i\infty} \{\exp(-p^{-a}x) - 1\}.\exp pt\, dp,$$

and then owing to the behaviour of the integrand at infinity the contour of integration can, when $t > 0$, be modified to be a loop about the negative half of the real axis, and when $t < 0$, $\xi(x, t) = 0$, thus

$$\xi(x, t) = \frac{1}{2\pi i}\int_{-\infty}^{0+} \exp(-p^{-a}x + pt)\, dp,$$

and the solution of the equation is

$$y = f(t - x) + \int_x^t \xi(x, t - \tau) f(\tau - x)\, d\tau$$

when $t > x$, and $y = 0$ when $t < x$.

The value of $\xi(x, t)$ is easily found by expansion in ascending powers of x to be given by

$$t\xi(x, t) = \sum_1^\infty \frac{(-1)^\nu (xt^a)^\nu}{\nu!\,\Gamma(a\nu)}.$$

These examples, and the theorems upon which the method of solving them is based, seem to show that avoidance of the use of complex integration involves assumptions comparable in magnitude with such theorems as the Mellin-Fourier theorem, and condemns the operational method to remaining permanently in an experimental state. The use of complex integration, does, on the contrary, encourage the belief that much of Heaviside's technique can be regarded as a consolidated gain to exact and methodical science.

DISCUSSION

Dr W. E. Sumpner (communicated). The author has given an admirably clear and concise exposition of the connexion between the two methods of Bromwich and of Carson. I desire to offer a tribute to the merits of the paper, but I do not consider that it contains anything to justify what appears to be the author's conclusion, indicated in the last paragraph, that operator methods must remain in an experimental state until justified by analysis such as that of Bromwich. My recent paper is mentioned and is apparently singled out for criticism from this point of view. It may be well to point out that it did not suggest any new method, or criticise any old one. Its aim was to offer, and to justify, a new interpretation of the operator used in Heaviside's method. The interpretation seems to be applicable to other methods, including those of Bromwich and of Carson. These methods establish, by a searching form of analysis, the equivalence of two functions of a quantity p,

$$f_1(p) = f_2(p).$$

The equivalence is much more than a numerical equality holding good for any number p whether simple or complex. It is proved true for any quantity p which satisfies the laws of algebraic combination, so that it is more correctly described by

the word identity. This being so, it can be used for any operator p which conforms to such laws, so that if we denote the operand by F we have

$$f_1(p)\,F = f_2(p)\,F.$$

If now $f_1(p)$ take a form like $(p+c)^{\frac{1}{2}}$ which is not directly workable as an operator, while $f_2(p)$ can be readily interpreted as such, it is legitimate to use the second form in place of the first.

The assumptions made are two in number; p must satisfy the ordinary algebraic laws in the analysis used for establishing the equality, and it must be used in a way consistent with this assumption when the result of the analysis is being interpreted.

Now so far as the result is concerned it does not matter whether p is considered to be Boole's differentiator, or what I have called the index operator defined by the equation

$$p^{\alpha}t^{\beta}/\beta! = t^{\beta-\alpha}/(\beta-\alpha)!$$

since, if we take a typical result such as

$$p^2t + p^{-3}t^2 + pt^{-1},$$

it will readily be seen that in the two cases we arrive at the same expression in terms of t.

But if we want to prove that powers of p form commutative operators in association with any operand, we find the proof quite easy when p is the index operator, but impossible if p is Boole's differentiator. It has frequently been pointed out in papers dealing with Heaviside's methods that p and p^{-1} are not commutative. If we consider the quantities

$$p^2\,.\,(p^{-3}t),\ p^{-1}\,.\,t,\ \text{and}\ p^{-3}\,.\,(p^2t),$$

the first two become $t^2/2!$, while the last becomes zero if, as is always the practice in operator methods, arbitrary constants of integration are omitted. If p is the index operator all three quantities become $t^2/2!$.

This criticism raises no question about the accuracy of the analysis involved in the methods of Bromwich and of Carson, but it does affect the interpretation of the corresponding theorems when used with operators. The use of the index operator with Heaviside's methods does not affect these methods, but it removes much of the obscurity associated with them. In particular it makes it needless to explain the meaning of $p^{\frac{1}{2}}$ in terms of the calculus. The operator is so defined as to interpret $p^{\frac{1}{2}}$ without reference to the calculus, but on the other hand the use of the operator has to be justified. My paper did this for certain simple cases. I have since found that it does so in more complex examples.

I differ from the view taken by the author. I consider that Heaviside's method can be justified without any reliance either on the use of the calculus or on the use of complex integration.

The PRESIDENT thanked the author for his concise exposition. In the past the speaker had had occasion to work a good deal with Heaviside's method and he had been obliged to read some very laborious justifications of it, occupying in some

cases twenty or thirty pages. The proof given by the author, on the other hand, was a model of conciseness.

AUTHOR'S REPLY. The definition of p as an index operator, as recommended by Dr Sumpner, seems to depend upon the expansion of the operand in a power series, but this restriction is unnecessary when p is introduced by conversion, as advocated in the paper. The demonstration of the properties of the index operator by examination of particular cases does not appear to offer complete assurance that other important particular cases may not evade verification.

In my opinion Dr Sumpner exaggerates the significance of the fact that a function of p can be expressed in two ways, and my view may be illustrated by an example of the type quoted recently by Bromwich from p. 244 of the second volume of *Electromagnetic Theory*. Consider, for instance, a function $F(p)$ which is expansible in a series of descending powers of p for large values of p, and in a convergent series of ascending powers of p for small values of p. Then the index rules applied to the interpretation of $F(p)$ operating on the unit impulse function $H(t)$ give two series, one of which is an everywhere convergent series of ascending powers of t, and the other an asymptotic series of descending powers of t, which does not always fully represent the correct result. As Heaviside says, in p. 488 of the same volume, "it usually happens that the divergent result is not the full equivalent of the convergent result." We may take, as a particular example,

$$F(p) = p^{3/2}/(1 + p^2),$$

where, accordingly,

$$F(p) = p^{-1/2} - p^{-5/2} + p^{-9/2} + \ldots$$

when $|p| > 1$, and also

$$F(p) = p^{3/2} - p^{7/2} + p^{11/2} + \ldots$$

when $|p| < 1$. It is to be noticed that the two series represent precisely the same function, but in different regions; while they are certainly not operationally equivalent, when p is regarded as an index operator. The operational identity may be written in the paradoxical form,

$$\frac{p^{3/2}}{1+p^2} - \frac{1}{\sqrt{2}} + \frac{1}{\sqrt{2}}\cdot\frac{1-p}{1+p^2} = \frac{p^{3/2}}{1+p^2},$$

where the left-hand side must be expanded in descending powers of p, the right-hand side in ascending powers of p, and the operand is $H(t)$. The general rule, of course, for constructing such cases is to write down a function $F(p)$ which has both branch-points and poles. In the cases usually considered there is one branch-point at $p = 0$ and a pair of poles. The residues at the poles are responsible for the discrepancy between the asymptotic and convergent expansions obtained by the index rules. When complex integrals are used the question of commutation of operations does not arise, and I am inclined generally to regard the operational rules merely as a technical convenience. Finally, I thank both the President and Dr Sumpner for their courteous remarks.

THE J-PHENOMENON IN X-RAYS

By N. S. ALEXANDER, M.Sc., National Research Scholar, Auckland University College, New Zealand.

Communicated by Prof. P. W. Burbidge, October 19, 1929. *Read December* 13, 1929.

ABSTRACT. A series of experiments have been carried out with the view of repeating as far as possible the work of Barkla and others on the J-phenomenon. The results obtained in no case provide any evidence for this phenomenon, and, considered in conjunction with the work of Dunbar, Worsnop, and Gaertner, the experimental results suggest that the J-phenomenon has no real existence as an X-ray absorption effect.

§ 1. INTRODUCTION

In a number of papers, Barkla and co-workers have described an effect which was observed in experiments on X-radiation; this was at first considered to be due to a characteristic radiation of short wave-length, but is now attributed to a new absorption phenomenon—the so-called J-phenomenon.

The evidence for and against the existence of this characteristic radiation may be considered briefly. Barkla and others found:

(*a*) Scattered heterogeneous radiation to be softer than the primary radiation, the amount of softening varying with the primary wave-length, as would be expected were a characteristic radiation excited[1].

(*b*) Increased corpuscular radiation from aluminium at short wave-lengths, associated with J-absorption[1].

(*c*) Discontinuities in absorption in aluminium, carbon, oxygen, etc.[2].

These discontinuities were explained as a result of J-absorption. Williams[3], using a monochromatic radiation, found similar discontinuities for aluminium and copper. In many cases, however, his results appear to be open to a graphical interpretation other than that given, nor are they particularly concordant with regard to wave-length. Owen[4] found slight selective absorption of a narrow band of wave-lengths and, as a tentative suggestion, ascribes the results to J-absorption in silicon in the crystal used. His results have not been confirmed. Dauvillier[5] reported absorption-discontinuities for aluminium, and Khastgir and Watson[6] claimed spectroscopic evidence for a J-radiation.

The investigations of Richtmeyer[7], Hewlett[8], Hull and Rice[9], Olson, Dershem, and Storch[10], Allen[11] and Dunbar[12], have failed to show absorption-discontinuities in aluminium, copper, molybdenum, silver, water, carbon, oxygen, lithium, iron, and sulphur, at the predicted wave-lengths. Most of these observers employed spectrometric methods and used monochromatic radiation; that employed by Dunbar was heterogeneous. Richtmeyer also explains certain of Dauvillier's results. No trace of J-emission was found by Dauvillier for boron, or by

Duane and Shimizu[13] for aluminium. Dunbar explains the excess corpuscular radiation at short wave-lengths on Compton's theory. Siegbahn[14] and Nipper[15] both show that no spectroscopic evidence such as that claimed by Khastgir and Watson exists.

The weight of evidence is thus against the existence of a J-characteristic radiation, and Barkla now considers[16] that no direct evidence for such radiation exists.

§ 2. BARKLA'S LATER THEORY

Since the hypothesis of a new characteristic radiation could not be entertained, Barkla found it necessary, in order to explain experimental results, to postulate a new phenomenon in X-ray absorption. This phenomenon he has stated briefly thus: "A beam of X-rays in transmission through matter under certain critical

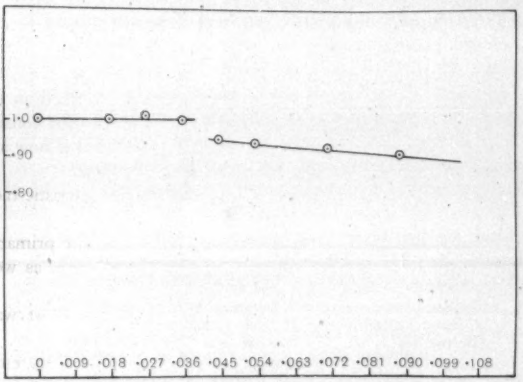


Fig. 1. Secondary and primary beams filtered simultaneously through aluminium sheets.

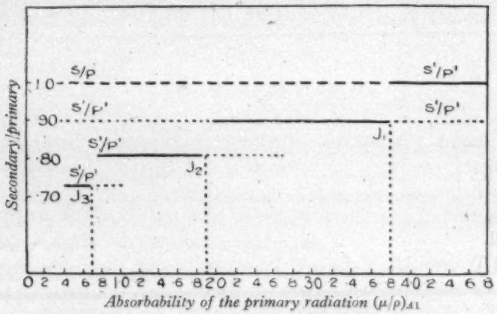
Soft tube: $\left(\frac{\mu}{\rho}\right)_{AL} = 3{\cdot}5$.

conditions becomes considerably more absorbable both in that and in other substances"[17]. "The transformation is not in the process of scattering, but in the transmission of scattered radiation through the radiating substance and through absorbers"[17]. "The phenomenon is, however, neither X-ray scattering nor X-ray fluorescence, as these terms have been understood"[16]. "After the J-transformation has taken place in a scattered X-radiation, and this has become a 'modified scattered' radiation when its absorbability is measured in certain substances, it has, even after passing through these substances, precisely the same absorbability as the primary when measured in certain other substances"[18].

This phenomenon comprises, in certain experiments, a softening of the scattered radiation and is thus similar to the effect explained on Compton's theory. In the

experiments on the J-phenomenon, however, Barkla finds that in no case does Compton's theory of scattering apply.

The experimental results may now be considered. Barkla and Mrs Sale[19] found in general the hardness of primary and scattered radiations to be identical, though at times the scattered radiation appeared softer. The latter was not, however, usually the case. Barkla and Khastgir[20] determined the change in ratio of intensities of scattered and primary radiations (as measured by their ionising-action) on absorption of each by equal thicknesses of aluminium. Discontinuities are observed and, as will be seen from Fig. 1, they occur in every case in the scattered radiation. These results are taken to show that the transformation into a softer radiation takes place outside the scattering substance. In 1925 Barkla[16] pointed



Broken line - - - - - - - is "unintercepted ratio" S/P.
Continuous line ——— is "intercepted ratio" S'/P'.
Dotted line ..... is occasional "intercepted ratio" S'/P'.

Fig. 2.

out additional characteristics of the phenomenon. The discontinuity is absolutely abrupt, even with heterogeneous beams, and appears to be more closely connected with absorption coefficient than with wave-length. The scattered radiation appears much more susceptible to transformation than the primary, and the appearance or non-appearance of the effect seems to be governed by unknown conditions. Barkla and Khastgir[20] observed discontinuities in absorption of scattered radiation. The ratio of the intensities of secondary and primary radiations, after absorption in equal thicknesses of aluminium, was found to remain constant, as the primary radiation was hardened, up to a certain critical point, when the ratio changed abruptly to another value, which again remained constant over a certain range. As many as five separate discontinuities of this nature have been observed.

Fig. 2 shows the nature of this effect. The primary radiation could be hardened either by filtering or by increasing the tube potential, but in each case the discontinuity appeared at the appropriate absorption coefficient. Similar experiments were described in November[21] 1925, when Barkla and Khastgir point out this

feature: If S and P be the intensities of scattered and primary radiations, and S' and P' the intensities after absorption in equal thin sheets of aluminium, the ratio $S/P : S'/P'$, that is $e^{-(\mu_1-\mu_2)x}$ where μ_1, μ_2 are the absorption coefficients of primary and scattered radiations, and x the thickness of absorber, is found to be accurately constant, and independent of wave-length.

Barkla and Miss Mackenzie[22] observed discontinuities in absorption of radiation scattered at an angle of 120° to the primary beam. Other experiments gave, instead of discontinuities, a continuous variation in the ratio of ionisations produced by beams scattered at 60° and 120°. Results are also quoted to show that the absorption of a complex beam is not the sum of the absorptions of its constituents. This is deduced from the fact that on superposing two radiations, each showing a J-discontinuity at a particular thickness of absorbing screen, instead of two discontinuities being observed, only one appears, and this at a thickness of absorber appropriate to the new complex radiation. Barkla and Khastgir[23] showed that a radiation which appeared to be modified when its absorption was measured in certain substances, appeared to be unmodified in others. Thus the J-phenomenon appears to depend in part on the material of the absorbing screen. Barkla and Miss Mackenzie[24] found the J-phenomenon to be dependent on the frequency of interruption of the primary current in the induction coil used to excite the X-radiation. The J-discontinuities in absorption appeared only over a limited range of frequencies, the critical frequency being dependent on the particular X-ray bulb employed.

Barkla and Watson[25], using a Coolidge tube, found that the position of the discontinuity, i.e. the absorption coefficient at which it occurred, depends on the current through the tube. They suggest that a possible explanation of this fact is to be found in the method used to measure the absorption coefficient. They also suggest that in cases where a continuous difference in absorbability of two radiations is found an explanation is to be sought in the fact in question.

Barkla[26] found that, with increasing thickness of scattering material, the scattered radiation became increasingly softened, the amount of softening ultimately reaching a limiting value. In other cases, however, with apparently the same experimental arrangement, the full amount of softening was produced by the thinnest sheets.

The chief experimental results summarised from the various papers appear to be as follows:

1. Softening of scattered radiation, such as would be expected were a characteristic radiation excited[1].

2. Increased corpuscular radiation at short wave-lengths[1].

3. Absorption discontinuities[2, 3, 4, 5].

4. Equality of hardness of scattered and primary radiation as a general rule, though at times a softening of the secondary radiation occurs[19].

5. Discontinuities in absorption of scattered radiation, the discontinuities appearing at definite values of absorption coefficient, independent of the method of attaining this hardness[20, 21, 22, 23, 24, 25].

6. Change of absorption coefficient at the discontinuity independent of wave-length[21].

7. Discontinuities in radiation scattered at an angle of 120°, with occasionally a continuous softening of this beam as compared with one scattered at 60°[22].

8. Absorption of a complex beam not the sum of the absorptions of its constituents[22].

9. Radiation modified in some substances but unmodified in others[23].

10. Dependence of the phenomenon on frequency of exciting impulses, and on current through the X-ray bulb[24, 25].

11. Dependence of the amount of softening of a scattered radiation on the mass per unit area of scattering material[26].

In the work published on this subject, several discrepancies may be observed. It is stated[17] that the changes are absolutely abrupt, even with heterogeneous beams. This effect is not shown in earlier experiments[1], nor is it of the type expected to be produced by the excitation of a characteristic radiation by a heterogeneous primary beam. It is also suggested as possible[27] that the absorption is an absorption of all the constituents of the radiation. This does not agree with the results obtained by Owen, where selective absorption is noted. It is remarkable also that when direct radiation is compared with that scattered at 90°, it is always the scattered radiation which shows the discontinuities, never the primary beam. Similarly, on comparing the radiations scattered at 60° and at 120°, it is found that in the latter only are discontinuities observed. This suggests that the phenomenon is dependent on the angle of scattering, though apparently that inference has not been investigated.

The work of other observers has in few cases afforded evidence for the J-phenomenon. Watson[28] considered that the effect might be due to some change in the absorbing material, and proposed to test this by means of a second beam from an independent source. Results were, however, not conclusive. Crowther[29] found that radiation scattered from aluminium, paraffin wax and paper was more absorbable than the primary radiation, but since he used heterogeneous beams his results may be completely explained by the Compton effect of change of wave-length on scattering, and of variation of scattering coefficient with wave-length.

Compton[30], using a balance method, found the scattered radiation to be appreciably softer than the primary, the amount of softening agreeing closely with that predicted by the quantum theory. Beatty[31] found X-radiation scattered from air to be softened, and Sadler and Mesham[32] observed a similar effect for a homogeneous radiation scattered from carbon. Dunbar[12] repeated much of Barkla's work but found no evidence for the J-phenomenon, though certain irregularities which appeared in his results have been interpreted as such by Barkla. Dunbar found heterogeneous scattered radiation to be softer than the primary by an amount agreeing closely with the results of Compton's theory.

Towards the completion of the present work, papers by Worsnop and by Gaertner appeared. Worsnop[19], using heterogeneous radiation from a Coolidge tube, excited by unrectified current from a transformer, investigated the variation

of the ratios S/P and S'/P' (defined as above) with increasing hardness of primary radiation, but found no evidence for the J-phenomenon. Gaertner[40], using a hot-cathode tube, excited by valve-rectified alternating current from a transformer, scattered the heterogeneous radiation from paraffin wax, and investigated the change in the ratio S/P with increasing thickness of filter. The results obtained afforded no evidence for the J-phenomenon.

There is thus a lack of agreement among the various observers. The J-phenomenon is as yet unexplained, and its apparently elusive nature hinders complete investigation. Also, in no case where the J-phenomenon is observed does Barkla admit that Compton's theory applies, though there is frequently a superficial resemblance. The Compton effect has, however, been supported by many experiments; by absorption measurements[30], spectroscopically by the use of both photographic[33, 34, 35, etc.] and ionisation[36] methods, and by observations on recoil electrons[37, 38, etc.]; it thus rests on a very firm experimental foundation. In view of these facts it was thought desirable to repeat as far as possible the experiments of Barkla and others, with the view of obtaining further information on the J-phenomenon, but on account of the fact that in none of the papers published in support of the effect has there been given a full description of experimental arrangements, it has been a matter of some difficulty to ensure repetition of previous work. It was hoped, however, that by varying experimental conditions as much as possible, repetition of the main characters of Barkla's work could be assured.

§ 3. EXPERIMENTAL INVESTIGATION

The principle of the method employed was briefly as follows. A heterogeneous radiation was scattered from thin sheets of material, in a direction at right angles to the primary beam. The intensities of the scattered and primary radiation were compared by the balancing in turn of the ionisation currents produced by them against the current produced by a separate beam ("standard beam") from the same source. The amount of ionisation produced by this separate beam could be varied by limitation of the width of the beam by means of an adjustable slit. The penetrating powers of the radiations could be compared by measurement of the absorption in aluminium. The balance method used had the advantage of eliminating the effect of variations in intensity of the source, while variations in quality were shown up very clearly, owing to differences in the method of measurement of the "standard" and scattered beams. Throughout the experiments it was found that the limit of accuracy was fixed by the variations in the source, the apparatus being extraordinarily sensitive to variations in the tube potential caused by fluctuations in current through the primary of the induction coil that was used as a source of high voltage. This current, which could be kept constant to within 2 per cent., was obtained from the city mains at 230 volts d.c. The voltage of the supply was subject to sudden variations of the order of 5 per cent. Owing to shortage of apparatus, an induction coil with mercury interrupter was the only available source of high voltage. The use of lead cells to supply primary current effected a considerable improvement, but only a limited supply of these was available. High-

voltages were measured by means of a gap between 10 cm. spheres in air. The bulb, mounted in a support which enabled it to be turned at any angle, was enclosed in a large lead-covered box, lead screens serving to eliminate the effect of stray radiation.

The adjustable slit in front of the "standard" ionisation chamber was of iron 1·2 cm. thick, and was fitted with scale and vernier reading to 0·01 mm. Calibration of the slit by a timing method showed that, up to the maximum opening used, namely 1 cm., intensities were exactly proportional to slit-widths. Fine adjustment

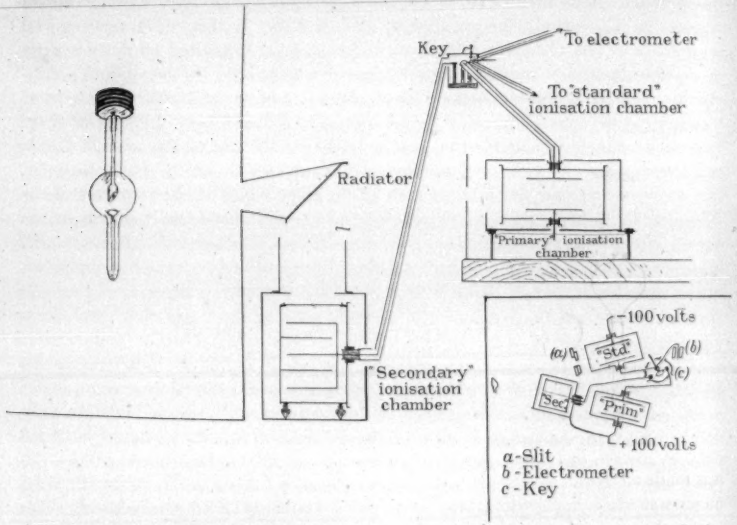


Fig. 3 (*a*). Electrical apparatus. Fig. 3 (*b*). Diagram of connections.

could be effected by means of a slow-motion screw. The "standard" ionisation chamber was of the two-electrode type, filled with air at atmospheric pressure. The beam of X-radiation passed through without impinging on the electrodes. The "primary" chamber was of exactly similar type during most of the experiments, but later was modified as described below. The area of aperture of this chamber was 2·5 cm.^{2}

The secondary chamber was of a different type in view of the fact that the radiation here was much less intense. The chamber was fitted with electrodes in the form of parallel plates placed normal to the direction of the radiation. The chamber was charged, and carried plates similar to those on the electrode and alternating with them, as shown in Fig. 3 (*a*).

These plates were made of aluminium foil, 0·005 cm. thick. The chamber was

filled with air saturated with methyl iodide vapour. The apparatus used to ensure saturation of the air was designed also to prevent changes in the composition of the mixture with temperature and pressure, Fig. 3 (*c*). By raising and lowering of the movable reservoirs in turn, air was circulated from the fixed reservoirs back and forth through drying tubes of calcium chloride and phosphorus pentoxide, and through methyl iodide. By suitable adjustment of the stopcocks, the gas in the chamber could be shut off from contact with the liquid, in order to avoid disturbances due to changing vapour pressure, and was placed under slight pressure, 0·5 cm. of mercury, to eliminate the effect of slight leaks in the chamber, which

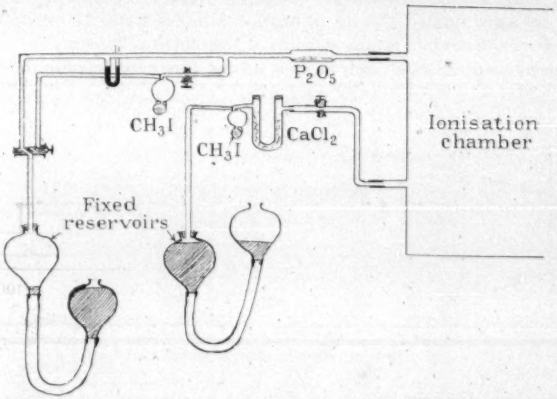


Fig. 3 (*c*). Apparatus for controlling composition of air-and-vapour mixture.

was made airtight with soft wax. To indicate the position of balance a string electrometer was used, normally with potentials of 100 volts on the knife-edges, the sensitivity being about 500 divisions per volt. Since a null method was adopted, the most sensitive portion of the scale could be used. Balance could be obtained within less than 1 per cent. of the variation in width of the adjustable slit.

All ionisation chambers were enclosed in lead boxes fitted with levelling screws, to facilitate alignment with the beams. Wherever necessary, in order to avoid the effects of characteristic L-radiation from the lead, edges of slits, etc., were covered with a sufficient thickness, 0·7 mm., of aluminium.

The scattering material, in the form of thin sheets, was placed so as to bisect the angle between the incident primary and the scattered beam, as shown in Fig. 3 (*a*). This arrangement ensured approximately equal absorption of primary and scattered beams in the scattering material. Care was taken that no radiation, except that scattered from the desired material, should enter the ionisation chamber. Experiment showed that this stray radiation agreed in intensity and in quality with the radiation which would be scattered from the air in the vicinity of the

radiator. The effect of polarisation of the primary beam was found by experiment to be small, and in any case was eliminated by arranging the direction of the cathode stream to make an angle of 45° with the direction of scattering.

§ 4. RESULTS

1. The primary radiation being kept constant, the same absorbing screen was inserted in the path of the primary and scattered beams in turn, and the intensities were determined. This procedure was repeated with different thicknesses of absorber. Figs. 4, 5 and 6 show the results of typical experiments. The scattered radiation appeared harder than the primary, but this fact may be explained by the method of measurement. It was considered that these differences in methods of measurement would be most likely to show up any difference between the radiations

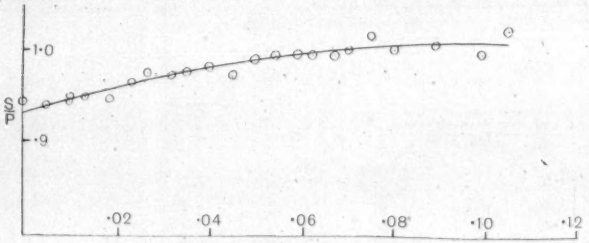


Fig. 4. Effect of filtering on S/P.

—differences which might be concealed by the balance method if the methods of measurement of the two beams were identical. Thus these experiments, while not calculated to measure differences in absorption coefficient, would certainly show discontinuities in absorption or sudden changes in the character of the radiation.

The experiments were repeated with primary radiations of various degrees of hardness, filtered by aluminium and by copper, unfiltered, from gas and Coolidge bulbs, with scatterers of aluminium, paper and paraffin wax; but in a series of more than forty sets of observations covering the range where the J-discontinuities have been observed, there did not once appear the slightest evidence for the J-phenomenon. On the contrary, curves similar to those in Figs. 4, 5 and 6 were obtained in every case. This result was independent of the frequency of the break and of the current through the tube.

Fig. 4 shows the effect of increasing filtering on the ratio of the ionisations produced by primary and scattered radiations, an unfiltered primary radiation of moderate hardness from a gas bulb being used. The mass absorption coefficient of the primary radiation varied with increasing thickness of filter from 2·4 to 1·8, thus including the position of the J_2 discontinuity ($\mu/\rho = 1{\cdot}9$). The ratio of ionisations increased uniformly throughout, the total variation being approximately

10 per cent. The points lie, within the experimental error of 2 per cent., on a smooth curve. Fig. 5 shows the results of a similar experiment, with a slightly harder primary radiation, the filtering being carried out to a greater extent. The same radiator, 0·35 cm. aluminium, was used, and the primary radiation was again

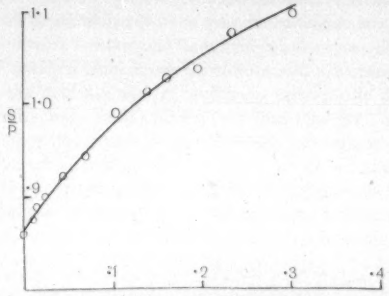


Fig. 5. Effect of filtering on S/P for harder radiation.

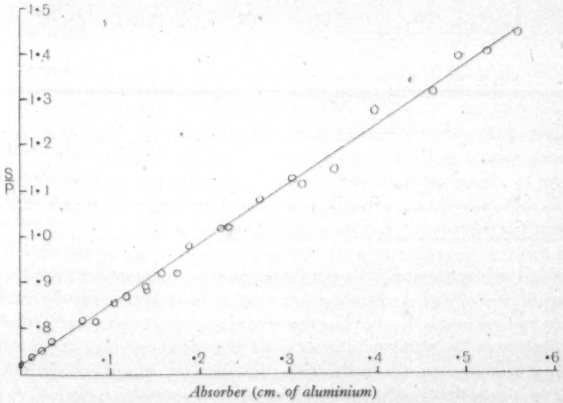


Fig. 6. Effect of filtering on S/P for still harder radiation.

unfiltered. The range of mass absorption coefficients was from 2·3 to 1·5, again including the position of the J_2 discontinuity. Again no trace of discontinuity appeared.

Fig. 6 shows the effect of filtering on a harder radiation, and the absorption coefficients vary from 1·4 to 0·5, covering the position of the J_2 discontinuity

($\mu/\rho = 0{\cdot}7$). The points lie very nearly on a straight line. Variations up to 4 per cent. occur, but no trace of a discontinuity is observed. Owing to the smaller intensity of radiation obtainable, the experimental error was here greater, and was probably of the order of 3 or 4 per cent.

The upward trend of these curves, showing the scattered radiation to be relatively harder, may be explained as a result of the differences in absorption of the components of a heterogeneous radiation. The scattered radiation was absorbed in methyl iodide vapour, the direct in air. Two possible explanations are available, and probably both effects were operative: (1) the selective absorption in iodine, causing the harder components of the radiation to be more effective in producing ionisation, and (2) the greater absorption of all softer components of the beam in the aluminium plates and in the methyl iodide vapour, giving a filtering effect.

Both these effects could be avoided by making the methods of absorption of the primary and scattered beams similar. The "primary" ionisation chamber was modified accordingly so as to be exactly similar to the "secondary" chamber. The

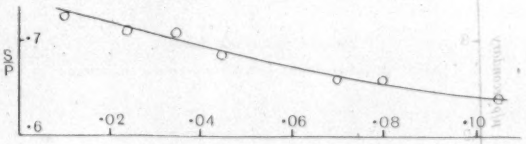


Fig. 7. Effect of absorption on S/P when the primary and secondary ionisation chambers are similar.

dimensions of the primary beam had now to be limited to those of pinholes in a lead screen, with a total area of 1·5 mm². Screens to eliminate lead L-radiation could not, of course, be employed in these circumstances. Experiments of a type similar to those already described now indicated that the secondary radiation was softer than the primary. Fig. 7 shows typical results.

In a series of experiments with this arrangement of apparatus there likewise appeared no evidence of a J-discontinuity, but, on the contrary, results show a softening of the scattered radiation agreeing at least approximately with those predicted by Compton's theory. The theoretical amount of softening on Compton's theory could not be calculated exactly since the radiation employed was heterogeneous, of unknown energy-distribution, unknown amount of polarisation, etc.

2. The absorption coefficients of primary and scattered radiation, calculated on an approximately 50 per cent. absorption, were determined for different degrees of hardness of the primary radiation. The amount of softening produced was found to be greater than that observed by Dunbar, but this fact may be explained by the variation in scattering coefficient with wave-length, since the radiations employed were unfiltered, and thus heterogeneous. This view is supported by the fact that when filtered radiations were employed the change in absorption coefficient was found to be lessened.

These results are shown in Fig. 8, where the absorption coefficient of the scattered radiation is plotted against that of the primary. In the lower curve filtered radiations were employed.

3. The variation in the amount of softening produced by different thicknesses of scattering material was determined. It was found that the change in absorption coefficient of the scattered radiation from the thinnest sheets was within 6 per cent. of the change when thick sheets were used. This result was verified for aluminium, paraffin wax, and paper; Fig. 9 shows a typical case.

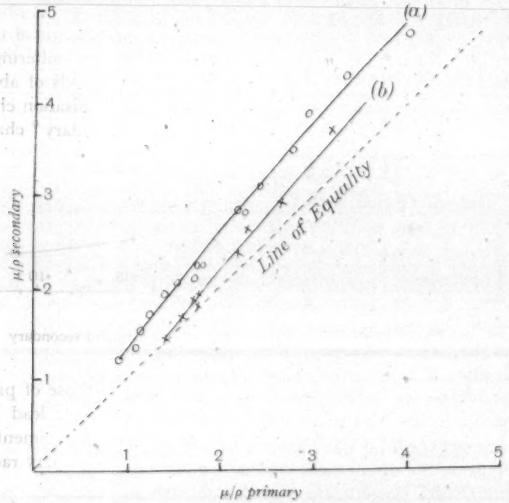


Fig. 8. μ/ρ for primary and secondary radiation (aluminium).
(*a*) Unfiltered radiation. (*b*) Filtered radiation.

Nine similar curves were obtained. Thus these experiments have failed to show the effect mentioned by Barkla in a letter to *Nature*[26], where he brings forward evidence that "modified scattered radiation is due to the superposition of unmodified scattered radiations." This effect is stated thus—"when the sheet of paper or paraffin wax was made gradually thinner, the difference between the primary and scattered radiations became smaller and ultimately almost vanished." "Plotting the change of absorbability against mass per unit area of the scattering sheet, we obtained curves exactly like the familiar ionisation-pressure curves showing saturation current."

The results obtained agree, however, with those obtained by Barkla, under apparently the same experimental conditions, when the radiation "settled down to

a state where thick sheets, thin sheets, and even air itself produced a scattered radiation showing the full amount of modification."

4. The ratios of the absorption coefficients for copper and aluminium were determined for radiations of different degrees of hardness. The results obtained agree with those of Dunbar, and show similarly no evidence of a J-discontinuity.

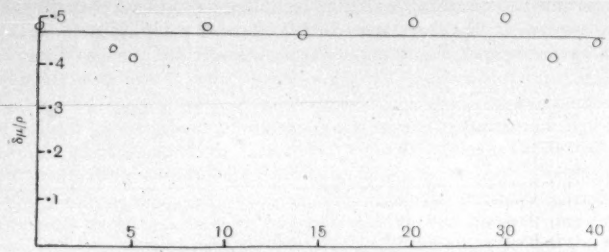


Fig. 9. Effect of thickness of paper on absorption coefficient.

§ 5. CONCLUSION

Thus in a wide range of experiments the results obtained show in no case the slightest evidence for the J-phenomenon, and confirm the work of Worsnop[39] and Gaertner[40]. The only explanation of these results is that under the conditions of experiment the factors necessary for the appearance of the J-phenomenon have not been operative. It is apparent, therefore, that these factors are not ones which commonly occur, but are restricted to very special experimental conditions. This view is supported by the fact that, as the work of Barkla shows, even when the conditions necessary for the J-phenomenon have been realised, a very slight change is sufficient to cause the disappearance of the effect.

On account of the elusive nature of the phenomenon, and the difficulty experienced in realising the necessary conditions, it is desirable that full details of the experimental arrangements used by Barkla and his co-workers should be published. The lack of detail in the accounts published renders the exact repetition of the experiments impossible, and, until this repetition is ensured, it is impossible to investigate more fully the nature of the phenomenon, or even to substantiate its existence.

§ 6. ACKNOWLEDGMENTS

Finally, I wish to express my thanks to Prof. P. W. Burbidge, under whose direction this research was carried out, for his generous advice and assistance; to the Department of Scientific and Industrial Research, for a scholarship which enabled this work to be carried out; to Dr N. McDougall for his donation of most of the X-ray plant, and to the staff of the Physics Department for assistance generally.

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DISCUSSION

Mr I. Backhurst. Mr Alexander mentions (1) that the accuracy of his investigation was limited by the sensitivity of his experimental arrangement to variations of tube potential, and (2) that variations in the quality of the radiation were shown up very clearly by the balance method of measurement employed. These two effects are directly connected and as, in my opinion, due attention has not always been given to them by investigators of the J-phenomenon, I should like to make some observations as to how they come about. The distribution curve of spectral intensity

for white radiation has a steep slope, as it rises from zero at the short wave-length limit and remains steep until it is comparatively near the wave-length of maximum intensity. The absorption coefficient in aluminium for any constituent wave-length λ is approximately proportional to λ^3, so that the greater the amount of aluminium placed in the path of the beam, the more is the transmitted radiation limited in the main to the short wave-lengths associated with the steep part of the spectral intensity curve. Clearly the intensity of these short wave-lengths is critically dependent on the position of the short wave-length limit and therefore on the potential applied to the X-ray tube.

White radiation, after transmission through a thickness x of aluminium, will have a percentage variation in intensity equal to K times the percentage variation of the voltage V applied to the X-ray tube, where K is a function of x and V which increases as x increases. If x is zero, K will commonly be about 2·5. If x is other than zero, K will usually have values up to about 10, and for heavily filtered beams may be 25 or more. When X-ray beams are compared in intensity by being balanced in turn against a beam of lower K value, effects (1) and (2), referred to above, are bound to appear, since two sensitive voltage indicators are being compared by means of a less sensitive one.

In conclusion I should like, if I may, to congratulate Mr Alexander on his success in obtaining results while hampered by difficulties such as arise from a fluctuating voltage supply: they are particularly inconvenient in a research of this kind.

ON THE DETERMINATION OF THE PARAMETERS IN AN EMPIRICAL FORMULA

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Communicated by J. H. Awbery, B.A., B.Sc., September 3, 1929.
Read by Mr Awbery and discussed December 13, 1929.

ABSTRACT. The writer reviews the method of least squares and gives reasons for recalling some cautions regarding its use that were mentioned by Stewart and Uhler several years ago. Examples are worked out to illustrate the discussion. The method recently mentioned by Awbery is compared with those of least squares and zero sum, from the practical point of view. Judging from the few examples given, the method of zero sum appears to be the quickest and its results to be well in accord with common-sense judgment. The amount of computation involved in the application of the various methods must be viewed in the light of the short cuts and elaborate calculating machines now available, since these may lighten the labour for one method more than for another. Attention is called to the fact that the method recently expounded by Awbery is the one given by Cauchy in 1847.

§ 1. INTRODUCTION

The subject of least squares and curve-fitting is so old that it is customary in this age to apologise for introducing the topic into the literature. Nevertheless the matter appears to be still of lively interest, for one may find a number of articles every year treating the subject. Papers of fundamental importance by Condon, Birge and Shea* have appeared within the past two years. These will be mentioned later. The recent publication of a paper by Awbery† suggests that there may be enough interest in the matter to justify the devotion of further space to it.

The primary purpose of this paper is to call to mind several facts regarding the method of least squares that seem often to be overlooked. The secondary purpose is to make two remarks concerning Mr Awbery's paper, and to make a few observations from a practical standpoint regarding the different methods, including Norman Campbell's zero sum method.

§ 2. COMMENTS ON THE THEORY OF LEAST SQUARES

The rule of least squares is based on two fundamental postulates: (1) the probability that an error will fall between two limits is a function of those limits; (2) in the case of several observations to determine a single quantity, the weighted average is the most probable value. These postulates are the mathematical equivalent of

* Condon, *University of California Publ. in Math.* **2**, No. 4 (1927); Birge and Shea, *ibid.* No. 5.
† *Proc. Phys. Soc.* **41**, 384 (1929).

the Gaussian law of errors. Just as a system of geometry stands or falls with its postulates, so does the Gaussian law of errors and the method of least squares stand or fall with the two postulates stated above. Their validity will not be discussed here, except to remark that their experimental basis has been vigorously questioned at times. To admit them is to admit that the method of least squares gives the best solution to the problem at hand. (The question whether the method is ever worth while is another matter and is to be kept out of the argument at present.) How, then, can one countenance the two postulates and yet make remarks to the effect that in curve-fitting the method of least squares gives undue weight to certain points, or that the curve determined thereby is obviously out of place? Such remarks would then be an acknowledgment that the method has been misused. One might not wonder at an occasional abuse of the method in the adjustment of a set of observations, because the rules given in most textbooks treating of it do not warn the reader against likely pitfalls—in fact all the textbooks that I can find lead one directly into them. But one might well show astonishment at the confusion of ideas regarding the method that has been presented in some papers devoted to the subject of curve-fitting, and especially at errors in textbooks themselves. The prevalence of erroneous ideas concerning least squares was pointed out in two papers that appeared several years ago*†.

The commonest misuse of the method of least squares arises from throwing all the adjustment on to one set of observed quantities and none on the others, or from throwing it on to the wrong set. The simplest case to consider is that of observations subject to linear condition equations—the case of the straight line, as it would be called in curve-fitting. Suppose there are n sets of observed points $(x_1, y_1), (x_2, y_2), \ldots, (x_n, y_n)$. Let w_{xi} and w_{yi} be the weights of x_i and y_i; u_i and v_i their most probable values; and let

$$v_i = mu_i + b \qquad (i = 1, 2, \ldots, n) \qquad \ldots\ldots(1)$$

be the linear conditions to be satisfied. The method of least squares demands that

$$\Phi \equiv \sum_{i-1}^{n} w_{xi}(x_i - u_i)^2 + \sum_{i-1}^{n} w_{yi}(y_i - v_i)^2 \qquad \ldots\ldots(2)$$

be a minimum. Φ is a function of the $2n + 2$ quantities u_i, v_i $(i = 1, 2, \ldots, n)$, m, and b. For Φ to be a minimum,

$$\left.\begin{aligned} \sum_{1}^{n} w_{xi}(x_i - u_i)\,du_i + \sum_{1}^{n} w_{yi}(y_i - v_i)\,dv_i = 0, &\quad 1 \text{ equation} \\ m\,du_i - dv_i + u_i\,dm + db = 0, &\quad n \text{ equations} \end{aligned}\right\}.$$

Using Lagrangian multipliers $\lambda_1, \lambda_2, \ldots, \lambda_n$, we get the $2n + 2$ equations

$$\left.\begin{aligned} w_{xi}(x_i - u_i) + m\lambda_i = 0, &\quad n \text{ equations} \quad a \\ w_{yi}(y_i - v_i) - \lambda_i = 0, &\quad n \text{ equations} \quad b \\ \Sigma\, u_i\lambda_i = 0, &\quad 1 \text{ equation} \quad c \\ \Sigma\, \lambda_i = 0, &\quad 1 \text{ equation} \quad d \end{aligned}\right\} \qquad \ldots\ldots(3),$$

* H. S. Uhler, *Journ. Opt. Soc.* **7**, 1043 (1923).
† R. Meldrum Stewart, *Phil. Mag.* **40**, 217 (1920).

which with the n equations (1) are sufficient to determine the $3n + 2$ unknowns, u_i, v_i, λ_i, m, and b.

The elimination of λ_i from (3 *a*) and (3 *b*) gives

$$\frac{y_i - v_i}{x_i - u_i} = -\frac{1}{m}\frac{w_{xi}}{w_{yi}} \equiv \tan \delta_i \qquad \ldots\ldots(4).$$

The angle δ_i as thus defined is the angle that the line joining (x_i, y_i) with (u_i, v_i) makes with the x axis. If $w_{xi} = w_{yi}$, then $\tan \delta_i = -1/m$ and this line is normal to $v = mu + b$, the curve given by the least-squares adjustment. This line is parallel to the x axis if $w_{xi}/w_{yi} = 0$, and is parallel to the y axis if $w_{yi}/w_{xi} = 0$. So, the n observation equations $mx_i + b = y_i$ and weights w_i $(i = 1, 2, \ldots, n)$ being given, the usual rule given in textbooks that leads to the normal equations

$$m\Sigma w_i x_i^2 + b\Sigma w_i x_i = \Sigma w_i x_i y_i,$$
$$m\Sigma w_i x_i + b\Sigma w_i = \Sigma w_i y_i$$

can be used only when the observation on each x_i is considered infallible compared with that on y_i. When $w_{yi}/w_{xi} \neq 0$ it is not surprising that the m and b thus determined give a line that when plotted looks out of place. A graphical method is preferable to rules called least-square, which are merely devices for reducing the number of equations to that equal to the number of unknowns.

The above analysis was made on a linear relation in two variables, but similar remarks hold for any function. More general cases have been discussed by Stewart* and by Uhler* and need not be repeated. The conclusion is that the method of least squares does give a unique solution to the problem, and an alternative proposed method should not be compared with it unless it has been properly applied.

Before leaving this part of the discussion it is well to consider the following question. Given a set of observations (x_i, y_i) $(i = 1, 2, \ldots, n)$, is there any practical difference between throwing all the adjustment on to the x_i and all on to the y_i? To state the question another way—practically does it make any difference what is chosen to be "residual" in a particular problem? The answer is in general, yes, but a definite reply can be given only for a specific problem. Often the difference is so great that some other method may give intermediate results. A good problem† to consider is: "Six points, supposed to lie on the arc of a circle, have the following measured coordinates:

x	3·15	2·67	1·80	1·07	−0·20	−1·84
y	2·49	3·72	4·69	5·33	5·98	6·25

Find the most probable coordinates of the centre and the most probable radius." This problem as stated cannot be solved by the method of least squares, because there is no information about the relative weights of the observations. But for our immediate purpose that is of no moment; we should disregard such information anyway and work the problem by defining "residual" in two ways. Two different answers cannot both arise from the method of least squares; it happens here that

* *Loc. cit.*

† Taken from Weld's *Theory of Errors and Least Squares*, p. 96 (Macmillan, 1916).

neither of them does. Definition (*a*): the "residual" is the square of the tangential (if the point lies outside) or principal chordal (if inside) distance from the point to the circle. Definition (*b*): the "residual" is the radial distance from the point to the circle.

For comparison, the observation equations were written

$$x_i^2 + 2gx_i + y_i^2 + 2fy_i + c = 0 \qquad (i = 1, 2, \ldots, 6),$$

and solved by the method of zero sum*. The results are shown in Table 1.

Table 1. Results obtained with definitions (*a*) and (*b*) of "residual," and by the method of zero sum.

Method	*x*-coordinate of centre	*y*-coordinate of centre	Radius
(*a*)	−1·899	0·957	5·292
(*b*)	−2·050	0·775	5·496
Zero sum	−2·062	0·726	5·551

The circle determined by (*a*) is clearly out of place, though of course it fulfils the requirement laid down for it. Those determined by (*b*) and by the method of zero sum are scarcely resolvable, and each is a good guess, as can be seen by graphing them. This is to be expected because if one were to estimate the "best" circle, one would naturally be most concerned about radial distances. It should be kept in mind that none of the circles were determined by the method of least squares.

§ 3. REMARKS CONCERNING MR AWBERY'S PAPER

The method proposed by Awbery† shows promise of giving reliable results with less labour than that of least squares. It is easily extended to equations having any number of parameters, and, by the application of Taylor's series, to transcendental functions. It cannot be called new, however, because it appears to be a special case of a general method for any number of parameters, first given by Cauchy‡ in 1847. It was tested in this laboratory with the view of putting it into use. Awbery gives an example (I refer to his Example 1) comprising 26 observations on the linear variation with temperature t of the resistance R of a coil, where $R = at + b$. He solves for the parameters a and b by the method of least squares, throwing all the adjustment on to R (a procedure which I assume to be justifiable),

* Norman Campbell, *Phil. Mag.* **39**, 177 (1920); **47**, 816 (1924); *Physics*, ch. 17 (Cambridge Univ. Press, 1920); *Measurements and Calculation*, ch. 10 (Longmans, 1928). Campbell pointed out in the discussion of Awbery's paper that this method appears to have been given first by Tobias Mayer in 1748, but that it was forgotten until after Campbell's paper in 1920. However, except for the rule given by Campbell to govern the grouping of the observations in order to make the method unique, this method seems to be identical with the so-called "method of averages," which is explained in most books on empirical curves.

† *Loc. cit.*

‡ Augustin Cauchy, *Comptes Rendus*, **25**, 650 (1847). Reprinted in his *Œuvres Complètes*, Série 1, **10**, 412 (Gauthier-Villars, Paris, 1897).

and by Cauchy's method. The disparity between the times required for the two methods, as he gives them, seemed a little too great, so I handed the same problem to a computer in this laboratory. She was thoroughly familiar with both methods, but, to make the comparison as fair as possible, we reviewed the procedures carefully and she worked another problem both ways before starting on this one. The results are given in Table 2. Results by the method of zero sum, obtained several days later by the same person under similar conditions, are included.

Table 2. Comparison of computations by various methods.

Method ...	Least squares		Cauchy		Zero sum
	(i)*	(ii)†	(i)*	(ii)†	(ii)†
Time required	3 hr.	16 min.	1 hr.	21 min.	8 min.
Value of a	0·181620	0·181625	0·181601	0·181601	0·181594
Value of b	46·3459	46·3459	46·3463	46·3463	46·3465
Value of $\Sigma (R_{obs} - R_{cal})^2 \times 10^8/26$	12·1	10·5	12·7	12·7	11·9

* (i) denotes Awbery's results. † (ii) denotes author's results.

The discrepancy between Awbery's and our results in the "least squares" column must arise because we carried more figures; it is certain that the mean square residual obtained by the least-squares method must be lower than that got by any other. The times given under (ii) do not include the additional five minutes required in each method to calculate R and the residuals and to add their squares.

Two conclusions may be drawn from this tabulation. First, if Cauchy's method is shorter in general than that of least squares, then Awbery picked out the wrong kind of example to illustrate the fact. Our experience has been that Cauchy's method requires less time for most problems than does that of least squares, but unfortunately in those examples where we have found it to be much shorter, its results differed so much from those got by the least-squares method that we never felt enough confidence in it to adopt it. Second, the zero-sum method here appears most practicable because of its rapidity and because it meets the least-squares method on its home ground 10·5 to 11·9 (though I know Dr Campbell would object to judging it on the latter basis).

In the problem of the circle considered previously the results by the (*b*) and zero-sum methods were practically identical, but the latter required only a fraction as much time, because in method (*b*) the equations had first to be reduced to linear form. The few examples chosen for this paper do not afford sufficient grounds for recommending any method, but they do show that of zero sum to be worthy of consideration.

One further comment of a practical nature may be worth making. The mention of least squares usually summons up visions of vast amounts of computation, often considered not worth the trouble they involve. It may be that now, with the latest types of calculating machines, the shortening of the arithmetic involved in the method of least squares is out of proportion to the shortening effected in other

methods. The least-squares adjustment of two variables, where the precision of one of them is so great that its error may be neglected in comparison with the error in the other, has been investigated by Condon*, who has explained how the computation in forming and solving the normal equations can be abridged considerably by making use of the orthogonal property of functions. Once tables for the application of Condon's scheme have been constructed for a particular type of problem, the saving in time is tremendous. The paper by Birge and Shea* illustrates what can be accomplished in this respect. By the use of Birge and Shea's tables for the type of problem to which they are applicable, the least-squares method becomes much shorter than any other yet proposed. Thus it is possible that our ideas regarding the amounts of labour necessary for the various methods may now need revision.

§ 4. ACKNOWLEDGMENT

In conclusion I wish to thank Miss Lola E. Shupe for making the calculations necessary in the preparation of this paper.

DISCUSSION

Mr J. H. Awbery (in presenting the paper). Dr Deming's results are summarised in his Table 2. I am surprised that the difference in times for the method of least squares should be so great, and suppose that it must be due to the use of Condon's method, with which I am not familiar. I repeated part of the calculation yesterday, and found that it took me 57 minutes to set up the normal equations, although without the checks that I would have applied if I had not been testing speed. Although I do not claim to be a computer, this still leaves a factor of something like 6; I did not know I was so bad. I am also surprised at the speed with which the calculated R can be obtained and the residuals formed, squared and added. Apart from the calculation, it is necessary to write down well over two hundred figures, and this would take me an appreciable portion of the five minutes allowed. However, on the times given, the conclusion seems to be that in my hands the method of Cauchy is quickest, and in Dr Deming's hands the method of least squares wins. I have no experience of the zero-sum method.

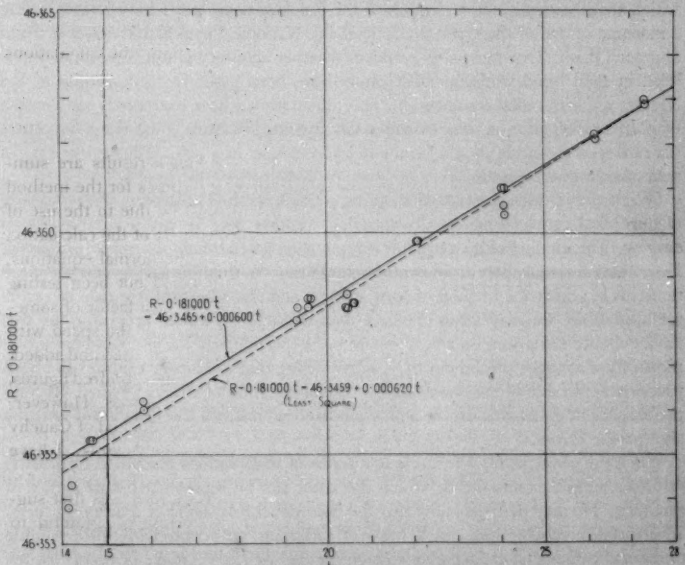
Dr Deming points out that the method brought forward by me† was first suggested by Cauchy. I am interested to find that this is so and am grateful to Dr Deming for pointing it out.

J. A. Hall. Having a certain interest in the resistance measurements which Mr Awbery used for his calculations, I have had the curiosity to repeat some of the work with the view of discovering the cause of the discrepancies between his and Dr Deming's values for the least-squares equation. My calculation, retaining all the figures that came, gave a value of 0·181621 for a. I have not had time, however, to check this calculation. I also investigated the difference between the values of 12·1 and 10·5 for the sum of the squares of the residuals. The value of 12·1 appears to

* *Loc. cit.* † *Proc. Phys. Soc.* **41**, 384 (1929).

have been taken from the values of the residuals in Mr Awbery's paper, and these residuals were only given to one significant figure. The extension to three figures in Σr^2 cannot, therefore, be justified. However, to clear up the difference, I re-calculated the residuals to three significant figures (though the additional figures have no experimental significance), using both Mr Awbery's and Dr Deming's least-squares equations. I obtained a value of 11·9 in the case of Mr Awbery's equation, and 11·8 in that of Dr Deming's equation. These calculations I have had an opportunity of checking, and I have not been able to find any error.

Mr A. F. Dufton. On p. 100 Dr Deming states that the circle determined by (*a*) is clearly out of place. This is by no means obvious to me, for the mean deviation of the points is only 0·019 compared with 0·040 for the circle found by the method



Graph illustrating Mr Dufton's remarks.

of zero sum. If the points are plotted on one-inch coordinate paper, the position of the centre can be estimated with the aid of ruler and compasses. I find it to be (− 1·95, 0·90). From the circle with this centre and of radius 5·36 the mean deviation of the points is 0·020. On p. 101 Dr Deming compares mean-square residuals. The mean of the actual deviations would appear to afford a useful comparison. The mean deviation of *R*, both from the "least square" line and from the "Cauchy"

line, is $3{\cdot}0 \times 10^{-4}$. Computing $(R - 0{\cdot}1810000t)$ and expressing it as a linear function of t by the graphical method which I recently described*, I find $R = 46{\cdot}3465 + 0{\cdot}1816001t$, with a mean deviation of $2{\cdot}4 \times 10^{-4}$ (see the accompanying graph).

Mr T. Smith. Dr Deming's paper contains a number of assertions which appear to need the addition of important restrictions not admitted by him. In the second section it is claimed that two postulates of a very broad character lead inevitably to the method of least squares. It would be interesting to see how the latter is deduced from the former without the introduction of any additional condition, as, for instance, a limitation of "function." My expectation is that the task is impossible.

In his criticism of Mr Awbery's method Dr Deming gives times of operation which are remarkably short. I doubt if the quickest computer I have known, using a machine of one of the types available at the National Physical Laboratory, could approach them. One inevitably wonders whether accuracy is not sometimes sacrificed in such rapid working. That errors have been made in the example of the circular arc with radial residuals (the only illustration I have examined) can be seen by plotting the points on squared paper and drawing the circle. With the given centre the radius of the circle should be about 5·509 instead of 5·496. [Illustrative graphs were shown at the meeting.]

We should, I think, beware of adopting a single method of dealing with a variety of numerical problems without considering whether it is the most suitable one to employ. The method of least squares is often used when others should be preferred. Discrimination must rest on an experimental basis. Without implying that there are no fields in which the method of least squares and the Gaussian doctrine of errors are applicable, we may stress the fact that deductions which according to this theory should have been on a very secure basis (particularly the probable error of a mean) have at times turned out to be so far wrong that the theory cannot have been properly applicable to the observations. The application of a theory which assumes the absence of systematic errors is only too likely to mislead when we know nothing about these errors.

When the physicist fits a formula to a series of observations his aim is frequently to find a simple expression by which the latter can be represented with sufficient accuracy. He may perhaps know that the theoretical relation is of a different form. The formula enables him, on a basis of sampling, to obtain with little trouble numerical values that are accurate enough for the use he will make of them. In the absence of a suitable formula or graph he would have to tabulate observations at close intervals and use them in the same way as mathematical tables are used. The error should nowhere exceed a certain value, just as the error in tables should nowhere exceed one-half of the last figure recorded. A mathematical table with a few errors in the penultimate place would unquestionably be bad compared with one in which numerous errors not exceeding one unit in the last place were present,

* *Nature*, **121**, 866 (1928).

although by the criterion of least squares the former might appear much the better of the two. On such grounds we can see that a method of equalising the departures of the curve from the observations will often be preferable to fitting by the method of least squares. The procedure will then be to fit two parallel curves, lying as close together as possible, between which all the observations lie, and to adopt as the solution the curve lying evenly between them. No questions of weight or probability (which are usually guesswork) are involved. Applying this method to the problem of the arc, I find the centre at about $x = -1{\cdot}8597$, $y = 0{\cdot}9301$, with radius $5{\cdot}2835$. The radial departures, together with those obtained from Dr Deming's solution, and the concentric solution with increased radius already mentioned, are given in the following table:

Point no.	Equal departures	Deming	Modified Deming
1	− 0·0365	− 0·0205	− 0·0335
2	+ 0·0365	+ 0·0674	+ 0·0544
3	− 0·0365	− 0·0051	− 0·0181
4	+ 0·0026	+ 0·0251	+ 0·0121
5	+ 0·0322	+ 0·0280	+ 0·0150
6	+ 0·0365	− 0·0170	− 0·0300

The advantages of the equal departure method would be more apparent were a larger number of observations involved. In this example a comparatively small increase in the magnitude of the departures at points 1, 2 and 3 permits those at points 5 and 6 to be much reduced. This suggests that the Deming solution for least squares is incorrect in the position of the centre as well as in the radius.

Dr N. R. Campbell (communicated). Of course I agree whole-heartedly with most of what Dr Deming says, but—(1) The postulate of least squares is not merely that the error is "a function of those limits"; it is that the error is a *continuous* function of them. This is a vital distinction, which all the orthodox treatises ignore vital because the first postulate is probably true, the second most certainly false. (2) It is quite true that zero sum has little or no advantage over least squares to the skilled computer armed with all the weapons of her trade. But in such conditions the two methods are not alternative. If we are going to squeeze the last ounce out of our observations then there is no single "right method"; we must sit down first and inquire what is the best method for our particular problem; and if one method can be shown to be superior to another, then it would be criminal to reject it on the score of extra labour. The people who ought to use zero sum regularly are those who now never think of adopting any but the graphical method. If they would only realise that it is actually quicker to do the computation than to draw the straight line, everybody, except the manufacturers of squared paper, would benefit. (3) I wish that my name were not so persistently attached to zero sum; the impression arises thereby that it is merely one out of many fads. In my original paper I put it forward as the method that must inevitably suggest itself to anyone who had not lost his common sense by worship at the Gaussian shrine.

Mr J. H. AWBERY (in reply to the discussion). I was interested in Mr Smith's remarks, and glad to hear that he too would find difficulty in working at the speed of a professional computer.

He objects to Dr Deming's statement of the postulates from which the Gaussian law of errors may be derived, and considers that it would be necessary to state definitely the function of the limits concerned in postulate (1). Actually postulate (2) does much to restrict the possible forms of function in (1), although to obtain the normal law it is necessary to assume in addition that the function is an even one (so that negative and positive errors of the same magnitude are equally likely). The question is discussed in detail in Keynes' *Probability*, pp. 195 *et seq.* and in Jordan's *Statistique Mathématique*.

Mr Hall's contribution to the discussion concerns me particularly, in connection with the value of the mean-square residual. The question is purely one of arithmetic, and it appears that Dr Deming and his computer have made some slight slip. I had not previously checked the facts as given by Dr Deming, but I now find that Mr Hall is right. The mean-square residual by least squares is about 11·8, and not 10·5, if sufficient figures are carried.

Mr Dufton puts forward the claims of his method recently expounded in *Nature*. It may be of interest to point out that his method, applied to a single quantity, assumes that the median (i.e. the value which occupies the middle place when the observations are ranged in order of magnitude) is the most probable value. The corresponding law of error is $ke^{-k|x|}$. The claims of the median were recently put very powerfully by Edgeworth in *Phil. Mag.* **46**.

AUTHOR'S REPLY (communicated). Condon's method was not used. Probably the disparity in time arises in the amount of pencil work. Miss Shupe read the figures directly from the June 15 *Proceedings*, and the only thing she put on paper was the normal equations and their solution. Sums of products can be formed in the machine without recording each one. Ordinarily we use the scheme for checking described in Leland's *Practical Least Squares*, but for this work we used a less trustworthy check in the formation of the normal equations, to shorten the time (*vide infra*). With the method that we used for calculating the mean-square residuals the only numbers recorded are the residuals, and if only one significant figure is used it is necessary to write only 26 figures, not 200. The 26 residuals can easily be computed in five minutes.

Mr Hall is perfectly correct; the value 12·1 came directly from Mr Awbery's residuals, which were given to one figure only. Our 10·5 was derived in the same way, because we wished to follow Mr Awbery, though it is probably more common to calculate one figure beyond the observations. Also, Mr Hall's value of a is correct; the discrepancy that he mentions came from our careless throwing away of a few decimals almost at the very end of the solution of our normal equations. We find, however, that this does not affect our residuals, which were calculated to one figure, so our value 10·5 stands.

In regard to the time of computation, I should like to add that on January 17,

1930, Miss Shupe repeated the task of finding a and b by the method of least squares, to check Mr Hall's value, and her time was 14 minutes—2 minutes less than it was previously. For a problem like this, the time would be nearly doubled if the sum check mentioned above were included, but in a problem involving more parameters the time would not be increased in so great a ratio.

I should like to add to Mr Smith's remarks by saying that the method of least squares, unlike most other methods, professes to take account of the weight of each observation. It not only finds "most probable" values for the parameters, but it also adjusts the observations. Now unless weights can be assigned to the observations with some confidence, the solution by least squares cannot even be commenced, and some other method must be used.

I feel under great obligations to Mr Awbery for communicating my paper to the Society, and to the Editor for the opportunity of making these final remarks.

DISCUSSION ON PRACTICAL EXAMINATIONS IN PHYSICS

In which the Science Masters' Association participated.

A NEW TYPE OF PRACTICAL EXAMINATION, DESIGNED TO BE A FAIR COMPETITION

By L. F. RICHARDSON, D.Sc., F.R.S. and R. S. MAXWELL, M.A., B.Sc.

Paper circulated before the meeting.

ABSTRACT. This new type of examination in practical physics consists of a number of short experiments. Each candidate attempts every experiment. There are two advantages:—(i) It is a fair competition between candidates. (ii) For every candidate the tests can be scattered widely over the syllabus. The correlation coefficients between success in practical physics and successes in other tests have been found by means of this examination to be: for theoretical physics $0{\cdot}4_6 \pm 0{\cdot}0_9$, for pure mathematics $0{\cdot}1_9 \pm 0{\cdot}1_8$, for the General B.Sc. $0{\cdot}2_7 \pm 0{\cdot}1_5$, for practical teaching 0·0, for Spearman's g $0{\cdot}3_5$. The correlation of g with theoretical physics is much higher. The numbers following the $\pm$ sign are standard errors.

§ 1. GENERAL DESCRIPTION AND ADVANTAGES

The practical examinations in physics set by the University of London to external candidates consist of two experiments, each of 1½ hours for the Intermediate Science, of 3 hours for the General B.Sc., and of 6 hours for the Special B.Sc. The experiments are not the same for all candidates; out of about 16 kinds of experiments 2 are allotted more or less at random. This kind of examination is, as far as we know, typical of present practice in physics.

One of us (L.F.R.) having tried to mark college examinations of this type during 5 years, and having found it very difficult to put the men in order of merit, suggested in 1925 that the experiments should be briefer so that they could be made the same for all candidates, and more numerous so that they could be spread over the syllabus. Something of this sort has long been done elsewhere when candidates in medical or psychological examinations each in turn inspect the same museum specimens. We have shared about equally in the conversion of this idea into a regular practice for physics and in the discussion of the results of eight such examinations held at Westminster Training College, London, S.W. 1.

It will be convenient to have a name for the new type of examination; and we suggest pantopiric* from Greek πειράω to attempt and πᾶν all, because each student tries all the experiments. Locally we have called it "progressive games."

As an example let us suppose that 20 men are to be examined and that the pantopiric examination is to last 3 hours. Then 20 experiments (plus two spares lest any

* We are indebted for this name to our classical colleague Mr F. C. G. Langford.

apparatus become broken) must be devised, each lasting 1/20 of 3 hours, that is 9 minutes. The apparatus is arranged on the benches in the laboratory during the two previous days, and beside each set is fixed a card bearing conspicuously the number of the experiment and, in smaller writing, brief instructions to the candidate, amounting usually to between 5 and 40 words. Five minutes before the examination begins, the candidates are arranged in alphabetical sequence in a passage outside, and each is given a booklet of blank paper and is told the number of the experiment at which he is to begin. The doors are then opened. After nine minutes a bell is rung and an invigilator calls loudly "Move on to next larger number!" There is usually rather a muddle at first, as some candidates are so deep in their work that they do not hear the order; but after the first hour all goes smoothly. Every nine minutes the bell is rung, orders are scarcely necessary, and there is a general commotion lasting only about 30 seconds. The invigilators keep a watchful eye on apparatus to see that it has not moved out of adjustment, and in each nine minutes can find time if necessary to check one or two settings, say one on a spectrometer, one on an optical bench. Perhaps an apparatus is broken and its number has to be transferred to one of the spares. Apart from such an accident, at the end of three hours every candidate has attempted every experiment. The candidates usually look rather tired at the end.

Actually the examinations have been held, inconveniently, in two groups of rooms connected by a long passage. One of us invigilated in each portion, and at the sound of the bell two candidates hastened in opposite ways along the passage. Special labels were put to direct them.

Because all candidates have done the same experiments we have found it possible to put them in order of merit with a confidence unknown in the ordinary type of examination. Marking has occupied two examiners for perhaps 6 to 10 hours because the examiners have had to perform the experiments to find out (i) what is correct, (ii) what marks to give to various deviations from the correct value. We have usually assigned a maximum of 5, 7 or 10 marks to each question.

At this stage the reader may well be in doubt, as we were when we began, as to whether an experiment lasting for so short a time as nine minutes can have any interest or value as a test.

Psychologists have for a decade been using such short practical tests*. We have found it possible to arrange brief tests of this type so that they could be either easy for a student who has only taken Matriculation physics or difficult for a student who six months later took honours in the General B.Sc. Further, there is no indication that we have reached the limits of ease or of difficulty. The task has been to select tests:

(i) Of a difficulty such that one small fraction of the class will obtain full marks and another small fraction zero marks for each test, and

(ii) So as to cover the syllabus, or such portions of it as the students had studied.

* See, for instance, *Performance Tests of Intelligence*, by F. Gaw. (Industrial Fatigue Research Board, Report No. 31, 1925.)

As the devising of these tests seems to be the chief original part of this work, we set out fully in § 6 the instructions that were placed before the candidates. To describe the apparatus would take too long; we are compelled to leave this mostly to the imagination of the reader aided by brief notes in square brackets. These notes were not shown to candidates.

To show how these tests discriminate between different students, let us consider a few tests in detail.

Example 1.

Class. Second year after Intermediate, 22 men. Dec. 1925.

Time allowed for experiment, 8 minutes.

Instructions. "Adjust the spectrometer so as to show the two yellow lines of sodium separate, and show them to the examiner."

Apparatus. Hilger spectrometer, 60° prism about 4 cm. in edge, of square face, bunsen sodium flame, dark room.

Inspection. An invigilator inspected each student's work and afterwards put telescope and collimator out of focus and disarranged the prism. Such need for inspection and disarrangement has been avoided in 9 tests out of 10.

Failures and successes. Some candidates, flustered or unskilled, did not succeed in finding any spectral image. Others, more practised, concerned themselves with the minimum deviation. This was unnecessary, as they were not asked to measure angles. A few, having found an image, simply focussed it carefully by Schuster's method, when the lines were seen double.

Example 2.

Class. First year after Intermediate, 12 candidates. March 1928.

Time allowed for experiment, 15 minutes.

Instructions. "Find the cooling constant for the given ball, i.e.

$$\frac{\text{rate of cooling in degrees C. per minute}}{\text{excess temperature of ball over surrounding air in degrees C.}}.$$

Apparatus. Copper ball, about 6 cm. in diameter, suspended by a wire. Bunsen for heating it. Two thermometers, the bulb of one in a deep hole in the ball, the other in the air protected from the ball by a screen. A stop-clock.

Failures and successes. One candidate overheated and destroyed the thermometer. Several others observed temperatures every minute, worked differences and then averaged them so as unconsciously to eliminate all except the first and last readings. Several others used the well-known better arithmetic in which all readings have weight in the result.

Example 3.

Class. Second year after Intermediate, 13 candidates. December, 1927.

Time allowed for experiment, 13 minutes.

Instructions. "Measure the resistance and self-inductance of the given solenoid using the Campbell variable standard."

Apparatus. Heaviside equal-ratio bridge all ready connected with buzzer and telephone.

Failures and successes. This test is obviously too difficult for those who have not passed the Intermediate B.Sc. But it can easily be done in half the time allowed by anyone familiar with such measurements. Those who have only a slight acquaintance do not realise that there are two adjustments which must be made alternately many times in succession, until both are made right together. Also the doubling of the inductance is often forgotten. So far it is a test of knowledge rather than of dexterity; but an accurate setting depends on a sensitive ear.

Example 4.

Class. Intermediate, 21 candidates. December 1926.

Time allowed for experiment, 8 minutes.

Instructions. "Set the upper surface of the glass plate horizontal. Show your setting to the examiner."

Apparatus. Plate glass 25 cm. square resting on three levelling screws. Clinometer, Field, Mark III, purposely about 10′ out of adjustment.

Failures and successes. A few students were entirely bewildered. Common mistakes were (i) to assume the clinometer to be perfect, and (ii) to set the spirit level in a random azimuth.

The experiment could be performed in half the time allowed if the clinometer were first set parallel to the line joining two of the three feet and then turned in azimuth successively through 180°, 180°, 90°, 180°.

We have on one or two occasions given a class the choice between the pantopiric and the ordinary examination, and they have chosen the pantopiric, saying that it was fairer.

§ 2. THE METHOD OF MARKING

To describe in detail the method of marking each experiment, as is always done for standardised psychological tests*, would occupy too much space. But a brief survey of the actual system can be made by counting the frequency with which the several marks 0, 1, 2...10 have been given for work for which the greatest possible mark is 10. Table I shows this.

Table 1.

Mark	10	9	8	7	6	5	4	3	2	1	0
Frequency	136	108	110	93	103	98	115	114	100	78	383

At any one examination there are two purposes for which marks may be assigned, namely:

(i) To show whether the candidates have or have not attained a prearranged standard, say that set by the University of London. For this purpose it might be quite correct to give all the candidates no marks or all full marks.

* Terman, *The Measurement of Intelligence*.

(ii) To put the candidates in a succession of merit; and further, if the candidates are numerous, to choose a scale which distributes the marks according to some known law, say the Gaussian normal frequency distribution, as was done by K. Pearson in an investigation of the inheritance of ability*.

These two purposes (i) and (ii) tend to coalesce when the whole academic life of the country is considered over a decade. For the Universities do in fact choose standards which sort the candidates into a sequence of merit: 1st class honours, 2nd class honours, pass and fail.

§ 3. DIFFICULTIES AND OBJECTIONS

Large numbers of candidates. Our largest group numbered 36, and we feel that this number is almost the practical limit for a single circulation during 3 hours; for with it the time allowable for each experiment is only 5 minutes, which is inconveniently short.

On two occasions we duplicated each experiment and the candidates circulated in two self-contained "vortices." This plan could obviously be developed for dealing with large numbers of candidates when apparatus is abundant.

Heat experiments are, many of them, too slow to fit into 10 minutes. Nevertheless, we have found a few, to be seen in § 6. This is, however, one of the serious weaknesses of the pantopiric examination.

Correlation with ordinary examinations. To what extent is success in 10-minute experiments correlated with success in thinking out a chain of complicated experiments lasting all day, such as those set in the Special Physics examination of the University of London? We do not know. The reliable way of finding out would be to test, say, 30 students both ways, and to work out the correlation coefficient. It might be supposed that the written examination would provide sufficient opportunity for testing powers of persistent logical thought; but in view of Professor Spearman's† destructive criticism of arbitrarily assigned "faculties" we have not much confidence in this supposition.

Perseverators‡ may be at a disadvantage. This objection, made by Mr G. E. Whiting, deserves consideration.

§ 4. RELIABILITY

An examination would not be considered reliable if the same group of candidates, on being examined twice, came out in quite different sequences. The reliability of the pantopiric examination is measured by a "reliability coefficient" defined, according to the practice of psychologists§, to be the correlation coefficient between the marks obtained by the same candidates in two examinations, both of which are pantopiric, although differing in the detail of the questions set.

* K. Pearson. *J. Anthrop. Inst. G.B. and I.* **33**, 179 (1903).

† C. Spearman, *The Abilities of Man* (Macmillan and Co. 1927), chap. 3.

‡ *Ibid.* chap. 17.

§ G. M. Whipple, *Manual of Mental and Physical Tests*, 1, 51–53 (1914). W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, p. 132 (Cambridge University Press, 1925).

As the candidates' state of training might vary during an interval between two examinations, the latter are taken to be the tasks having odd and even numbers, all performed during the same 3 hours. Computing all coefficients by product moments, we obtain Table 2.

Table 2.

Date	Examination	No. in class n	(Half-exam.) r_1
1925 (Dec.)	Final II	22	0·74
1925 (Dec.)	Intermediate	16	0·47
1926 (Mar.)	Final I	12	0·34
1926 (Dec.)	Intermediate	21	0·72
1927 (Mar.)	Final I	14	0·43
1927 (Dec.)	Final II	13	0·17
1927 (Oct.)	Intermediate	36	0·45
1928 (Mar.)	Final I	12	0·59

The best way of forming the weighted mean of such correlations appears to be R. A. Fisher's, according to whom the mean* is

$$\tanh\left\{\frac{\Sigma(n-3)\left\{\tanh^{-1}r_1-\frac{r_1}{2(n-1)}\right\}}{\Sigma(n-3)}\right\}=R_1 \text{ say.}$$

Here R_1 comes to 0·52.

But a whole examination is more reliable than either half of it, in fact, according to a formula due to William Brown†, the reliability coefficient R for one whole examination is

$$R=\frac{2R_1}{1+R_1}=\frac{1\cdot04}{1\cdot52}=0\cdot68.$$

This is the reliability of a pantopiric examination lasting 3 hours. For pantopiric examinations lasting 6 and 12 hours the reliability coefficients may be computed by repeated application of Brown's formula, and come to 0·81, 0·89.

It is interesting to compare the above results with those obtained when the same students were re-examined pantopirically after an interval of about a year: see Table 3.

Table 3

Pair of examinations	No. of students	r
Intermediate, 1925 Dec. / Final I, 1927 Mar.	4	0·97
Final I, 1927 Mar. / Final II, 1927 Dec.	13	0·74
Intermediate, 1926 Dec. / Final I, 1928 Mar.	5	0·39

* R. A. Fisher, *Statistical Methods for Research Workers*, pp. 163–177 (Oliver and Boyd, 1928).
† Brown and Thomson, *loc. cit.*

Mean R, by Fisher's process aforesaid, is 0·73 and agrees well with 0·68 found above for simultaneous examinations of the same length, namely three hours.

What, we wonder, is the correlation between the marks in the first and second experiments of an examination in which different candidates have each two different tasks assigned to them, as in the ordinary University examinations? We have only one such for which the marks were kept separately, namely: Finals II, March 1925—10 candidates, $r_1 = 0{\cdot}474$, $2r_1/(1 + r_1) = 0{\cdot}64$.

This examination lasted 3 hours.

Reliability coefficients must be interpreted with caution, for they can be made large or small, for a given type of test, by choosing the group of examinees to have a large or a small range of the appropriate ability. To illustrate this, imagine a group composed of 50 navvies plus 50 physicists who had recently obtained first class honours. In successive pantopiric examinations the marks would tend to be nearly 0 per cent. for the navvies and nearly 100 per cent. for the physicists on each occasion. Consequently the reliability coefficient, computed via the product moment, would be close to unity.

In our groups, on the contrary, the individuals were much alike, in so far as all had matriculated and none had won scholarships to universities. In the final classes there is the further selection involved in previously passing the Intermediate Science. This uniformity must tend to make the reliability coefficients small.

§ 5. CORRELATIONS WITH OTHER TESTS

Before coming to the facts, it may be well to remark on meaning. If two examinations were found to have perfect correlation (coefficient $r = 1{\cdot}00$) then one or other of them is a waste of time and should be discontinued.

Correlation with theoretical physics as tested by college examinations

There has been nothing novel about these theoretical tests, in fact they have often consisted of old examination papers of the University of London. The correlation coefficients are set out in Table 4.

Table 4. Correlation between marks in college examinations on theoretical physics and marks in a pantopiric examination

Date	Class	n	r
1925 (Dec.)	Final II	22	0·42
1925 (Dec.)	Intermediate	16	0·41
1926 (Mar.)	Final I	12	0·34
1926 (Dec.)	Intermediate	21	0·53
1927 (Mar.)	Final I	14	0·25
1927 (Dec.)	Final II	13	0·09
1927 (Oct., Dec.)	Intermediate	35	0·69
1928 (Mar.)	Final I	12	0·28

Notes. Final I is the class consisting of those men who are in their first Final year.
Final II is the class consisting of those men who are in their second Final year.
n is the number of persons in the group who were present at the examinations in both theory and practice.
r is the correlation coefficient by the product moment.

The best value of the correlation coefficient is $R = 0{\cdot}46$ by Fisher's process described in § 4, and the standard error of $\tanh^{-1} R$ is according to Fisher

$$\{\Sigma\,(n-3)\}^{-\frac{1}{2}} = 0{\cdot}09.$$

Correlation with pure mathematics at the external examinations of the University of London

The numbers in the body of Table 5 give the numbers of students in the groups defined in the margins.

Table 5

	More successful in pantopiric examination	Less successful in pantopiric examination	Totals
Passed in pure mathematics	39	32	71
Failed in pure mathematics	14	19	33
Totals...	53	51	104

The tetrachoric correlation coefficient comes to 0·19 with a standard error of 0·16*.

Fourfold tables, like the above, were prepared separately for three Intermediate classes and three Final classes of students. The sequence of merit in the pantopiric test was divided into two portions equal or nearly so. The mathematical examinations followed the pantopiric examinations at intervals of 6, 7 or 8 months. But these separate tables showed nothing that is not better shown in the above table, which is their summary.

The smallness of the correlation between success in practical physics and success in pure mathematics tends to explain and justify the European custom of appointing in each university a pair of professors, one for experimental physics, the other for theoretical physics. For the obstacles encountered in research in theoretical physics are much more mathematical than the difficulties of the "theoretical physics" in the General B.Sc. which are largely a matter of memory and of description.

Correlation with success in the External General B.Sc. or Intermediate Science of the University of London

We divided each class into (i) successes including all those who passed the university examination at the first attempt in all their subjects with or without honours, and (ii) the rest. Each class was also divided into two equal or nearly equal parts according to the marks in a pantopiric examination held during the ten months preceding the university examination. Then the statistics of the separate classes were summed. The correlation no doubt depends on the subjects, other than physics, taken in the university examinations. For the Intermediate Science these were pure

* Obtained by the aid of K. Pearson's *Tables for Statisticians and Biometricians*, Tables 29, 23, 24. (Cambridge University Press, 1914.)

mathematics for all, together with any two chosen from applied mathematics, chemistry and geography. For the Final B.Sc. the subjects, other than physics, were pure mathematics for all, together with any one chosen from applied mathematics, chemistry and geography. In each compartment of the following table the upper number refers to the Final B.Sc., the lower to the Intermediate Science.

Table 6

	Pantopiric		Totals
	Better marks	Worse marks	
Successes in university examinations	14 18	14 8	54
The rest	8 13	6 23	50
Totals...	53	51	104

For the sum of Intermediate plus Final the tetrachoric correlation coefficient is 0·27 with a standard error of 0·15.

Here we must beware of casting undeserved aspersions. Success in an examination, in which written work predominates, has been found to have a small correlation with success in practical work; but there is yet the further question as to which kind of success is the more desirable. As a manufacturing nation our livelihood depends largely on practical skill*.

Correlation with success in practical teaching

Our students, being teachers in training, went into elementary schools for some weeks during their college course, and were there graded according to success as teachers by J. S. Ross, M.A., Lecturer in Education, to whom we are indebted for the marks. To find the correlation, we first prepared fourfold tables for four separate pantopiric examinations and then summed the four tables, obtaining Table 7.

Table 7

Practical teaching	Pantopiric physics		Totals
	High marks	Low marks	
High marks	19	18	37
Low marks	19	21	40
Totals...	38	39	77

Evidently the correlation is negligibly small. This might have been expected, for success in practical teaching depends vitally on many qualities which have little influence in practical physical work, namely on dress, voice, posture, pleasantness, strictness and so on.

* See Presidential Address and proceedings of Section J, British Association, Glasgow, 1928.

It is regrettable that the dates at which the teaching marks were assigned varied from 0 to 2·3 years after the date of the pantopiric examination; but this is hardly likely to account for the negligibly small correlation.

Correlations with Spearman's general intellectual factor g

For a long explanation and defence of the importance of g we may refer to Spearman's book, *The Abilities of Man**, and especially to p. xvi, where we find

$$r_{ag} = (r_{ab} . r_{ac}/r_{bc})^{\frac{1}{2}},$$

in which the r denote correlation coefficients between the measurements indicated by the suffixes; and a, b, c are almost any mental measurements. For instance, let a denote marks for pantopiric physics, b marks in theoretical physics, c marks in pure mathematics. Then it has already been shown on page 115 that $r_{ab} = 0{\cdot}46$ and that $r_{ac} = 0{\cdot}19$. So in order to compute r_{ag} we need also r_{bc}. It can be found from Table 8, which summarises Inters and Finals.

Table 8

	University pure mathematics	
College theoretical physics	Passed	Failed
Upper part	47	5
Lower part	25	26

By Pearson's tables (*loc. cit*) the tetrachoric r_{bc} comes to 0·70 with a standard error of 0·10.

Then by Spearman's formula, used in three ways, it is found that the correlations with g are:

g and theoretical physics	1·30
g and pure mathematics	0·54
g and pantopiric physics	0·35

The large correlation of g with theoretical physics would be gratifying to physicists if it were not impossibly large. The error can be explained by the large fractional uncertainty of $r_{ac} = 0{\cdot}19$ as shown by its standard error of 0·16.

§ 6. LIST OF EXPERIMENTS

Notes: (i) Explanations in square brackets were not shown to candidates.
(ii) The December examinations for Intermediate Science are on one term's work; hence the narrow syllabus from which electricity and magnetism are excluded.

Intermediate, 1925 *December*. *22-minute experiments. Two "vortices"*

1. Find the periodic time T of the given pendulum under two circumstances: T_0 for small amplitudes; T_1 for a mean amplitude of one radian. Work out $(T_1 - T_0)/T_0$.

2. (*a*) Find the focal length of the given lens. (*b*) Measure the magnifying power of the lens directly, and compare your result with the theoretical formula. N.B. Remember that the virtual image produced by the lens must be at a distance of 25 cm. from the lens, for your comparison to have any meaning.

* Macmillan and Co. 1927.

3. Identify the ten specimens *A*, *B*, *C*, ... *J*, and state for what purpose they are used.

[Specimens: *A*, travelling microscope; *B*, cork borer; *C*, silk band of Calendar's apparatus; *D*, rider of Atwood's machine; *E*, trolley of Fletcher's apparatus; *F*, Dewar flask; G, lump of solder; *H*, stop-watch; *I*, glass prism; *J*, copper calorimeter.]

4. Round off the sharp edges on the ends of the piece of glass tubing. Bend the tube at right angles. Write your name on a label. Stick the label on the tube. Hand the tube to the examiner.

5. Determine the water equivalent of the copper bottle, without weighing it. What improvements would you make in the apparatus if you had more time?

Provided also: one thermometer, 100 cm.^{3} glass jar, access to water tap, bunsen, tripod, stopper, 3 large corks, tape.

6. Find the weight of a metre scale.

Provided: metre scale, support, thread, 200 gm. weight.

7. Find the radius of curvature of the given concave mirror. Show the correct setting of the apparatus to the examiner before you move it out of adjustment.

8. Find the freezing temperature of the given substance.

(Spare.) Find the mass and the diameter of the given sphere, and thence calculate its density.

Provided: balance and larger weights, screw micrometer, log tables.

Intermediate, 1926 *December*. 8-*minute experiments*

1. Find the refractive index of the given liquid for NaD light.

[Total internal reflection by air-film between parallel plates.]

2. Using the given prism and spectrometer, find the spectrum produced by the light from the electric light provided. Show the spectrum to the examiner. N.B. Do not alter the foctssing arrangements of the spectrometer.

3. Measure the specific gravity of the given liquid, using the common hydrometer.

4. Find the focal length of the given lens.

5. Measure the temperature recorded by the wet bulb thermometer, and take the temperature of the surrounding air. How could these observations be used to determine the hygrometric state of the atmosphere, and what further data would be necessary before you could calculate this?

6. Bore a hole in a cork, and fit through it the large glass rod provided. Show the completed experiment to the examiner.

7. Adjust the Fletcher's trolley so that it runs with a constant velocity. Write your name on the trace you have obtained, and leave it on the bench, on the space provided.

8. Make a rectangular bend in the piece of glass tubing provided. Fix a label with your name written on it on to the tube, and leave it in the place indicated.

9. In measuring an angle of approximately one radian there is an uncertainty in the reading which amounts to 20 min. 30 secs. What is the percentage error?

10. Identify the specimens *A*, *B*, *C*, ... *J*, and state what they are used for in a physical laboratory.

11. Determine directly the magnifying power of the simple convex lens provided. Compare your result with that to be expected on theoretical grounds.

Data: The focal length of the lens is 10 cm.

12. Weigh the given ball and return the weights to their places in the box.

13. Find the mass of the given metre stick by hanging it up. The weight and loop provided are to be assumed to total 200·00 gm.

14. Set the upper surface of the glass plate horizontal. Show your setting to the examiner.

15. Determine the period of oscillation of a wood rod when swinging as a pendulum.

16. Read the vernier and write down the result. The screw-adjustments of the instrument must not be touched.

A magnifying glass is provided. [Pulfrich refractometer.]

17. Write a list of the faults displayed by this apparatus for the measurement of the thermal conductivity of a copper bar as at present arranged.

[Searle's bar.]

18. State what this apparatus is ordinarily used for, and make a list of the faults shown in its present arrangement.

[Chorlton and Lees' thermal conductivity apparatus.]

19. Write your name on a scrap of paper ribbon. Cut off enough wire to make a circle 7 cm. in diameter, allowing for overlap. Push the wire through the paper label. Solder the wire ends together. Bend the wire to circular form. Drop it in this box.

20. There are three parallel lines A, B, C on the blackboard. Calling the distance AC one hundred units, estimate AB without measuring instruments.

21. What slovenly, non-conservative or anti-social acts have been committed here? (5 objects.)

[Hardened paintbrush, loose wire-end on a reel, stained weight, mercury in an aluminium cup, lens lying on emery cloth.]

(Spare.) Measure and record the radius of curvature of the given concave mirror.

Intermediate, 1927 *October*. 5-*minute experiments*

1. Determine the critical angle of the given liquid for sodium light.

[Total internal reflection by air film between parallel plates.]

2. If I find that the acceleration due to gravity is 935 cm. sec.$^{-2}$, what is my percentage error?

3. Set the spectrometer so as to show the continuous spectrum produced by the light of the electric lamp. Show the result to the examiner. N.B. Do not alter the focussing arrangements of the spectrometer.

4. Name this apparatus. Read the vernier.

[Fletcher's trolley.]

5. Measure the volume of the small glass vessel provided by running in water from a burette.

6. What is the difference between the frequencies of these two tuning forks.

[Method: counting beats with the aid of a stop-clock.]

7. Identify the three pieces of apparatus X, Y and Z.

[X, electrolysis apparatus; Y, Searle's conductivity bar; Z, apparatus to give a constant head of liquid.]

8. Find the diameter of the given ball.

[Micrometer screw-gauge.]

9. What is this instrument? Record the reading of the vernier.

[Sextant.]

10. Find the atmospheric pressure, using the barometer.

11. What are the given chemicals used for in a physics laboratory?
[Ether, lycopodium, Canada balsam, iron filings.]

12. What is this instrument called? For what purpose is it used?
[Moving coil mirror-galvanometer.]

13. What is this apparatus? Find the periodic time of vibration for small amplitudes.
[Kater's pendulum.]

14. Name the cells provided. Also state which are the positive and negative poles of each cell.
[Four cells: Weston, Leclanché, Daniell, accumulator.]

15. Solder two pieces of wire together. Write your name on a label, and stick the label on to the wire when you have finished.

16. Measure the thickness of the block of glass near the corner which has been chipped.
[Vernier slide calipers.]

17. A millivoltmeter and shunt are provided. What would be the reading of the pointer, if a current of 16 amperes was flowing through the combination?

18. What is wrong with this Post Office box, as set up to measure the resistance of the unknown coil of wire. (N.B. Do not alter any of the connections.)

19. State for each of the wires *A*, *B*, *C*, *D*, *E*: (i) the material of the core; (ii) the material of the covering, if any.

20. State which are the "refracting angles" of these two prisms. Which prism would you use if you wanted to obtain the spectrum of mercury, using the mercury vapour lamp and a spectrometer?
[One optical glass, one cheap glass.]

21. Find the specific gravity of the given liquid, using the common hydrometer.

22. Observe and record the time of a complete oscillation of the wood stick, using the stop-clock provided.

23. What is wrong with this apparatus?
[Dip circle.]

24. Add up the weights on the right-hand side of this balance. The case must not be opened.

25. What is the name of this piece of apparatus, and what is it used for?
[Hope's apparatus.]

26. Measure carefully the length of the bronze rod and write down your result.

27. Find the length of air column which resonates to the tuning fork provided.

28. *Do not touch this apparatus.* Is the point of the knitting-pin above, below, or at the centre of curvature of the glass dish?
[Glass dish acting as concave mirror.]

29. Read and write down the temperature of the water in the thermostat.

30. What is the name of this machine? For what purpose is it used? What indispensable parts are missing?
[Lathe, no belt.]

31. Stand in the chalk ring and look at the drawing-board. On a scale of lightness and darkness, call the black square 0, the white 100. What numbers do you then give to (*a*) the background; (*b*) the brown square; (*c*) the paler square?
[Results are reported in the *British Journal of Psychology* (*General Section*), **20**, 28 (1929).]

32. *Do not touch the apparatus!* Is the point of the black needle at the centre of curvature of the mirror? Or if not, is it set too high or too low? *Do not alter it!*

33. Find the volume of the piece of sealing wax. [Wax to be immersed in graduated jar containing water.]

34. Write down the reading of the vernier. *Do not touch the milled heads.*
[Pulfrich refractometer.]

35. Write your name on a gummed label. Cut off enough wire to make a circle 7 cm. in diam., allowing for overlap. Twist the ends together. Adjust to a circle. Attach label and drop wire into the box.

36. Record the reading of the spherometer when all four feet are touching the glass plate.

37. Estimate the position of the mark *X* on the scale of 0 to 100. Measuring, either with ruler or with pencil, is forbidden.
(Spare.) Read the two verniers on the cathetometer.

Final I, 1926 *March*. 30-*minute experiments*. *Two "vortices"*

1. Compare the e.m.f.'s of two batteries each of two cells. The connections of the vane must not be altered.
[Quadrant electrometer.]

2. Find the boiling point of ether by the Clowes *J* tube. *Caution*. The flame must be put out before the ether is poured. Hand tube to demonstrator at end.

3. Determine the radius of gyration of the block of metal provided. Do not remove the block of metal from its suspension. Compare your result with that obtained from measurements on the dimensions of the body.

4. What is the number of independent coordinates that must be known in order to specify completely the position of the following things: (1) Fletcher's trolley rolling on table; (2) wheel touching table at rim and at tip of axle; (3) ball rolling on dish; (4) balance free to swing; (5) india-rubber tube hung up?

5. Determine the density of the given sphere as accurately as you can. Indicate the order of accuracy you have attained, when writing up your results.
[Weighing sphere in air and water.]

6. Determine the logarithmic decrement of the ballistic galvanometer. Do not touch the shunt plugs.

Final I, 1927 *March*. 13-*minute experiments*

1. Adjust the spectrometer so that you can observe the separation of the *D*-lines of the sodium spectrum. Show the completed adjustment to the examiner.

2. Three mistakes have been made in setting up this Carey Foster bridge for measuring accurately the resistance of the coil marked No. 8831 by Pye and Co. State what these mistakes are. The plug is assumed to be connected to a 2-volt source of supply, and it *must not* be inserted into any socket.

3. What are the given chemicals for in a physics laboratory?
[Ammonium chloride, resin, ether, eosin, Canada balsam, platinum chloride, camphor, potassium iodide, acetone, aniline.]

4. Measure the accelerations of the given sphere and disc as they roll down the inclined plane. (N.B. The inclination of the plane is not to be altered by students.)
[Fletcher's trolley-bed.]

5. Cut the given piece of glass tubing in two, and join the two pieces together again, using the blowpipe flame. Round off the jagged ends, and stick a label on which your name has been written on your specimen. N.B. Only one tube is allowed for each student.

6. State what is wrong with this Rayleigh potentiometer, as it has been set up. (4 mistakes.)

7. Two prisms α and β, and two pieces of glass γ and δ are provided. Which of the prisms and which piece of glass would give the better definition when placed between the collimator and telescope of a spectrometer adjusted for parallel light? Give reasons for your answer. [Observation by unaided eye at nearly grazing incidence.]

8. The screws and clamps must *not* be altered, but stands may be lifted. Set the edge of the reflecting prism so that it coincides with its own image formed by reflection in the *further* surface of the simple lens. Measure the distance between image and reflecting surface by using the slide carrying a horizontal boxwood rod. Record your result.

9. Estimate the number of turns on the provided coil, being given the formula for a tangent galvanometer—(tan θ) is proportional to (turns)/(radius)—and that the turns are 500 and the mean diameter 14·6 cm. The plugs may be altered, but *the wires must not be disconnected*.

10. A body is hung up by two threads so that it can make torsional oscillations about a vertical axis. For these find the ratio (period for an amplitude of one right angle)/(period for a very small amplitude). Keep the plane of the ring horizontal.

11. A potentiometer will be seen connected ready for comparing an unknown with a standard cell. *Do not alter the wiring*. You may move the switch and the tapping keys. Observe and record the points of balance and work out the ratio (e.m.f. of unknown)/(e.m.f. of standard cell). The potentiometer wire is soldered to the copper plates where it first touches them.

12. Measure the resistance and self-inductance of the given solenoid, using the Campbell variable standard. [Heaviside equal-ratio bridge arranged ready.]

13. *The adjustments must not be altered*. Read and record the settings of (*a*) Pulfrich vernier; (*b*) Vernier of slide calipers "insides"; (*c*) Vernier *on pillar* of travelling microscope (ignoring micrometer screw and its scale); (*d*) Vernier on pillar of cathetometer; (*e*) setting of micrometer screw on cathetometer.

14. Compare the two tuning forks. State which has the higher frequency and measure the difference of frequency. Provided: forks, rubber striking blocks, resonator, wax or plasticine, pendulum having a period of 2 seconds.

Final I, 1928 *March*. 15-*minute experiments*

1. Compare the e.m.f.'s of the two given cells, using the Rayleigh potentiometer. N.B. The connections have been set in adjustment, and must not be altered. The combined resistances in the two boxes must not be less than 1000 ohms.

2. Measure the accelerations parallel to the inclined plane of the given ball and disc. N.B. The inclination of the Fletcher's trolley is not to be altered.

3. Calculate the capacity between the two plates of the given condenser. State what type of error is made by assuming the aforesaid capacity to be the capacity of the upper plate.

[Parallel plate electrostatic condenser.]

4. Find the cooling constant for the given ball, i.e. (rate of cooling in ° C. per minute)/(excess temperature of ball over surrounding air in ° C.).

5. Determine the period of small oscillation of the pendulum as accurately as you can, being given that the neon lamp flashes every two seconds exactly. The scale behind the pendulum is in centimetres.

6. Cut a length of the given wire from the coil, and make a circular ring of diameter 3 inches, soldering the two ends of the wire together. Write your name on a label and attach label to the wire. Drop completed circle into the tin provided.

7. *The adjustments must not be altered.* Read and record the settings of: (*a*) the Pulfrich vernier; (*b*) vernier of slide calipers "insides"; (*c*) vernier of sextant; (*d*) vernier on pillar of cathetometer; (*e*) setting of micrometer screw on cathetometer.

8. Find the sensitivity of the balance; that is to say, the number of milligrams per 1 division shift of the centre of oscillation. Do this: (*a*) for pans empty; (*b*) for a load of 100 gm. in each pan. The rider is 1 centigram.

9. Find, for rotation of the brass ring about a vertical axis, the ratio (period when the amplitude is 90°)/(period when the amplitude is small). The plane of the ring must be horizontal. [Bifilar.]

10. Two batteries are provided: *A* consists of two accumulators in series. *B* consists of some dry cells. Use its extreme terminals. Compare the electromotive force of *A* with that of *B*, by means of the quadrant electrometer. Wires must not be taken off the key.

11. Wires must not be altered on binding screws. Measure the resistance of the coil. Estimate the accuracy of the result. State the result in a form likely to be useful next session. What peculiarity do you notice in the coil? Take out *all* the plugs before you leave (marks for this). [A very inductive coil of copper.]

12. Saw from the rod enough ebonite to make a cylinder 2·5 cm. long and machine it exactly to that length in the lathe. The cylindrical surfaces need not be machined. Slide calipers provided. Hand finished work to invigilator. The invigilator will sharpen the tool when needed.

(Spare.) A telephone is emitting a note. The electrical connections of the telephone must not be altered. Determine by resonance the frequency of the vibration, with as much accuracy as time permits.

[Telephone fixed near mouth of a resonance tube.]

Final II, 1925 *December*. 8-*minute experiments*

1. Adjust the spectrometer so as to show the two yellow lines of sodium separate, and show them to the examiner.

2. The following data occur in an experiment for determining Young's modulus (E), where $E = MgL/lA$.

M = mass = 5000·3 gm.
l = extension of wire = 0·535 cm.
g = 981 cm./sec.^{2}
L = length of wire = 745·329 cm.
A = area of cross-section = 0·00703 sq. cm.

Obtain the result as accurately as possible, when you are told that the measurement of (l) is only accurate to 0·005 cm. What is the final accuracy of your result?

3. Adjust the lens, plate, reflector and telemicroscope so as to show Newton's rings, at nearly normal incidence, to the examiner.

4. Three mistakes have been made in setting up this Carey Foster bridge. When you have found them, write an account of them in your answer book. Do *not* alter any of the connections.

5. Determine accurately the weight of the sinker in the liquid together with the supporting wire. Record also the temperature of the liquid during the weighing.

6. Cut off a short length of wire from the two coils provided, and solder the two pieces together. Attach a label with your name on it, and give the wire, etc. to one of the examiners.

7. Decribe the correction for heat leakage in Callendar's apparatus. [*J* by friction.]

8. Find the electrical energy given out by the dynamo as a fraction of the electrical energy taken in by the motor, assuming that the voltage applied to the motor is 200. Do not alter the adjustment of the rheostats.

9. In an experiment on a flywheel, whose axle had a radius of 1·27 cm., a mass of 100 gm. was allowed to fall through a height of 141 cm. While the mass was falling, the flywheel made 16·5 revolutions, and after the mass had become detached, the wheel made 114·5 revolutions in 152 sec. before it came to rest. From the above data, calculate the moment of inertia of the flywheel.

10. Tune the 'cello to the fundamental or octave of the two forks supplied, but tell the examiner whether you mean it to be tuned to the fundamental or octave.

11. Cut the piece of glass tubing provided into two portions, and then join these two portions together again. Round off the sharp edges of the two ends in the flame. Write your name on the gummed label—stick the label on the tube—hand the tube to one of the examiners.

12. There are three mistakes in the connections of this Rayleigh potentiometer. Write an account of them in your answer books when you have found them. You are *not* to put them right or alter the connection in any way.

13. Determine the velocity of sound by means of the resonance tube and tuning fork provided.

14. Set the edge of the reflecting prism so that it coincides with its own image formed by reflection in the *further* surface of the simple lens. Write down the scale readings of the pointers of the two stands and subtract. Show to examiner.

15. Find the damping coefficient of this ballistic galvanometer.

16. Set the pillar of the cathetometer vertical. Show to examiner.

17. Identify the eight specimens *A*, *B*, *C*, *D*, *E*, *F*, *G*, *H*, and state what they are used for.

18. Compare the capacities of the two given condensers. (N.B. You are to put one condenser in circuit in place of the other, but must not alter any other connections.) [Method: ballistic galvanometer.]

19. Set the axis of rotation to pass through a nodal point, and a pin simultaneously to coincide with a principal focus. Write down scale reading of mark *A* on lens-slide. Show to examiner. [Compound optical system on turn-table.]

20. Balance the Kohlrausch bridge and write down the value of the tested resistance (marked 200).

21. Find out which of the four given capacities leak.

[Provided: gold leaf electroscope, electrostatic air-condensers.]

22. Determine the readings of the two sensitive thermometers at the transformation point of sodium sulphate. A common thermometer and warm and cold water are provided. State in writing any correction which you would have applied if you had more time. Do not heat bath above 50° C. The true value is 32·38° C.

(Spare.) Write a brief account of the correction for heat-leakage in Clément and Désormes' experiment.

Final II, 1927 December. 13-minute experiments

1. Adjust the spectrometer for parallel light by Schuster's method. Determine the angle between the bright green and the *bright* blue (violetish blue) line when each is in minimum deviation. End by showing the spectrometer to the invigilator.

2. Examine the refracting faces of the prism and report on their shape. Provided: large slab of glass flat on the average, mercury light, white light, tissue, benzene. Express your results with reference to the letters *ABC*, *DEF* at the corners of the prism. [Interference fringes.]

3. Cut off a suitable length of glass tubing, and make a "Clowes" boiling point tube. Stick a piece of paper with your name written on it on to the tube.

4. Identify the specimens *A*, *B*, ... *K*, and state what they are used for. [*Specimens set*: *A*, universal shunt; *B*, cutting diamond; *C*, CO_2 bomb; *D*, Leyden jar; *E*, part of Hibbert's balance; *F*, electromagnet with magnetic liquid; *G*, Edser and Butler's parallel plates and prism; *H*, neon lamp; *J*, Chorlton and Lees' apparatus for thermal conductivity; *K*, porous pot for diffusion experiments.]

5. Measure the acceleration of the trolley when acted upon by the given weight. (N.B. The weights are not to be altered.) Write your name on the trace you obtain and send it up in your answer book.

Assume the periodic time of the vibrator to be 1/5 sec., as stated on it.

6. Find the ratio of the rate-of-fall-of-temperature of the copper ball, to the excess of its temperature above that of the air. Do not go above 100° C.

7. Make a precise comparison of two weights each nominally of 100 gm. A centigram weight is provided. Refer to the large weights as "squat" and "tall." (Marks for method as well as for result.)

8. Determine the ratio of the frequencies of the two forks provided by means of the sonometer. Logs provided.

9. Determine the logarithmic decrement of the ballistic galvanometer on open circuit. The positions of the plugs must not be altered, nor the torsion head. Bring the needle to rest, using the given small magnet, and show the spot of light to the examiner.

10. Use the apparatus set up to verify Ohm's law. [Potentiometer-tangent galvanometer.]

11. Same as No. 12 for Final I, 1927 March.

12. Find for the optical system in the black tube, the distances of object and image from the ends of tube nearest to them; obtaining two or more such pairs. Do you notice any simple properties of the observations, if so, what? What is the name of any system possessing these properties? Show your setting to the invigilator when you have done with it. [Aldis unit sight; a telescopic system.]

13. Measure and record the deflections produced by the two cells separately when the quadrant electrometer is used idiostatically. Hence compare their e.m.f.'s. Log tables provided*.

(Spare.) This apparatus is supposed to be set up for measuring the resistance and sensitivity of the ballistic galvanometer. Find the mistakes which have been made in the circuit. (N.B. The second battery connection is *on no account* to be made.)

* This led to the paper in *Proc. Phys. Soc.* **40**, 234 (1928).

Paper by J. H. BRINKWORTH, M.Sc., A.R.C.S.

Circulated before the meeting.

Physics is so essentially a subject in which numerical results are of the utmost importance that a good general knowledge of the science can only be gained when the course of study includes practical work of a quantitative nature. From the commencement of, and throughout, a course of instruction in practical physics emphasis must be laid on: (*a*) the importance of recording *all* experimental observations clearly, (*b*) the suitable variation of experimental conditions, (*c*) the avoidance of constant and systematic errors, (*d*) the estimation of, and statement of results to, the order of accuracy attained, (*e*) the use of common sense, (*f*) the application of a knowledge of theory. Towards the end of a course some instruction in manipulation and in the laboratory arts is desirable.

One function of a practical examination is to find out how much a candidate understands and remembers of a course defined, and generally limited, by some syllabus, in which he has received proper instruction. This is all an internal examination should be expected to do. An external examination should have an additional object in view, namely, the investigation of the teacher's method of instructing the candidate. The external examiner is very materially helped in gaining some knowledge of this if there is, and there always should be, an instruction that the candidate must present a properly accredited record of his practical work.

I think that the present-day examinations in practical physics are well designed to elicit all the information about the candidate and his course of instruction that one could reasonably expect to obtain in the relatively short time—3 to 12 hours—of a practical examination. This implies that I consider the general type of question set to be in accordance with the various syllabuses, that the latter are sufficiently comprehensive and well defined, that the time allowed for a practical examination is reasonable and that, in consequence, the candidate who has covered the prescribed syllabus will have no just cause for complaint. I do not think that candidates, taking any examination up to a pass degree standard, should have a choice of questions, for all questions in practical physics up to and including this standard—which is well defined—should be framed with the idea of testing the candidate's knowledge of fundamental physical measurements. Were choice allowed, difficulties in respect to the amount of apparatus available would certainly arise in dealing with large batches of candidates, if certain questions were more acceptable than others to the majority of them. However, in an honours examination, it is the general custom to ask the candidates to delete one or two questions out of ten, say, but the candidates are told that they will be asked to perform one of the remaining questions, only if it is practicable for the examiners to distribute the questions not rejected by the candidates.

The examiner must deal drastically with the candidate who cribs or cooks, but it is not practicable to prevent candidates from seeing each other's work. If, then, a candidate does "gather some roses while he may" and has common sense enough not to show how recently he has gathered his blooms, should the presiding examiner blame him?

I think all examiners realise (candidates and their teachers should realise this too) that their chief function is to *help* candidates to pass examinations. Such help is often most suitably given by the examiner asking the candidate a few leading questions.

It is quite a good scheme for the *teacher* occasionally to test students working for examinations up to intermediate standard, by giving all of them the same questions—such as eye-estimation of a length, the reading of a barometer, the determination of the focal length of a convex lens, or the spotting of an incorrect circuit, but I do not think that any number of such independent tests would enable an *outside examiner* to gauge the true merit of an examinee, and certainly he would not be able to form any opinion of the method of teaching other than that the student had been more or less well crammed.

For example, suppose the problem is "to determine the focal length of a convex lens." In the type of examination mentioned above the candidate might be allowed five minutes. His result would be, one rough value, obtained by focussing a distant object (though most candidates omit to do this) and one value calculated from a no-parallax setting with pins. On the present-day system a candidate would be allowed 1½ hours; he should make a full investigation of the (u, v, f) relation, and he could be asked for certain graphed results. As there has been no choice of question, the examiner can confidently assume that the candidate who returns a good answer to the question, having been given sufficient time in the present-day type of examination, must have been properly taught and has undoubtedly benefited from his teaching, not only in optics but in other branches of physics.

The *School Science Review* of December, 1928, contains a symposium of the views of various teachers on examinations in practical science. Here is a quotation: "In the Higher School Examination the teacher must play for safety and his pupils must...perform the 'umpteen' variations on the Wheatstone bridge and potentiometer themes." Firstly, I wish that the writer had referred to the many *applications* of these themes (though the Wheatstone's bridge can be considered as a special application of the potentiometer theme), for such a wording would have emphasised the fact that the fundamental measurement is always the same, i.e. a potential difference. Secondly, I would ask if the words "playing for safety" indicate that more emphasis is laid on the many variations, or applications, rather than on the fundamental principle involved?

Suppose a candidate is asked to determine the specific resistance of a material in the form of a wire. How often will he show evidence that, during his course of instruction, sufficient emphasis has been laid on the points mentioned above? For instance, on (*a*) by recording several measurements of the diameter of the wire; on (*b*) by measuring the resistance of different lengths; on (*c*) by interchanging the

comparison resistance and the given wire; on (*d*) by recording shift of contact which will give a deflection first one way, then the other—see also (*a*) above; on (*e*) by uncoiling the given hank of wire (generally bare); and on (*f*) by recognising that the wiring diagram would have been the same if a Post Office box were used instead of the bridge.

The examiner can generally decide whether the candidate's sins of omission are due to nervousness or to forgetfulness: he can, and will, often prompt the candidate, but the latter will recognise that help is being given, only if he has been trained to make physical measurements properly.

Paper by Prof. W. WILSON, Ph.D., D.Sc., F.R.S.

Circulated before the meeting.

The purpose of an examination is, or should be, to test with what success a student has followed a course of study and this means, in the special case of practical physics, to test his acquaintance with the experimental side of physical investigation. It is unfortunate that examinations have become an end in themselves. It is the chief goal of many or most students to pass examinations, and the efforts of teachers are too much directed to drilling their pupils in methods of meeting examination questions, when they should be devoted primarily to giving them an intelligent grasp of the subject. Examinations are not in themselves very evil institutions; but they bring a multitude of evils in their train.

My experience of practical examinations in physics is chiefly derived from the London External Intermediate and Higher School examinations and I am convinced that, in these examinations at all events, the practical part is distinctly more reliable than the paper examination, owing largely, no doubt, to the contact between the candidate and the examiner. A paper examination has always the weakness that no subject about which the candidate writes in an unsatisfactory way can be followed up, and the examiner is often in doubt about the value of what he reads. In a practical examination he has it in his power in some degree to resolve any doubts.

The syllabus should define the scope of the examination in broad outline only, and should not be minutely detailed. A detailed syllabus encourages an objectionable kind of parsimony and also misleads the candidate.

The exercises usually set in practical examinations at the present time seem to me to be of the right type. They mostly consist in the determination of physical quantities, e.g. thermal conductivity, intensities of magnetic fields, focal lengths and so forth, or in the completion of some short physical investigation such as the dependence of the frequency of vibration of a wire on the stretching force.

As far as possible the exercise should require the candidate to devise expedients and to think out how best to utilise the apparatus at his disposal. It should test his accuracy and ability to obtain observations or data, as well as his ability to make the best use of them and to eliminate, or estimate and allow for, systematic errors. Ample time should be allowed for the exercises.

It is quite common to allot only two exercises to the candidate and I get the impression that this is thought to furnish too narrow a basis on which to assess his merits. A candidate may apparently have the misfortune to be judged by one type of exercise he has never done and so suffer injustice. This kind of apprehension is not well founded. Nobody is better aware than the examiner how unsafe it is to judge a candidate on his failure to do the particular exercise allotted to him. The examiner can at his discretion give an alternative exercise; and he should have,

and usually has, at his disposal a very great variety of exercises. He may, and often does, give assistance to the candidate, and it is an extremely unlikely contingency that a reasonably experienced examiner will fail to estimate with considerable accuracy the merits or demerits of a candidate, notwithstanding the apparently narrow basis of two exercises.

No right of choice should be given. The examiner should allot the exercises, but he should be at liberty to allow an individual candidate to choose if he thinks this desirable, as for example in the case of a weak candidate.

A new suggestion for practical examinations, evoked, as I understand, by the apparent narrowness of the present examinations, has been made by Dr Richardson and Mr Maxwell. It is proposed that the candidate should carry out a large number of short elementary exercises of every variety, each to be completed in a very short time, perhaps in 10 minutes. I cannot help thinking that such an examination misses the main thing. In not a few of the experimental investigations that have contributed appreciably to the advancement of physics the observations have been made by a laboratory assistant who, with no grasp of the science at all, was nevertheless trained to take accurate readings. The practical examination should test, among other things, to what extent the candidate has advanced along the road to becoming an experimental physicist. This new type of examination is calculated, I think, to injure the teaching of laboratory work. It is desirable that the student should take a delight in carrying out physical experiments. Some of the most eminent experimental physicists have owed their success in no small measure to the aesthetic satisfaction experienced in carrying out measurements involving a long train of observations or the use of some ingeniously devised apparatus. The suggested type of examination would cause attention to be directed mainly to details like vernier readings, adjustment of spectrometers and so on, and the far more important things which are to be attained by the aid of such elementary operations would be lost sight of, and the student's interest and satisfaction with his work would suffer.

Before closing this rather meagre contribution to the subject of practical examinations, I cannot help referring to one of the headings proposed for this discussion, namely the problem of the help derived by the candidate from seeing another doing the same experiment. It is not a formidable problem at all. If a candidate is so good a physicist that he is able, by the scanty observation of someone several yards away, to seize the ideas involved in the exercise allotted to him and so bring it to a successful conclusion, he should certainly pass.

GENERAL DISCUSSION

(*Abridged report.*)

Mr J. H. BRINKWORTH, in presenting his paper, exhibited a large number of diagrams of a metre bridge, handed in by candidates at a recent examination. The diagrams were generally incorrect and indicated, in the speaker's view, failure on the part of the teachers to relate the symmetrical diagram of the Wheatstone net with its embodiment in laboratory apparatus.

Mr Brinkworth then proceeded as follows: Quite certainly, the only thing for which we, as teachers, should be able to blame our students is forgetfulness. In my written contribution to this discussion I have expressed the view that the present-day examinations in practical physics (I speak only of University of London examinations of Higher School, Intermediate and Final standards) are well designed, especially so because the examinee is given sufficient time to carry out an experiment properly. Thus, in $1\frac{1}{2}$ hours (which should be the minimum time allowable) a candidate can perform the Wheatstone's bridge experiment to which I have referred and he will be able to pay attention to the various points mentioned. Should he forget, judicious prompting by the examiner will help him to remember if, and only if, he has been taught to recognise the importance of these several measurements.

I feel that all external examiners must criticise the pantopiric method adversely because it does not allow time for an examinee to show that he has been taught, and that he knows how to carry out, a physical experiment properly. If I am right in thinking that the main object of a practical examination is to elicit such information, then my criticism of the new method can be indicated by the suggestion that the new adjective, when used before the word "examination," should be spelt "pantopyrrhic"; for this homonym may surely be taken to mean "that which completely defeats its own ends." I would like even the most junior students to be taught to carry out every experiment as if it were a piece of research. As only a limited amount of time is available in a school for the study of physics, this may necessitate curtailment of the syllabus of practical work suitable for such students. But, if I may still harp, I fear rather monotonously, on the single wire theme, I would emphasise that it is, in my opinion, better for the elementary student to try to master this important experiment than it is for him to attempt to be Jack of all trades and master of none.

Dr A. W. BARTON (Repton). As I am the first member of the Science Masters' Association to speak, it is perhaps appropriate that I should say how much we appreciate, as an association, the privilege of joining in this discussion by the invitation of the Physical Society. Having been asked to confine my remarks to elementary examinations of the School Certificate standard, I should like to emphasise in the first place that such examinations are designed to test the general

education of the candidates, not their ability as specialists in physics. Nevertheless I am strongly in favour of the retention of practical examinations at this stage for the following reason. The candidates spend some 20 out of the 24 hours of the whole examination in *writing* about things; it is only in the 4 hours' practical examination that they are required to show how well they can actually *do* things. The ability to do things is perhaps of greater value in after life than the ability to write about how to do them, and it is the practical examination which tests this "doing faculty."

The examination should test the candidate's ability to make careful and precise observations and to carry out accurate and reliable measurements. He should also be required to deduce any logical conclusions that may follow from his experimental results and be able to set out the whole of his work clearly and concisely. The latter point is most important, as complaints are continually being made that there are so many people who have something to say but cannot say it intelligibly. The Rector of the Imperial College mentioned only yesterday that he had come across cases of research students, working for the Department of Scientific and Industrial Research, who had to be turned away because the reports of their work were incomprehensible to their supervisors! The Oxford and Cambridge Joint Board School Certificate papers—the only ones of which I have any experience—do satisfy these criteria.

I feel that a comprehensive syllabus of the practical work on which the candidate will be examined should be supplied, but that it should not be unnecessarily detailed. I am not in favour of a scheme whereby a list of experiments which the candidate is expected to know is supplied, the examiner being forbidden to set any experiment not on the list; on the contrary I welcome the type of question that involves an experiment which the candidate will not have done in the course of his preparation for the examination, and even an experiment of which he does not know the theory. But I should like to enter a strong plea that such questions be worded with the greatest care so that the instructions may not be likely to be misunderstood. I may perhaps be allowed to illustrate this point by a question which we had at Repton last July; "Compare by a potentiometer the e.m.f. of the given cell with the p.d. between its terminals when *delivering a current through* the resistance *A*." I submit that the words *delivering a current through* should have been replaced by *short-circuited by*; as it was, quite three-quarters of the candidates placed the resistance *A* between the given cell and the galvanometer! It is easy to say that they should have known better; but I feel that it is expecting too much of boys to wrestle with a somewhat strange apparatus involving three circuits, especially when one bears in mind the trying conditions of an examination and their lack of discretion.

We now come to the matter of choice of questions and the time to be allowed for the examination. I believe that the fairest method is to have no choice of questions, as is the case at present. But I should like to see the duration of the examination extended from 2 to 2½ hours, as I am sure that a good candidate is unduly hurried at present.

The Oxford and Cambridge Joint Board conduct their School Certificate practical examinations without the presence of an external examiner. This is surely

unsatisfactory; if an external examiner had been present last July he could have helped the candidates in the question criticised above had he seen fit to do so. He would then have seen whether they could get an accurate result, and whether they knew what to do, but actually the examiner had little or no material on which he could estimate the candidates' worth with regard to that question. There are, of course, grave difficulties in the way of supplying examiners for such a large examination, but I hope that some attempt will be made in the near future.

Lastly I have been very interested in the "pantopiric method" described by Dr Richardson and Mr Maxwell. I should not like to express a definite opinion about it as yet, but I feel that there are possibilities in it and that it is likely to be more successful in elementary examinations than in those of a more advanced type.

To sum up, it is my opinion that elementary practical examinations serve their purpose reasonably well. Certain improvements, such as those I have indicated, might well be made. But, on the whole, the abler candidates do come out at the top and the weaker ones at the bottom. And I believe that the practical examination influences the result to just the right extent; it is only a very bad mark in the practical examination which causes a candidate who has done a good theoretical paper to lose his "credit," and he deserves to! Conversely a candidate who does a moderate theoretical paper often gets a "credit" by doing a very good practical paper, and he deserves it also.

Mr F. A. Meier (Rugby). The efficacy of any examination or system of examinations is judged by results. If by this means a large number of boys can be graded according to their merits in a particular subject, the system may be said to be justified by its results. However much we may condemn examinations, we are compelled to admit that with few exceptions this is secured by the present system, at any rate in theoretical subjects. But what I have just said does not apply to practical examinations. I am very doubtful myself as to what faculties a practical examination does test. But whatever it tests, only the very sanguine would maintain that it successfully grades a number of boys according to their practical ability.

After an experience of nearly 20 years in preparing boys for Oxford and Cambridge Entrance Scholarships and for Higher Certificate Joint Board, I feel it is still largely a matter of luck whether a boy who is proficient in experimental physics will do well in the practical examination. Now this is obviously not a state of affairs that should be permanently tolerated, and the question to be decided is whether this is inevitable in any practical examination or whether the present system could be so modified as to secure more satisfactory results.

I shall confine my remarks to the examinations just mentioned, as I can speak about them from personal experience, and I take it that they are typical of practical examinations all over the country. What should advanced practical examinations test, and what do they test?

The following is a quotation from a practical physics paper set at Cambridge many years ago: "Candidates will be expected to send up a short statement of the

method they intend to employ for each of the 8 experiments together with the measurements they intend to make. No candidate will be expected to attempt the practical work of more than 3 questions."

Here a boy was confronted simultaneously with 8 problems. He had—at a moment's notice—to think out and put down the methods he intended to adopt; and not only that, but to state the measurements he proposed to make. He was acutely aware that he had only 3 hours at his disposal and that every minute was precious. Not only had he to write 8 accounts, but he was compelled to make a choice of the 3 questions which he finally selected. In doing so he undoubtedly often chose those which he was least competent to deal with. Happily the situation has improved greatly since those days, though the element of time is still a factor of too great importance.

What are the qualities that enable the physicist successfully to discover the laws of Nature and to enlarge the boundaries of scientific knowledge? Surely they are imagination, originality, initiative, power of concentration and perseverance, the ability to observe carefully and to measure accurately. In fine—do they not include the power to draw the correct inferences and to present them in such a way as to be understood and appreciated by others? I maintain that in discussing practical examinations we should have these qualities in mind as an ideal.

I feel very strongly that in advanced examinations the element of hurry should as far as possible be eliminated, so that an imaginative boy, who is influenced most by an atmosphere of bustle and haste, may be given a chance of bringing his mind to bear in peace on one problem at a time. He should be encouraged to observe carefully and to measure accurately, and should receive credit for so doing. The physicist is always on the look-out for alternative methods for testing the results of his work and is never content with an isolated result; so in a practical examination opportunity should if possible be given for obtaining the result by more than one method. No candidate should rest satisfied with a number of isolated numerical results which are the answers to a set of different problems.

I would therefore put in a special plea for *accuracy* of observation and measurement. And this leads me to the very heart of the matter. If a candidate is to be assessed for the accuracy of his work, it is obviously necessary to know to the required degree of accuracy the answers to the various problems set. But examiners cannot spend laborious days and nights determining the constants of lenses, reduction factors of galvanometers, resistances of cells, moments of inertia, etc. to the degree of accuracy required. Possibly they shelve the difficulty by asking the master in attendance to provide all the relevant and necessary information. But this has to be obtained on the day of the examination, and the master whose lot it is to obtain it does not feel great confidence in results produced, as they are, in a hurry. If he is wise, he will allow a reasonable margin for his own errors and will send in, for example, the focal length of a concave lens as 30 ± 1 cm. instead of giving it to the nearest millimetre. This effectively prevents the examiner from marking for accuracy. Supposing even for a moment that the solutions are known to the required degree of accuracy, it is still necessary, if justice is to be done, to

assess the relative degree of accuracy for each experiment requiring the same amount of skill from the candidate. This is a far harder problem for the examiner and along this line I can see little hope.

Yet another difficulty confronts him. An experiment on magnetic moments is as fundamentally different from one on optics or heat as the scent of a rose is from that of a violet; and yet he is asked to make a numerical comparison.

In offering a solution to the difficulties just raised without sacrificing any of the principles laid down as essential, I shall deal at once with the syllabus, type of questions set, choice of questions, and time allowed.

I should like to see one question only set in the advanced examination without any choice being given. The scope of the question may be wide but if it is of an unusual kind it should be accompanied by very full instructions. The time allowed should be ample so that the candidate may not be hustled, and he should not be required to hand in his paper on the stroke of the clock, if he still has important measurements to take on which the final result depends. Possibly he should be slightly penalised for overtime.

This to my mind would solve the chief difficulty, and in large measure overcome the unsatisfactory nature of the present tests. In brief the result would be that: (1) The boy would be able to concentrate his attention on one problem at a time and give all his powers to careful and accurate observation and measurement. (2) The sense of rush engendered by the simultaneous presentation of a number of practical problems involving the visualization of a large number of experiments would be eliminated. (3) There would be no necessity for making a rapid choice in a state of excitement. (4) There would be time and opportunity for devising alternative methods of solving the problem set.

From the examiner's point of view: (5) He would no longer be compelled to compare the incomparable. (6) He would be able, at a small sacrifice of time, to find out accurately the solution to the problem and become acquainted with the practical difficulties from first-hand experience. He would therefore know the degree of accuracy that could be expected in the time available. (7) He would no longer feel any qualms of conscience in marking highly for accuracy and no uncertainty in his mind as to the justice of his marking.

Already there are signs of a change. The universities very often set two problems which are to be done in 3 hours, and give no choice. I hope that in time the number will still further be reduced to one. There are of course practical difficulties in the way of such a reform. If all the candidates are to do the same experiment there is the problem of apparatus to be considered. The equipment of the university is available for the Entrance Scholarship examination but the authorities responsible for the Higher Certificate practical examination have to be content with what can be supplied at the various centres at which the examination is held. But even so I think that something might be done to reduce the present element of luck. If three questions were set, two of which were allotted to each candidate, it would at any rate give the examiner a better chance of making a fair comparison of the work of the candidates. Needless to say I would like to know that it was obligatory

for the examiner who sets the questions to have performed all the experiments himself shortly before the paper is finally set.

As far as I know, examiners are always present at advanced practical examinations. Whether they obtain all the information they might by so being present is a debateable question that would lead us too far afield.

With regard to the problem of help gained by a candidate by seeing the other candidates at work, I feel certain that material help is often obtained in this way.

I should like to suggest that no practical work be started until a short written account of the method and the measurements each candidate proposes to take has been given by him to the examiner, together with a diagram of any required electrical circuit, optical arrangements or the like. Possibly it might be of assistance to the candidate to have before him, whilst writing the description, the actual apparatus he is intended to use. Such an account should be marked 20 or 25 per cent. of the maximum for the examination. I would even recommend that if a candidate be unable to deal with all the theory, he should receive help from the examiner and forfeit all or most of the marks allowed for that portion of the paper: but that he should not be penalised further in carrying out the practical work. I have tried the method of requiring a preliminary account on various occasions recently, and am of the opinion that it is a partial solution of the difficulty. For example, in using a voltmeter a boy connected it up quite wrongly in his preliminary diagram but in actually using it he made the correct connections, having either realised his own mistake or seen the work of a boy near him. By the new method such a boy would be penalised, whilst by the old method his mistake would pass unnoticed. Of course experimental details could be handed in with the rest of the work at the end of the examination, and only where a radical change of method had been adopted would it be notifiable to the presiding examiner.

In conclusion I am sure that practical examinations should be *practical* examinations and should not depend on ability to solve a mere theoretical conundrum. What could be more unsatisfactory than a question like the following: "Find the time of oscillation of the given rigid pendulum about two different axes. Hence deduce a value for g"? The whole of the practical work involved is merely to find a time of swing of a pendulum and the rest is unadulterated theory. This is only one example of many. On learning of the "pantopiric method" which has been described to-night I decided that I would try the experiment at Rugby so as to be able to speak with experience about it. I must say I was fascinated by the paper, and equally so by the long list of experiments given at the end of it, and I intend to borrow copiously from them in future.

I tested 60 boys in two ways, giving them first six experiments in 1½ hours, and then one experiment in 1½ hours. I was amazed at the confidence I felt in marking for the results of these short experiments. It was really perfectly wonderful to feel that I really knew that those boys whom I brought out at the top deserved to be there. I had never felt that before.

But I am not so sure what those 15 minutes test. They do test something, and test it with certainty; but I do not feel certain in my own mind that they test those

qualities in a student who is studying advanced physics that they ought to test, and I think the ideal would be a combination of the two methods: a 3 hours' paper of the short experiments, which would test technique and so on, and a 3 hours' question without choice. If we combined those two, we should come pretty near to an ideal.

Mr E. R. THOMAS (Newcastle). May I have the opportunity of giving evidence of the experience of the presence of an examiner in the School Certificate practical examination viva physics and chemistry. That has been the practice for some years, and it is the experience of all that the amount of collaboration, the amount of co-ordination between the work of the schools and the university, what the teachers have learnt from the examiners, and what the examiners have learnt from the schools in that time, the mutual confidence and respect which has grown up, is definite evidence that it is well worth while considering whether some of these very large examinations might not be broken up, or whether some means could be arranged whereby in every School Certificate practical science examination there shall be an accredited examiner present.

Mr E. NIGHTINGALE (St Albans). I should just like to make a few remarks on the first paper. First of all, from the point of view of the schools. If we could have, say, 20 experiments set up, 9 minutes to devote to each, and 20 examiners in order to check the results of those experiments, it might be rather interesting to see what would happen. As far as I could make out, you must do the checking as you go along, which necessitates a large number of examiners. We have been trying for years to get examiners to come and see what is being done in the schools during the practical examinations.

My criticism of the method is that it sorts out only the quick candidates: the fellow who can set up a spectrometer and adjustment in 9 minutes is a genius. The best work in my opinion is not done in physics by the rushers. That brings me to the point that I think it is a great mistake even in honours examinations to expect too much from the candidates. Very often those who get the first class honours are not necessarily the best physicists in the long run. Really good work is only done slowly, and people like Newton, Boyle and Cavendish all had ample time to think about what they were doing. They were not working in a rush. That is a point well worth bearing in mind. Sometimes I wonder how Faraday with his limited mathematical ability would have got on if he had had to take a university degree. I am afraid he would not have taken one!

Mr L. OULTON (Abertillery). We ought to bear in mind that 90 per cent., perhaps we may say 95 per cent., of the pupils who are being examined will not go on any further with science; they will go out into the world. We are inclined to speak as if we were going to test the ability of potential scientists or potential physicists only. I wonder if we sufficiently bear in mind that in a very large number of schools the practical examinations have an almost overwhelming influence on the type of education which is given in science to these 95 per cent. of pupils who will never go on any further after the School Certificate stage.

Mr V. T. SAUNDERS (Uppingham). I take it we are all agreed that we require practical examinations. Then we take the marks of these practical examinations and add them to the marks that we get from written papers. I am not quite sure (I think this is a point that we really need to state in discussing this matter) that we are in fact measuring two things which can be measured in the same units. I personally doubt very much whether anything is gained by adding the marks in a practical examination to those marks that we get from a written examination. That is a point which seems to me to need consideration.

If I may return for a moment to the pantopiric examination, I am totally unable to understand how you can get a correlation of 1·3°.

Mr R. S. MAXWELL. It is because the standard error is so high. The correlation is 1·19. The standard error is 1·06. That is too great, as is mentioned in the paper. Of course, it cannot really be valid.

Mr V. T. SAUNDERS. It seems to me that the distribution of marks which has arisen from the pantopiric method is totally abnormal. I cannot feel much confidence in a method of examination which seems to lead to the conclusion that a great number of candidates were excellent, a greater number were bad, and that a medium number of marks were scattered irregularly through the middle zones. I think we should like to hear something more about a method that leads to that distribution.

Mr I. WILLIAMS (University of Bristol). I am strongly opposed to practical examinations in any shape or form. My objection to these examinations is based on the following considerations: (i) The practical examination, as usually conducted, is very unfair and untrustworthy; for example, one candidate *A* may be given an experiment involving considerable manipulation while a second candidate *B* may have to make a few simple readings on the scale of an instrument. (ii) Candidates are taken away from their own laboratories, where they feel at home, and are examined in some other laboratory where everything is unfamiliar. This objection is not diminished in the slightest degree by the suggestions put forward this evening. (iii) The candidate who comes from a well-equipped school in a large town stands a much better chance in a practical examination than an equally good candidate who has been educated at a small country school. (iv) Most of the people who favour these examinations admit quite freely that candidates do get suggestions from seeing the work of others; this connivance at cheating seems to me a very disagreeable feature in connection with these examinations. The very people who wink at cheating in a practical examination would be the first to condemn anything of the sort in connection with an examination in theoretical physics. (v) The practical examination tends to encourage cramming. The proposals of Dr Richardson and Mr Maxwell will, I think, only make matters worse. The poor unfortunate candidates, for several weeks before the examination, will be cogitating over such fundamental problems as the following: (*a*) "Can you identify the Richardson silk band?" (*b*) "Do you recognise the Maxwell conductivity bar?"

Having stated some of my objections to these examinations, I will indicate briefly what I propose as a substitute: (i) Let the teacher in charge of physics give the boys as much experimental work as is possible within the limits of the money at his disposal for the purchase of apparatus. (ii) Let the teacher supply to the examiners a report on the experimental work of each candidate. I feel confident that if the teachers were allowed to work on the lines which I have indicated above they would get excellent results.

Mr H. ROTTENBURG. I cannot help feeling that the authors of the pantopiric type of examination have not gone on to the logical conclusion of their train of argument. Some years ago I examined in applied physics at Oundle School, and as a result my view was that practical examinations were not satisfactory as ordinarily set; this led me to analyse the qualities of candidates that are tested by a practical examination. I came to the conclusion that practical examinations test: (*a*) the candidate's knowledge of how the experiment should be conducted; (*b*) his physical dexterity of eye, ear and hand; (*c*) his genius or capacity for taking infinite pains with details; (*d*) his inventiveness or power to strike off suddenly in a new direction.

(*a*) can be tested by a written examination though it cannot be learnt properly except by practical work, especially as regards such points as the precautions to be taken to ensure accuracy. (*c*) may actually tell against the candidate by prompting him to strive for specially accurate results which it is beyond the power of the examiner to assess justly in marking. It will in many cases prompt a boy to spend far too much time, as judged by mark-getting results, in trying to adjust an instrument which may be out of adjustment. I have certainly found this so. (*d*) can be tested to some extent by written papers but in practical examinations the nerve tension will deter most candidates from giving rein to it, and the adventurer type may succeed inordinately or fail ignominiously. (*c*) and (*d*) are mutually antagonistic and the judgment to know when to allow qualities (*c*) or (*d*) to guide one is too difficult for examination work.

This leaves (*b*), which can be perfectly tested by a series of individual tests of which the setting of a vernier is merely one. The suggestion discussed with the late Dr Sanderson was to examine classes of, say, 24 boys at a time, giving each 24 tests for perfection of co-ordination between hand, ear, eye and brain. The tests were to take, say, 3 minutes each with intervals of 2 minutes between. Each test was to be so designed as to eliminate inaccuracy or want of adjustment of the instruments. For instance, where a rotating pointer was timed by a stop-watch the watch and pointer were driven by the same mechanism, so that there could be no disagreement between them. A number of similar experiments was actually designed. The advantage of such a system of tests is that it definitely tests only physical as distinct from purely mental excellencies. The tests can be applied to a boy or man at different ages and give him an idea of the progress of the education of his hand, ear and eye resulting from the doing of experiments. They show also the relative excellence of different classes in a school. Many will say that it would lead to boys' practising soulless exercises. The answer to this is that soulless exercises of that sort are what

make the great singer or musician; and the superlative excellence of the American golfers in matches against British golfers is almost universally put down to their will to practice soulless operations like putting, etc. The fact is that the will power to concentrate on soulless exercises is what makes the real genius, while the possibility that they may kill the future researcher is answered by remembering that they certainly do eliminate the superficially-minded researcher who has not the determination or incentive to persevere in that direction.

As regards the evil of teaching practical physics with one eye on examinations—is this not a natural reaction of teachers to the evil of setting catch questions?

Mr C. G. Vernon (Bedales). May I emphasise the importance of having an examiner present? I personally have been disgusted with the discrepancy between what I know to be the ability of some pupils and the marks obtained. Otherwise the practical examination chiefly tests the boys' ability to write an account of how they ought to be able to do things.

Mr A. G. Coombs (Barnard Castle). I should like to ask, in connection with this pantopiric method of examination, if there was any attempt at setting the candidates in order according to the estimation of the teacher before the pantopiric method was applied. Every master knows in his own mind the order of merit, and I wondered whether the correlation between the estimation of the teacher and the pantopiric results had been worked out? A great deal of the difficulty of the practical examination could be overcome by asking the master to furnish the examiner with a list of the candidates in order of merit.

Mr Dungey Lunn (Birmingham). I happen to be a member of an examining body which is examining the largest number of candidates in this country above the School Certificate and the Higher School Certificate. I think the vital point in a practical examination, if it is to be worth while at all—almost an essential point, as has been emphasised by one or two speakers this afternoon—is that it is absolutely necessary that the examiner should be present. There are 14,000 candidates examined by the board to which I refer in the Northern Universities. 8000 of those candidates took physics this year. Would you kindly inform me how any scheme whatever is to be devised whereby an examiner can visit some 600 schools and examine 7000 candidates personally?

For that reason, and for that reason only, I am entirely opposed to a practical examination at the School Certificate stage. It is an ideal but it cannot be made practicable. The same difficulty occurs at the higher stage, and the only solution of the difficulty at the higher stage is being met because the examining body, the Joint Board, tests implicitly the physics teachers in the schools which it examines, and the degree of correlation between the estimate of the master teaching the subject and the results of the examination is exceedingly high, about 70 per cent. this year. Every master sends an estimate of the qualities of his candidates. He arranges them in order of merit, and the results are correlated. But I ought to say that so far as science is concerned, the estimate is on the subject as a whole. There is no separate estimate for practical and theoretical examinations.

With regard to Mr Thomas's point about splitting up the examinations, the Examinations Council is already in very serious difficulties: it is attempting to correlate the results of eight examining bodies and it finds very considerable difficulty in securing that the standard of the examination shall be the same throughout the eight bodies. This is an important point, because there are 54,000 candidates for the School Certificate. All who are successful are going to be hall-marked with the School Certificate, and we are aiming at making the standard the same for the whole country, but we are confronted with the very serious difficulty of correlating the Joint Board examination closely with the examinations held by all the other bodies.

Mr C. J. WILTSHIRE (Cardiff). Like the previous speaker I do not think practical examinations are advisable at the School Certificate stage. In our school we should have 70 boys going in for a practical examination. We have not enough accommodation. But for the higher examination, I think it is most desirable that a practical test should be held. I am sure also that most of us welcome the presence of an outside examiner; we benefit very considerably, and I think the examiner does so too.

At the end of the examination last year I asked one of the examiners, "What is the use of these note-books? What part do they play in assessing the marks?" He replied, "We test the defeated by the note-books." I think that is a very good point, because if a boy has a note-book kept up to date and carefully written, with diagrams and so on, it is a criterion at any rate of his work during the term and I think he should be given a fair percentage of the marks on his note-book. Of course, there is the difficulty that a boy may write up very nice things copied from a card, but at the same time the examiner can judge pretty well what he has been doing during the year, especially if he is compared with other boys in the same class.

Mr E. R. THOMAS. Examiners can be found. Are they not found for oral examinations in French? The time required would not be any longer. If they are not found for oral examinations, they should be. Moreover the advantage to the schools far outweighs even the great importance of the accuracy of the examination results.

Mr W. F. HARLING (Marlborough). We have been told that candidates can be judged after a quarter of an hour's examination, and also it seems to me that we might for once take a hint out of the note-book of the Army. I refer to the Certificate A examination, in which there is a practical test and a theoretical test. The practical test is purely a qualifying one. The boy passes, or he does not; if he does, he takes the theory examination.

It seems to me that the same system might be adopted in physics examinations. The boys should take the practical examination in the presence of an external examiner. These gentlemen are extremely skilled in assessing practical ability at short notice, and after half an hour they will be perfectly clear what boys have the practical ability to entitle them to spend time on the theory.

Mr J. C. PLATT (Salford). It seems to me we have heard a very great deal about the examination but very little about the candidates. They are human beings. At first they are a bundle of nerves. For the first quarter of an hour they have not settled down at all. How can you apply the suggestion of Dr Richardson and Mr Maxwell in a practical examination? For the first 10 minutes they will do nothing at all. They are excited. They will grow worse and worse under that scheme.

Mr F. TWYMAN. In reading accounts of experiments, I always find one thing that troubles me, and that is ambiguity. It is almost universal. One may read scientific papers by the greatest exponents and sometimes be in doubt whether they mean one thing or another. Telling a truth in a way that cannot be mistaken is a thing that cannot be done in a short time, and I think that for the writing up of results of a practical examination practically no time limit should be set. Unlimited time should be given.

Mr S. T. E. DARK (Strand School). We have tried the pantopiric method at Strand School for a small number of Higher School candidates in their first year and have found it very successful indeed. It does test quite a different thing from the ordinary examination. On the other hand you want to test whether a boy has done certain experiments. If he has done them, he ought to be able to set them up quickly and get results quickly. It seems to me that the ideal plan is to combine the two methods. We had the same thing in our theoretical papers. One was a test of pure memory work, and curiously enough we found that the same boys came out well in all these methods. But the ideal plan is to combine the methods and add the results together.

Prof. J. C. PHILIP (Chairman). I will ask those who opened the discussion to reply briefly.

Mr F. A. MEIER. The pantopiric method does not require a large number of examiners. I think one examiner can perfectly well look after 20 candidates and test the accuracy of their results as he goes along. I find 18 about the maximum number that can be handled.

Dr A. W. BARTON. May I refer to two points. One is the question of schools where there are large numbers of boys taking the certificate. We often have a large number compared to our size, 45 boys, taking the certificate at any one time out of a school of 420. What we do is to split them into two batches. We have the first batch in, complete the examination, then lock them up and get the next batch started, and then let the lions out.

Secondly, I would like to refer again to the question I quoted about a potentiometer. I admit that the boys ought to have known that it was no use putting the resistance between the battery and the galvanometer, but the point is that boys do not know those things when questions are sprung on them under examination conditions. Five minutes later they all knew what they had done wrong. I do feel that in a question of that sort, which is just a novelty to them, a clear instruction might

have been given. Too much was expected of them under examination conditions at that early age.

Mr J. H. Brinkworth. I remember saying that the only thing we should blame the candidates for is forgetfulness, and Dr Barton has just made that point quite clear. When I said that I thought the present-day examinations were quite good, I was referring only to examinations of Higher School and standard upwards. I know nothing about the School Certificate type of examination. I am very sorry to hear that two candidates are expected to do two questions in 2 hours. That certainly is insufficient time. With regard to the Wheatstone bridge and the network, I am very pleased indeed to hear that everyone teaches the network at the same time that they teach the potentiometer. What I did was to bring concrete evidence that there are hundreds of candidates who do not know that the Wheatstone bridge and the network are the same thing. I think that the Wheatstone bridge experiment, to take that as an example, should be taught even to the most elementary student, that he should be taught to carry it out in the way that he would carry out a research of the finest type. I see all the students who take the Higher School examinations in the University of London, about 4000 students or so, and I find regularly that when taking this experiment a student connects the apparatus up, finds the balance point, and works out the result, and that is that. That is not sufficient. My point is that he should be able to indicate the kind of accuracy that he has obtained, and if that is not taught, then I think that the teaching is at fault.

Prof. W. Wilson. I should like to express my disagreement with the views of two or three gentlemen who have maintained, if I have understood them correctly, that you should teach physics of rather a different kind to the boy who is not going to be a scientist or a physicist but is going out into commerce or practical life. I entirely disagree with them. I think that what is desirable to be taught to one who intends to be a physicist is of immense benefit to every boy who takes an interest in science.

Mr R. S. Maxwell. Various speakers seem to imply that if we adopted pantopiric examinations the laboratory course would be altered. I am afraid they must have rather a low opinion of human nature if they seriously maintain that, because surely the laboratory course is independent of any examination, and most universities insist that a certain laboratory course should be shown to have been performed when the candidates send in their note-books.

Mr Meier and Mr Dark added a very excellent suggestion that there should be two kinds of practical examinations. Two papers should be set testing different things, one pantopiric, one ordinary, but in that case I should like if possible to stipulate that in the ordinary type of examination there should be no choice of questions; everybody should do the same question. I think that is rather important.

With regard to Mr Platt's point about nervousness, we do not find the candidates nervous when once they have settled down. They adapt themselves very quickly to the conditions.

We have not made any attempt to form our own opinions of the candidates first of all and then to correlate with the pantopiric marks.

Prof. J. C. PHILIP (Chairman). Personally I should like to do without practical examinations at all. I should be inclined to go on the pupil's record, not to depend on the result of any practical examination. That may be, of course, an unattainable ideal, and it has been very interesting this afternoon to hear different points of view expressed. The suggestion made by Dr Richardson and Mr Maxwell is perhaps of a rather revolutionary character, and I have no doubt that as Mr Meier has already put the scheme into tentative operation, many of you will take the opportunity of trying out something on the same lines. It is all to the good, if we can do it, that these variants on the stock methods of examination should be tried.

I am sure we are glad to thank the men who have opened the discussion and have read papers to us, and all who have contributed to it.

We are grateful also to the Physical Society for giving us this opportunity of airing a very important matter.

COMMUNICATED REMARKS

Mr J. R. CLARKE (University of Sheffield). Prof. Wilson states the greatest difficulty with respect to practical physics examinations when he says that the examinations have become an end in themselves. On account of apparatus limitations the number of experiments which can be set on a given syllabus (especially in electricity and magnetism) to a large number of candidates is very restricted. There is, therefore, a tendency on the part of examinees and their instructors to concentrate on these standard experiments, such as the comparison of resistances by the meter bridge. It is true that this concentration may lead to the fulfilment of one of the purposes of practical physics training, namely, the development of the ability to make accurate measurements. On the other hand, it frequently happens that if a candidate so trained be asked to make a deduction from his observations, other than that to which he has been accustomed, he is unable to do so.

It seems to me that if the pantopiric examination devised by Prof. Richardson and Mr Maxwell were adopted there would be even less incentive to train students to be able to use their observations than there is at present. This type of examination is open to other objections. One school which I have examined yearly in practical physics for the Northern Universities School Certificate presents about 80 candidates, and this necessitates four or five sessions each year. The difficulty of conducting a pantopiric examination, even for one year, and using several of the questions at more than one session will be appreciated, but I think it would be impossible to conduct it for several years without presenting the physics master with a large number of "certainties."

The system adopted by the Northern Universities Joint Matriculation Board in their optional practical physics test is the most satisfactory I know, except the internal examinations of the universities where considerable weight can be given to the opinion of the demonstrators who have observed the candidate for two or

three years. The Board instruct their examiners to regard sixteen as the normal number to be examined in a $2\frac{1}{2}$- or 3-hour session. Every candidate is required to carry out two experiments, without choice, but the limitation of the number of boys enables the examiner to assist a candidate who is "at sea" and, if desirable, to change his experiment. There is also time to ask a boy who is doing an electrical experiment one or two questions on practical magnetism, for example. Nearly all the experiments set involve three or more measurements and usually these have to be used in a way designed to test the boy's intelligence, though the measurements themselves are quite clearly indicated. If the examiner obtains the opinion of the master as to which boys are on the border-lines between passing and failing or passing with distinction, he can pay a little extra attention to them, and even a hasty perusal of laboratory note-books assists in the final placings.

Mr J. A. Tomkins (Leeds). I have not had time to study the papers very fully, but I have been much interested in the paper describing a new type of practical examination. It does seem to possess the two advantages claimed, viz. that it is a fair competition and that the tests can be spread fairly over the whole syllabus. For some years past the practical examinations in physics held in the Bradford Technical College have, wherever possible, been of the kind fulfilling the first condition, that is, all the students have been given the same experiment, but these have been of the usual type and not the short experiments described by the authors. Thus in a class of intermediate standard two experiments are set to be done in 3 hours. Where the number of candidates exceeds the number of pieces of apparatus, half the students work on each experiment and they change over at "half-time." One objection to the new method appears to be that pointed out by Prof. W. Wilson. It is very doubtful whether ten or more short experiments call for the same sustained effort and attention and the train of thought involved in one or two longer experiments of the usual type. This point is referred to by the authors in their paper and, perhaps, can only be settled by further investigation. A further slight disadvantage seems to be implied in the authors' remark that "the candidates usually looked rather tired at the end," but this probably holds to some extent for all types of examination.

DEMONSTRATIONS AND EXHIBITIONS

A Model Four-wheeled Vehicle caused to Skid by the Application of the Brakes. *Demonstration given on November* 22, 1929, *by* J. BRADLEY, B.A., The National Physical Laboratory.

The model used for the demonstration had rubber-tyred wheels 4 in. in diameter and fitted with independent brakes. It could be projected along the floor by a simple form of catapult and any desired combination of brakes could be applied. It was shown that the model tended to turn round when the back wheels were locked but continued in a straight line if the front wheels or all four wheels were locked. This effect was also illustrated with other small models projected by hand along the table. One of these models was arranged to show how in the case of a back-wheel skid the driver could avoid turning round by holding his front wheels parallel to the direction of travel of the centre of gravity of the car.

A New Periodic Effect in a Neon Discharge Tube. *Demonstration given on December* 13, 1929, *by* Dr W. A. LEYSHON.

Observations were made recently with a cathode-ray oscillograph on the shape of the discharge-current/time curves in a number of beehive Osglim lamps arranged in turn in a "flashing" circuit. It was noticed that in the case of two lamps having cathodes consisting of more open spirals than the others, the times taken to reach the maximum current in the discharge were greater. It was suggested by Dr W. H. Eccles, F.R.S., that the discharge might "creep" up the cathode, and that it might be possible to observe the movement by a specially devised stroboscopic method. It should be remarked that the order of time concerned in the rise of the discharge to a maximum value was 10^{-6} seconds.

It was thought that the effect might be accentuated, and therefore might be easier to detect, if the cathode were very much larger than the anode, and if the latter were placed as asymmetrically as possible with respect to the former. The two I-type lamps shown were made up in the research laboratories of the General Electric Company; in each the anode is about 4 mm. long and is placed opposite the lower end of the cathode, which is of the dimensions usual in this type of lamp. The lamps were filled with pure neon at a pressure of 14 mm. of mercury.

During an attempt to obtain the static volt/ampere characteristics of these lamps in the usual way, it was found that for certain values of voltage and current small periodic fluctuations occurred, the cathode glow and the anode glow undergoing changes in position simultaneously with the fluctuations. When the two glows were nearest to each other the current was greater and the voltage less than when they were more separated. The actual changes in current were in general a small fraction of the total current passing through the tube, and they took place quite suddenly.

The movement of the glows could be followed by eye, the periodic time being of the order of $\frac{1}{2}$ min. in most cases. In one type of oscillation it was noticed that the top of the cathode glow crept up the electrode and, when it just reached the top, the whole glow contracted suddenly to the top of the cathode, the anode glow moving to the far side. The change in current occurred at the instant of contraction. The glow now stayed at the top of the cathode for some time, very gradually creeping downwards, and then suddenly dropped, the current suddenly rising. Sometimes the movement was in two stages, the glow waiting for some seconds half way up the cathode before going on its upward or downward way. The current here had an intermediate value. Other types of movement were noticed with different values of current through the lamp.

The phenomenon was observed originally with a high variable resistance, of the order of 100,000 ohms, in series with the neon tube. When this resistance was slightly reduced the periodic time of the fluctuations was diminished. A similar effect was obtained by increase of the applied potential. Later, a saturated diode (triode with grid and anode connected) was connected in series to control the current. This arrangement was found very convenient. In one experiment, heating the tube slowed up and finally stopped the oscillations; cooling it by resting it on a block of ice increased the frequency. One tube gave rapid flickers with comparatively large fluctuations in current when near the extinction point. It was found that a transverse magnetic field in one direction (such that an electron current would be urged upwards) increased the frequency; in the opposite direction it reduced the frequency. The anode glow moved rapidly from one part of the anode to the other, changing in appearance as it went.

For certain circuit conditions a note was heard in telephones included in the circuit, generally when no periodic movements of the glow were being observed. In one case the note gradually fell in frequency, and when it could no longer be heard the glow suddenly contracted. The glow then gradually spread out again and jumped upwards, and the process was repeated. The phenomenon may well be associated with the production of ionic oscillations. The observed movements of the glows and the changes in current indicate that the periodic occurrences observed are some form of relaxation oscillation.

It is hoped to examine the phenomenon in more detail in the future.

A Method of Showing the Phase Relation between the Flash and the Fork in a Fork-controlled Flashing Neon-tube Circuit. *Demonstration given on December* 13, 1929, *by* Dr W. A. LEYSHON *and* Miss TERESA M. DILLON.

The fork-controlled flashing neon-tube circuit was first described by Dr W. H. Eccles and Dr W. A. Leyshon*. Later, a theoretical and experimental investigation of the controlling action was made, a cathode-ray oscillograph being used†. It was

* "Mechanical and Electrical Vibrations," *The Electrician*, **97**, 65 (1926).

† "On the Control of the Frequency of Flashing of a Neon Tube by a Maintained Mechanical Vibrator," *Phil. Mag.* **4**, 305 (1927).

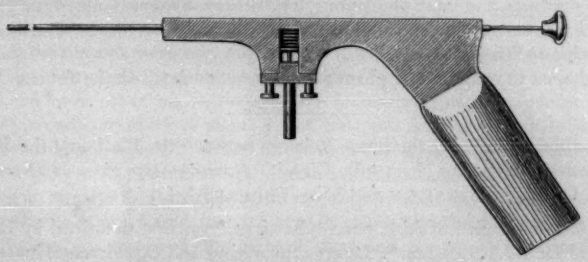
shown that the fork would attain its maximum amplitude when its effect on the period of flashing was greatest, the period being shortened by the reaction of the fork. Further, in this condition the flash occurred when the fork was in a position of maximum displacement away from the attracting coils. It was also shown that if the circuit conditions were altered within a suitable range so that the frequency of flashing without control would be rather greater, the fork could still be maintained with diminution of amplitude and alteration of phase.

In the present apparatus light from the neon lamp is received on the objective of a microscope. Part of the field is obscured by one prong of the tuning fork, which in this apparatus has a frequency of 100 ~. The microscope is focussed on the upper surface of the prong, and when the fork is being steadily maintained by the flashing circuit the dark edge appears to be quite steady. Its position alters laterally with any change in the phase relationship. Light from a small lamp is focussed on the edge of the fork, so that a number of bright spots of light can be seen when the fork is stationary. These are seen as bright lines when the fork is vibrating, their length giving the amplitude of the vibration. The position of the dark edge with respect to these bright lines is an indication of the phase. When the fork is vibrating with maximum amplitude it is seen that the dark edge is at the end of the bright lines corresponding to the position of the fork furthest from the coils. By this simple means the corresponding changes of amplitude and phase can be observed and measured.

The apparatus also shows visually the non-uniformity of beats which occurs when the fork has not yet attained sufficient amplitude to pull the circuit into tune.

Apparatus for use in a Hospital Radium Service *exhibited on December* 13, 1929 *by* Prof. F. Ll. Hopwood, D.Sc.

Radium is used in hospital practice in two forms. Solid radium salt ($RaSO_4$) is enclosed in hollow needles made of platinum; while radon or radium emanation



Instrument for implantation of radon seeds.

is pumped off from a solution of radium bromide in acidified distilled water, purified, and sealed into minute glass capillaries which are in their turn inserted into

platinum jackets. Radium needles vary in length from 1 to 6 cm., and radon "seeds" are usually about 6 mm. long.

Tests for leakage of needles are carried out by means of an α-ray electroscope, and the strength of needles and "seeds" is measured by means of a γ-ray electroscope due to Hollweck.

In addition to the above-mentioned apparatus, special protected carriers, and a microscope designed for the rapid visual examination of radium containers were exhibited.

Finally various forceps for handling and inserting needles were exhibited and also a "gun" (see illustration) which is used for the insertion of radon seeds into patients.

BOOK REVIEWS

The Effects of Moisture on Chemical and Physical Changes, by J. W. SMITH, B.Sc., Ph.D. (London: Longmans, Green & Co. Text-books of Physical Chemistry edited by Sir William Ramsay and F. Donnan.) 15*s*.

For many years past the action of minute traces of water in catalysing chemical changes has been a matter of considerable interest. There was, however, no connected account of the subject available, and, in order to comprehend it, it was necessary to read through most of the original literature. The above book will be extremely useful to those who wish to know something of the work that has been done on intensive drying, but have no time to adopt the above procedure. It can also be recommended to the research student, as it is an excellent guide to the original literature.

The latter part of the book will, probably, be the more interesting to the physicist. It deals with the changes that have been observed in boiling point, vapour pressure, density and other physical properties when the last traces of water are removed from pure substances. The theories of molecular association that have been put forward to explain these changes are discussed. It is a pity that the author was unable to indicate the relative merit of the work described in this section, since there is a certain amount of contradiction in the results obtained by different workers.

In a chapter on dissociating compounds, it is stated that the accepted value for the vapour density of ammonium chloride is that obtained by Smith and Lombard which corresponds with 60 per cent. dissociation. This suggests that the results of H. B. Baker were incorrect, for he found that normally the vapour was completely dissociated but that when intensively dried it was practically undissociated. Actually, Baker's experiments are confirmed in two recent papers—Braune and Knoke, *Z. physikal. Chem.* **135**, 49 (1928); Smits and de Lange, *J. Chem. Soc.* **48**, 2499 (1928)—where it is conclusively shown that the undried vapour is completely dissociated.

Gaseous Combustion at High Pressures, by WILLIAM A. BONE, D.Sc., LL.D., F.R.S., DUDLEY M. NEWITT, Ph.D., D.I.C. and DONALD T. A. TOWNEND. Pp. xiii + 396, with 148 illustrations and 14 plates. (London: Longmans, Green & Co., 1929.) 42*s*. net.

This is a companion volume to the book on *Flame and Combustion in Gases* published in 1927. It tells the story of the combustion of hydrogen-air, carbonic-acid-air and methane-air mixtures at various initial pressures between 3 and 175 atmospheres. All this has made necessary a very special technique which has been very largely developed in Prof. Bone's laboratories at South Kensington. The first seven chapters are devoted to a description of these laboratories and their special equipment for high pressure work. Eight chapters devoted to the characteristics of explosions, including a discussion of the much discussed "activation of nitrogen in carbonic-acid-air explosions" follow, and then come chapters devoted to the distribution of energy, the condition of the medium at the moment of maximum pressure, the calculation of mean maximum pressure, the heat capacities of the gaseous products and the influences of pressure and temperature upon the explosion ranges of gaseous mixtures. Most of this work is very recent and a good deal is extremely controversial. Practically all the experimental results have appeared in either the *Proceedings* or the *Transactions of the Royal Society* since 1915. The data contained in the original papers are now superseded by "corrected" data contained in the present volume. So far as the investigator is concerned, that would appear to be the main

justification for the appearance of the volume at this time. A good deal of its matter is of the nature of *unreife Frucht*. What, for instance, shall we say of all the pother about the activation of nitrogen in carbonic-acid-air explosions? Industrial concerns here and abroad are avid for processes of nitrogen fixation. Would they venture a brass farthing on processes based upon the experimental results and theory contained in the volume? We think not; they would at least look on both sides of their coin before parting. Industrialists ask for nitrates and are given *p/t* diagrams. Evidence of the combination of nitrogen and oxygen, to a small extent, was obtained in Cavendish's original experiment of sparking a mixture of oxygen and nitrogen. Is there any evidence that the amount of "activation" or combination occurring in Prof. Bone's experiments is any greater, and such as could be detected chemically by anything but the most delicate reaction? If there is, it has escaped our notice. The book is one of considerable value for the general scientific worker and technologist, more especially those concerned with the preparation and handling of gases. These will find the chapter on the preparation, storage and compression of gases very valuable. Why our old, familiar, tried and trusted Bone and Wheeler gas analysis apparatus, modified principally by the use of hydraulic pressure for controlling the mercury level—a device due to Stockings—should be called the Bone-Newitt apparatus is an inscrutable riddle. What's in a name? We note that the Wheeler-Payman "supposed" law of flame speeds is given its *congé* on p. 117. The book is well got up, and is very well printed on good paper; adequate name and subject indexes are included. Future editions would be improved by reference to the work of Haber and Wolokitin on the oxidation of nitrogen in the hydrogen flame and to the work of Garner, Lewis, Canby and Woodbury. And still the occurrence of what may be called "South Kensington myopia" in authors who have felt "reluctance in writing this volume because of its necessarily being devoted chiefly to our own researches" is excusable, if perhaps regrettable. The book is not unreasonable in price.

J. S. G. T.

The Kinematical Design of Couplings in Instrument Mechanisms, by A. F. C. Pollard, A.R.C.S., A.M.I.E.E. Pp. 64. (Adam Hilger, Ltd.) 4*s*. 6*d*. net.

It is pleasing to find a writer on the subject of mechanisms who prefaces his essay with a reference to principles laid down by Willis, Kelvin and Clerk Maxwell and so approaches the discussion of the correct design of couplings from the only logical standpoint, and this to good purpose. This mode of approach, as the author of the present little monograph explains clearly in his earlier pages, consists in obtaining first a clear conception of the number of degrees of freedom required in the coupling or connection and then in an examination of the means by which the necessary number of constraints and no more may most conveniently be provided.

After a brief but adequate treatment of the subjects of variance and accuracy in mechanisms and a necessary classification of couplings according to their degrees of relative freedom, there follows the principal section of the book, devoted to a number of well-chosen examples of the application of correct kinematical design in actual instruments by well-known makers, all of which are admirably explained. The book closes with a short section on procedure in design, expressed in rather general terms, and it is to be hoped that the author will in a future edition develop this important aspect of the matter more fully.

The need for such a book as this is obvious enough, for there are few experimenters who have not had to say hard things about the performance and the adjustments of many of the instruments which they handle. This well-printed and lucid little treatise must therefore be commended to all users and designers, especially of surveyors' instruments, as well as to the much wider circle engaged upon other kinds of mechanical designing, seeing that it draws attention to fundamental principles of kinematics which are too often neglected or ignored.

E. H. L.

THE PROCEEDINGS OF THE PHYSICAL SOCIETY

VOL. 42, PART 3 *April* 15, 1930 NO. 233

NOTE ON THE PHOTOGRAPHY OF FABRY AND PEROT INTERFEROMETER FRINGES BY THE USE OF A SIMPLE OPTICAL SYSTEM

BY S. E. GREEN, B.Sc., Imperial College of Science.

Received November 16, 1929. *Read and discussed January* 24, 1930.

ABSTRACT. A simple method of photographing Fabry and Perot interferometer fringes is outlined, and some examples of the results obtained with mercury radiation are given.

AFTER some experiments on the photography of the fringe systems produced by a Fabry and Perot interferometer it was thought that a few examples such as those which accompany this note, while possessing no originality, might not be devoid of interest as roughly indicating the results to be obtained by the use of a simple optical system. The only auxiliary apparatus employed, together with the necessary camera and source of light, was a convergent lens. The arrangement adopted is shown diagrammatically in Fig. 1.

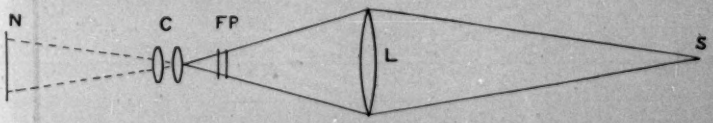


Fig. 1. Arrangement of apparatus.

The condensing lens *L* caused light from the source *S* to pass through the interferometer plates *FP* to form an image of the source approximately at the front of the camera lens *C*, the fringe system being observed on the receiving screen *N*. A convenient arrangement was obtained by placing the camera lens and the source of light at distances of about 40 cm. and 80 cm. respectively from the condenser *L*, which was a single uncorrected lens of approximately 25 cm. mean focal length. This lens was of large aperture (14·5 cm.) and thus possessed the dual advantage of collecting a considerable amount of light from the source while subtending a large solid angle at the camera lens. The interferometer plates were placed as close to the camera lens as the interferometer mounting would allow (about 6 cm. away). By

this means the plates, which had an aperture of 2·6 cm., were prevented from cutting off an appreciable fraction of the cone of light converging on to the camera lens from the condenser. Hence the angle of the cone of illumination falling on to the receiving screen (shown approximately by the dotted lines in the diagram) was maintained fairly large. The visible portion of the mercury vapour lamp employed as the source of light being rectangular (3·5 × 2 cm.), the condensing lens L was turned until the patch of light on the nearer interferometer plate was approximately square and just about covered the plate so that a minimum quantity of light was lost.

With the optical system arranged approximately as described, the quarter-plate screen of the camera could be almost entirely covered with fringes. The camera lens possessed a focal length of 29 cm. and for the photography was used with the stop at $f/11$.

The use of a medium aperture condensing lens of short focal length, placed closer to the interferometer plates to obtain good covering power on the receiving screen, was prevented by the presence of the interferometer bed which carried the movable plate. This bed extended for a distance of nearly 20 cm. beyond the plates towards the condensing lens. All the apparatus rested on shelves fixed to the laboratory wall, the interferometer being on a separate shelf. Mechanical insulation of the interferometer was further improved by the insertion of a thick layer of cotton wool underneath the baseboard of the instrument.

Ilford Special Rapid Panchromatic plates were employed in taking the photographs shown in Fig. 2. The details were as follows:

Table. Details relating to Fig. 2

Photograph	Fringe system	Approximate plate separation (mm.)	Exposure (sec.)
(*a*)	Full mercury	0·7	0·5
(*b*)	Full mercury	2·0	1·0
(*c*)	Mercury green λ 5461	0·9	75
(*d*)	Mercury green λ 5461	2·8	75

The satellite shown in Fig. 2 (*d*) appears to be situated at about a quarter of an Ångstrom unit from the main line*. Calculation shows that with the plates approximately 3 mm. apart such a satellite should give fringes about equally spaced between those of the principal system. The edges of the main fringes on the sides of higher wave-length are diffuse. These results are in very fair agreement with the measurements given by H. G. Gale and H. B. Lemon in a discussion of the fine structure of the salient mercury lines†. In their paper a grating photograph of λ 5461, obtained from a Cooper-Hewitt lamp, is given.

A filter to isolate this radiation was conveniently made by mixture of dilute solutions of malachite green and potassium bichromate. The glass cell containing

* This satellite is on the side of lower wave-length.

† *Astrophysical Journal*, **31**, 78 (1910). See also J. C. McLennan, *Proc. R. S.* A. **87**, 276 (1912).

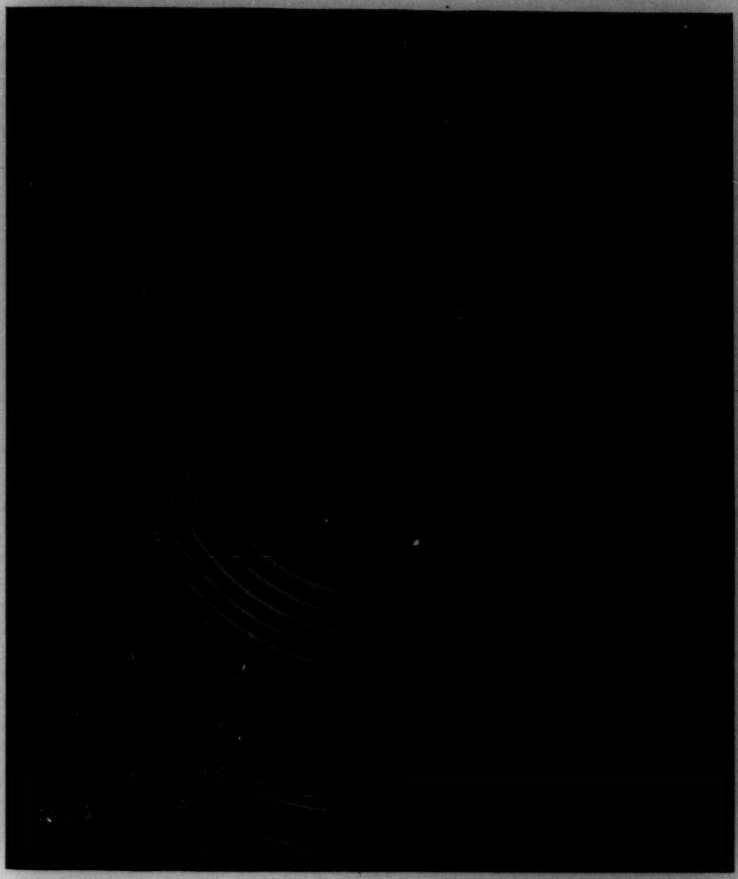


Fig. 2 (*a*). (Original width 6·2 cm.)

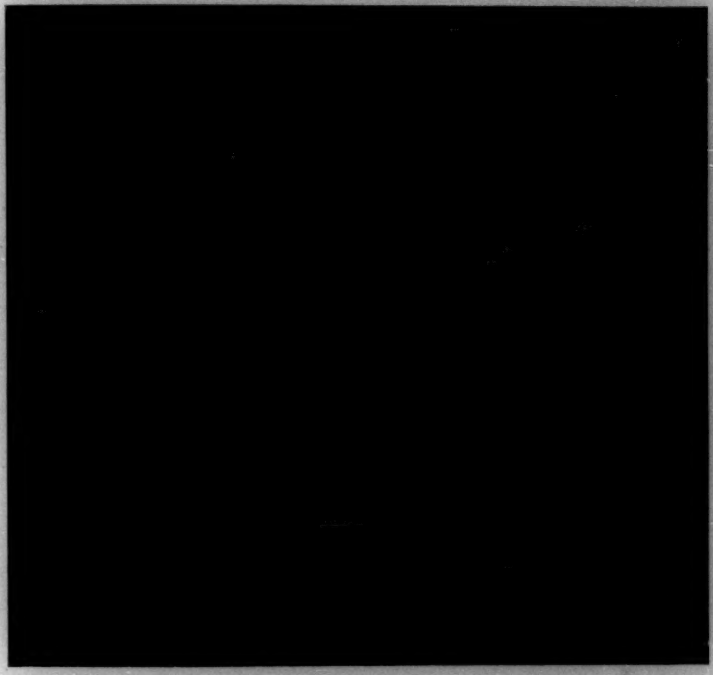


Fig. 2 (*b*). (Original width 5·8 cm.)

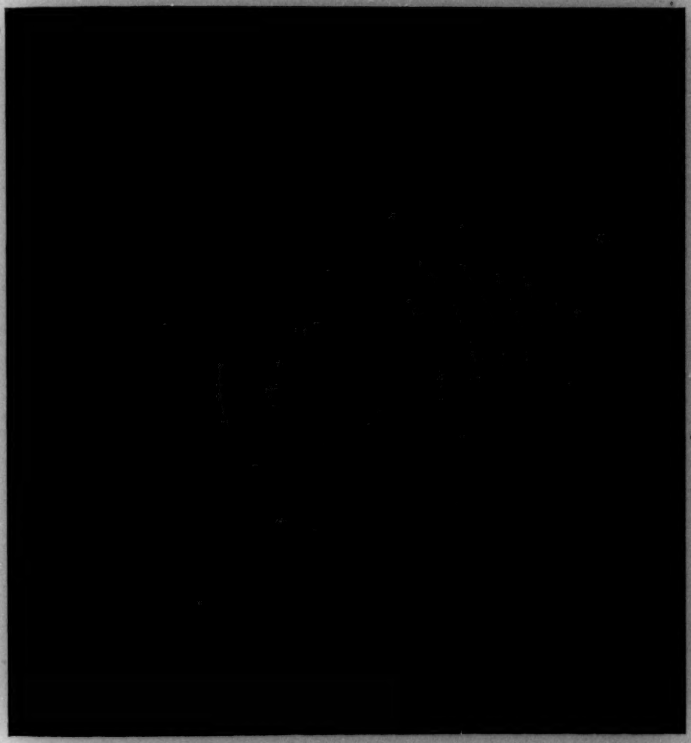


Fig. 2 (*c*). (Original width 5·8 cm.)

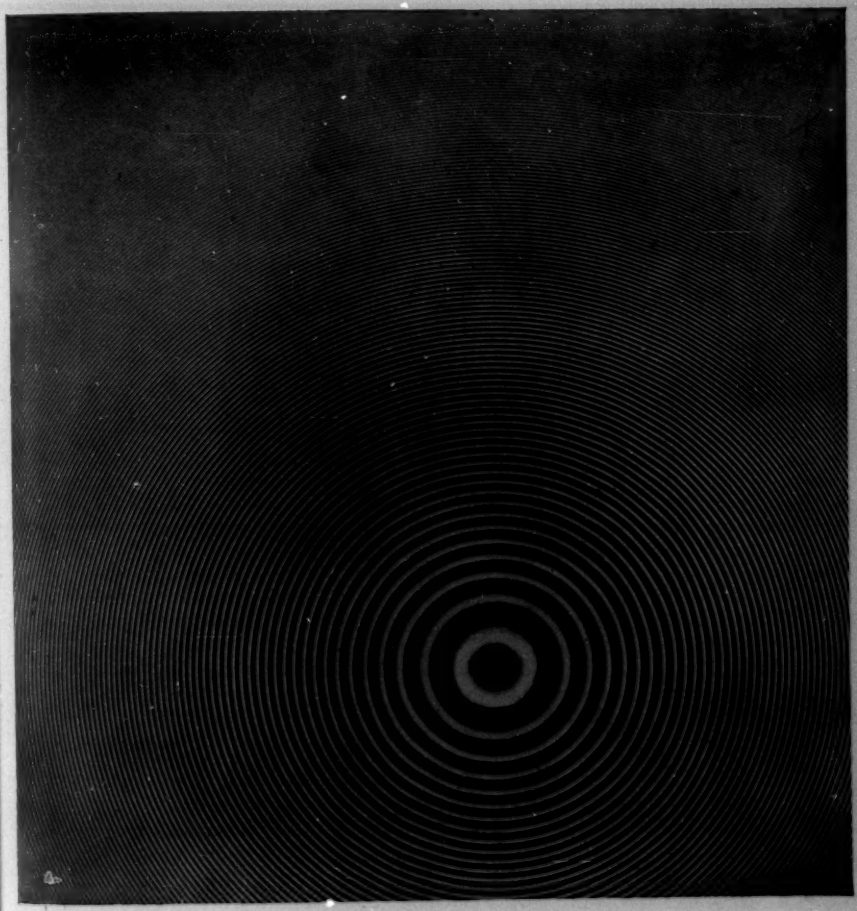


Fig. 2 (*d*). (Original size 4·3 cm.)

the liquid was placed close to the interferometer plates on the side towards the incident light. The manner in which such a filter may be adjusted to isolate the desired green line is approximately as follows. A fresh dilute solution of malachite green crystals absorbs the orange and red portions of the spectrum roughly between the wave-lengths 6400 and 6000 Å.U. By increase of the concentration this absorption band may be made to extend into the red at 6700 and the green at 5100. The edge of the band in the red is sharp and varies in position with the solution-strength much less than does the more diffuse edge of lower wave-length. When the concentration of the malachite green is adjusted so that the range of elimination stretches from about 6600 to 5500 the strong pair of yellow lines λλ 5791, 5770 are cut out while the required green radiation is transmitted. A solution of this strength also exhibits absorption in the violet, and in the blue as far as about λ 4500. In this region, however, the absorption is not strong enough to remove entirely the strong violet line λ 4358, so that the addition of a little potassium bichromate is necessary both to complete the absorption in the violet and to carry the elimination sufficiently far into the blue to remove the green-blue radiation λ 4960. Incidentally this filter cuts out a group of orange-red lines in the neighbourhood of 6200. In the case of the mercury lamp used in the present experiments these radiations were relatively strong only when the lamp had just been switched on when cold. Otherwise they were practically invisible.

Figures 2(*a*) and 2(*b*) exhibit some lack of uniformity in illumination. In these cases, the necessary exposures being short, this uniformity could have been improved by placing a circular stop 2 cm. in diameter immediately in front of the lamp and keeping the condenser *L* perpendicular to the optic axis of the system. The resulting diminution in the amount of light falling on the receiving screen would not have mattered greatly, being corrected for by longer exposure. The much more extended exposures necessary owing to the presence of the filter when the green fringe system alone was being photographed made the arrangement actually adopted preferable for figures (*c*) and (*d*), since thereby little light was lost.

I should like to take the opportunity of expressing my gratitude to Dr L. J. Freeman, in collaboration with whom some earlier experiments on the subject were performed, for his very extensive assistance and interest and for being so kind as to lend me the camera with which these photographs were taken.

DISCUSSION

Dr J. S. Anderson. I think the author is to be congratulated on obtaining such beautiful interferograms with simple apparatus. It would enhance the value of the paper if he would add information regarding the type of mercury vapour lamp used and its current consumption, and also the degree of enlargement of the photographs from the originals. Has the author tried any other type of plate? In work with the Hilger universal lens interferometer I have obtained satisfactory photographs with exposures of about 15 sec., using Ilford rapid chromatic plates and a Wratten and Wainwright mercury green filter, and overrunning the mercury

vapour lamp (a quartz one). In photographing Fabry and Perot fringes the conditions are somewhat different and it would not, I presume, be advisable to overrun the lamp. For my work it would be of great advantage to obtain a more sensitive plate, but the grain must be of small size as the original photographs are small and require to be enlarged.

Dr L. C. Martin. It is usually the experience that a quartz mercury vapour lamp is unsuitable for interferometric work on account of the internal pressure developed when the lamp grows hot. I take it that Mr Green used a Cooper-Hewitt lamp for such beautiful photographs as those which he has shown us?

Author's reply. The mercury lamp used was a Cooper-Hewitt lamp, having a glass tube 2 cm. in diameter and carrying a current of 3·3 amp. It was inadvisable to obtain a brighter source of light by overrunning the lamp, in view of the broadening of the spectral lines consequent upon the increase of temperature and pressure. The preliminary set of photographs taken in earlier experiments and shown at the meeting have enlargement factors from the original negatives varying between 1·8 and 2·5, while the positives in Fig. 2 have corresponding values 2·2 in the case of (*a*), (*b*) and (*c*) and 3·5 in the case of (*d*). No systematic test was made with other types of photographic plates.

With regard to the effect of the conditions in the source on the interference pattern produced, perhaps I may mention that on attempting to analyse the mercury radiation from a vertical capillary arc operating at a pressure of about 90 cm. of mercury and carrying 0·3 amp. I found that the fringe system in a Fabry and Perot étalon had vanished almost completely, whereas the Cooper-Hewitt lamp gave satisfactory fringes. This was presumably due to the excessive widening of the spectral lines at the high temperature and pressure of the capillary arc. It is possible that the fringes given by the Cooper-Hewitt lamp might have been further sharpened by reduction of the current strength below 3·3 amp., but any advantage gained would have been largely offset by the necessity for longer exposures.

CHARACTERISTICS OF DISCHARGE TUBES UNDER "FLASHING" CONDITIONS AS DETERMINED BY MEANS OF THE CATHODE RAY OSCILLOGRAPH

By W. A. LEYSHON, Ph.D.

Received November 7, 1929. Read and discussed January 24, 1930.

ABSTRACT. An account is given of a method of determining the current-voltage characteristics of discharge tubes under flashing conditions, a cathode ray oscillograph being used. Results are given for neon lamps of the commercial Osglim type. Current/time and voltage/time curves are derived from the oscillograph records. It is suggested that the method might prove useful in investigations of intermittent discharges in various gases under different conditions of pressure and disposition of the electrodes.

§1. INTRODUCTION

Experiments have been carried out during the past two or three years in which a flashing neon tube circuit has been used for the excitation of nerve preparations in order to produce limb movements of natural form. It was thought desirable that some information should be obtained about the nature and duration of the condenser discharges which were utilised for this purpose, and accordingly a number of observations were made with a cathode ray oscillograph. A very brief account of the experimental method and of the results obtained has been given in a recent paper[1].

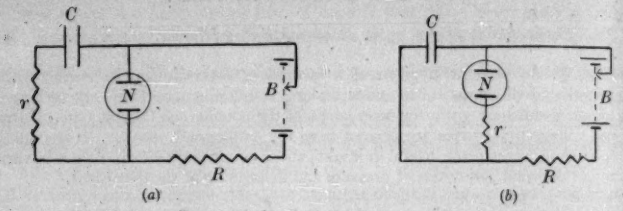
In the course of the experiments results were obtained relating to the characteristics of flashing neon tubes which were not relevant to the original purpose of the investigation, but seemed to be of general interest. It is these results, not previously recorded, which are set out here.

Clarkson[2] in a series of papers has given accounts of experiments on the characteristics of discharge tubes in flashing circuits; the method of experimentation and the circuits which he used differed from those described in the present paper. For the purpose of the physiological experiments it was necessary to include a resistance or an inductance in series with the condenser or the neon lamp in the now well-known flashing circuit first described by M. D. Hart and also by Pearson and Anson. The characteristic curves given here are therefore those for a neon lamp in series with an ohmic resistance, or an inductance, or both, through which a condenser discharges at regular intervals.

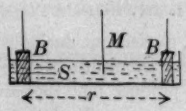
Observations have been made on about ten different Osglim lamps, chiefly of the "beehive" pattern; the latter were used in the nerve excitation apparatus, since, with other circuit conditions the same, larger quantities of electricity passed during discharge through this type of lamp than during discharge through other types.

§ 2. DESCRIPTION OF APPARATUS USED

Fig. 1 at (*a*) and (*b*) shows diagrammatically the arrangement of the flashing circuit. The neon lamp *N*, from which the series resistance in the cap has been removed, is in parallel with the condenser *C* which has the non-inductive resistance *r* in series with it, Fig. 1 (*a*), or alternatively the resistance *r* is in series with the neon lamp, Fig. 1 (*b*). The charging circuit consists of a battery of dry cells or high-tension accumulators and a resistance *R* which may vary from 10,000 to 100,000 ohms.

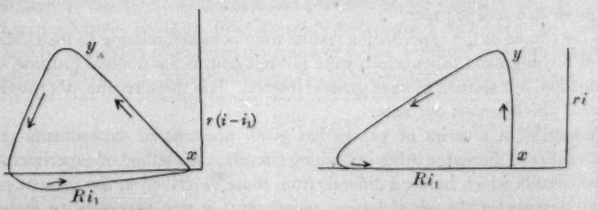


N neon lamp; *R*, *r* non-inductive resistances; *C* condenser; *B* battery.



B, *B* copper blocks; *S* copper sulphate solution; *M* copper wire.

Fig. 1. The flashing circuit.



(*a*) Beehive lamp *N*: $r = 7600$ ohms; $C = 0{\cdot}015\ \mu F$.

(*b*) Lamp *B*5: $r = 1000$ ohms; $C = {\cdot}05\ \mu F$.

Fig. 2. Typical oscillograph figures recorded by tracing.

In the physiological experiments a non-inductive potential divider of the type shown in Fig. 1 (*c*) has been used in the discharge circuit. This consists of two copper blocks, *B*, *B*, in a solution of copper sulphate; the copper wire *M* dipping into the solution acts as the movable point. For the purpose of obtaining the oscillograph records, this arrangement has occasionally been replaced by a copper sulphate liquid resistance of the pattern described by Jordan[3].

The potential differences developed across R and r are applied to the two pairs of deflecting plates of a cathode ray oscillograph. The figures seen have been recorded either by tracing or photographically using bromide paper.

It will be observed that in the arrangement of Fig. 1 (*a*) both the charging and the discharge current of the condenser C pass through the resistance r, whereas in the arrangement of Fig. 1 (*b*) the discharge current only passes through r.

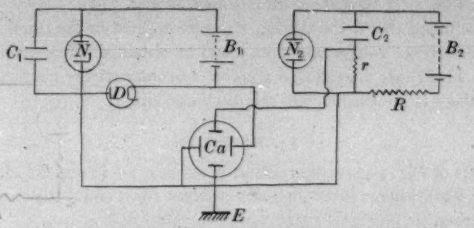


Fig. 3. Apparatus for obtaining discharge-current/time curves.
C_1, C_2 condensers; B_1, B_2 batteries; N_1, N_2 neon lamps;
Ca cathode ray oscillograph; R, r resistances.

Some typical results are shown in Fig. 2. The parts marked XY are rather faint in the actual oscillograph figures owing to the rapidity of travel of the oscillograph spot, and are therefore more difficult to trace accurately than the rest of the figure. Discharge-current/time curves have also been obtained with a neon-tube diode flashing circuit, shown diagrammatically in Fig. 3. The lamp N_1 (usually an I-type lamp) is in series with a diode D (a triode with anode and grid connected) and

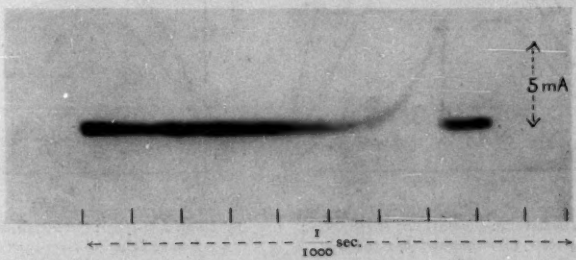


Fig. 3 (*a*). Discharge current (circuit resistance 2000 ohms).

battery B, the condenser C_1 being in parallel with N_1. Since the charging current to the condenser is by this means kept constant (the filament temperature of the diode being low enough to ensure that the saturation current is taken throughout) the potential difference across the condenser (and therefore that across the diode) changes linearly with time during the charging period. This latter potential difference is applied to one pair of plates of the oscillograph, and affords a time base. To

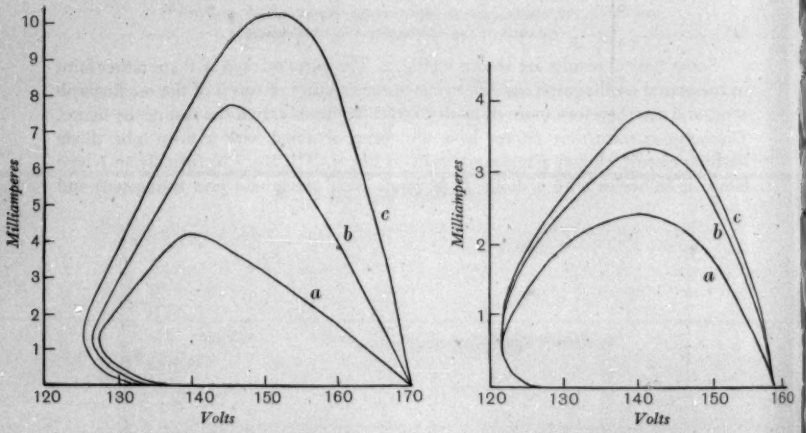
the other pair of plates are connected the two ends of the resistance r in the discharge circuit of the second neon lamp N_2. The lamp N_1 is made to flash at a frequency much higher than that of N_2. In order to keep the oscillograph figure steady, a small coupling is introduced between the two circuits.

The discharge-current/time curves, recorded photographically on bromide paper, are found to be of the same shape as those deduced (see § 3) from the records like those of Fig. 2. A typical record is shown in Fig. 3 (*a*).

It may be noted that if the voltage Ri_1 were applied to the oscillograph plates, a record of condenser voltage and time could be obtained, since condenser voltage $= B - Ri_1$. From this it would be possible to construct the characteristic of the lamp when there is no resistance in the discharge circuit.

§ 3. RESULTS

The curves of Fig. 2 show the relation between $r(i - i_1)$ and Ri_1, Fig. 2 (*a*), or ri and Ri_1, Fig. 2 (*b*), where i is the discharge current from and i_1 the charging current to the condenser.

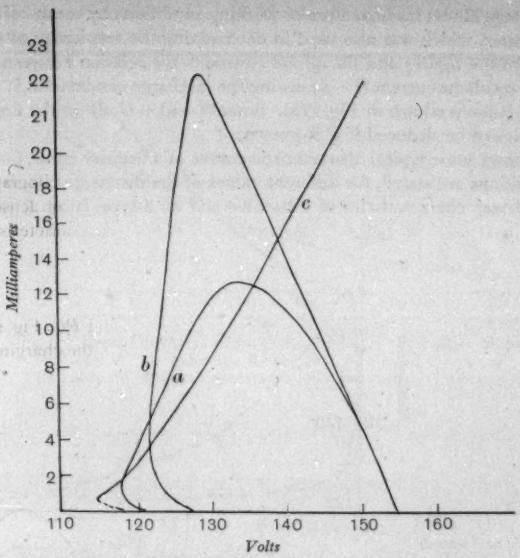


(A) Lamp beehive N: $C = 0{\cdot}015\,\mu F$; $R = 70{,}000\,\omega$; $B = 174{\cdot}4$ volts; (*a*) $r = 7600\,\omega$; (*b*) $r = 2800\,\omega$; (*c*) $r = 880\,\omega$.

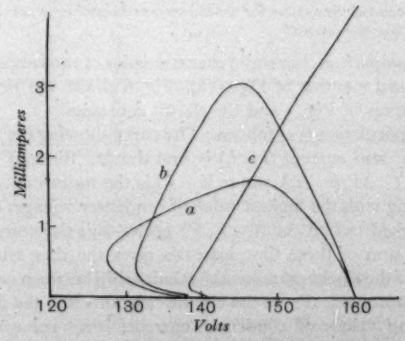
(B) Lamp I_2 (I-type): $C = 0{\cdot}0022\,\mu F$; $R = 100{,}000\,\omega$; $B = 162$ volts; (*a*) $r = 3000\,\omega$; (*b*) $r = 1850\,\omega$; (*c*) $r = 1200\,\omega$.

Fig. 4. Characteristics of neon lamp under flashing conditions.

The voltage across the lamp is $(B - Ri_1)$, Fig. 1*a*, or $(B - Ri_1 - ri)$, Fig. 1 (*b*), where B is the battery voltage and i the current through the lamp, so that if B, R, r and the sensitivity of the oscillograph are known the characteristic of the lamp under flashing conditions can readily be derived from the figures. The latter, in fact, for the arrangement of Fig. 1 (*a*), give directly the general form of the characteristic.



(A) Lamp B_5: $B=156$; $R=100{,}000\,\omega$; $r=1000\,\omega$; (*a*) $C=0{\cdot}003\,\mu F$, $n=614$; (*b*) $C=0{\cdot}05\,\mu F$, $n=72$; (*c*) Static characteristic.



(B) Lamp I_1: $B=162$ volts; $R=100{,}000\,\omega$; $r=3000\,\omega$; (*a*) $C=0{\cdot}0022\,\mu F$, $n=750$; (*b*) $C=0{\cdot}0222\,\mu F$, $n=103$; (*c*) Static characteristic.

Fig. 5. Characteristics of neon lamps: (A) beehive; (B) I-type.

The voltage B was measured under working conditions by means of an electrostatic voltmeter, which was also used in determining the sensitivity of the oscillograph. From the figures and the circuit constants the relation between condenser voltage and resultant current $(i - i_1)$ during the discharge can be found; the general form of the curve is shown in Fig. 2 (*b*). Since $(i - i_1) = C.dV/dt$ the time relations in the circuit can be deduced if C is known.

Fig. 4 shows some typical characteristic curves of a beehive lamp, for which the circuit conditions are stated, for different values of the discharge circuit resistance. Fig. 5 illustrates characteristics of a beehive and an I-type lamp at (A) and (B)

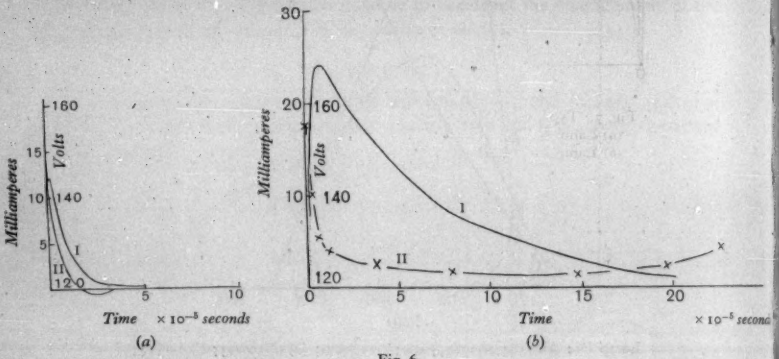


Fig. 6.

(*a*) I. Discharge-current/time curve for conditions corresponding to curve *a*, Fig. 5 (A).
II. Lamp-voltage/time curve.

(*b*) I. Discharge-current/time curve for conditions corresponding to curve *b*, Fig. 5 (B).
II. Lamp-voltage/time curve " " "

respectively. For comparison, the static characteristics of the lamps are also shown. The arrangement used was that of Fig. 1 (*b*). Fig. 6 shows the time relations calculated from the curves of Fig. 5 and the circuit constants.

The method of calculation is as follows: The curve showing the relation between condenser voltage V and current $(i - i_1)$ is first drawn. Since $(i - i_1) = C.dV/dt$ we may write $\Delta t = C.\Delta V/[i - i_1]$, where $[i - i_1]$ is the mean value of $(i - i_1)$ in the interval ΔV. Starting with the highest value of condenser voltage, small changes of voltage ΔV are marked out on the $(i - i_1, V)$ graph, and the corresponding times Δt calculated. The sum of these time-intervals gives the time which has elapsed since the initiation of the discharge; thus the relationship between condenser current and time can be determined. From the circuit constants and the figures originally traced corresponding values of condenser current, lamp voltage and discharge current are found, so that the time relations for all the quantities concerned may be traced out. The results may be checked by calculation of the quantity of electricity lost by the condenser during discharge (i) from the area of the $\{(i - i_1), t\}$ curve,

(ii) from $C(V_1 - V_2)$, where V_1 and V_2 are the highest and lowest condenser voltages recorded, respectively.

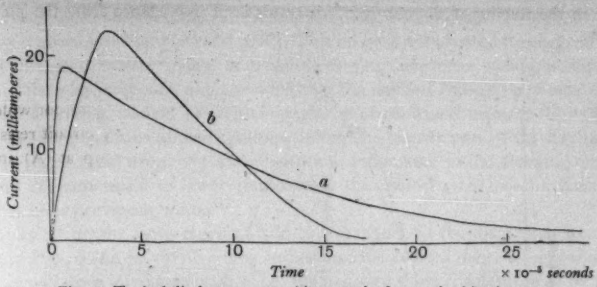


Fig. 7. Typical discharge-current/time graphs for two beehive lamps.
(*a*) Lamp *B*1: $B = 174$ volts; $R = 60{,}000\ \omega$; $r = 1000\ \omega$; $C = 0{\cdot}05\ \mu F$
(*b*) Lamp *B*6: $B = 192$ volts; $R = 70{,}000\ \omega$; $r = 1000\ \omega$; $C = 0{\cdot}05\ \mu F$

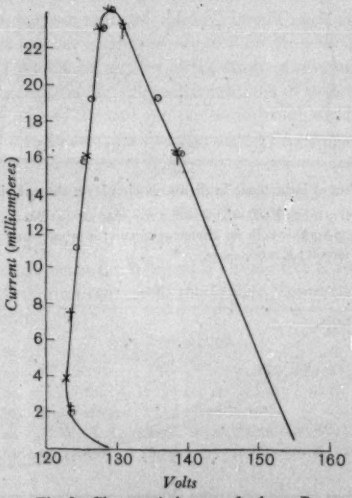


Fig. 8. Characteristic curve for lamp *B*5.

Lamp *B*5 { ⊙ $R = 100{,}000$, $n = 72$; $C = 0{\cdot}05$.
× $R = 80{,}000$, $n = 128$; $B = 156$.
† $R = 50{,}000$, $n = 192$; $r = 1000$.

Fig. 7 illustrates the difference in the current/time relationships for two beehive lamps, one, *B*6, having a cathode consisting of a more open spiral than the other, *B*1.

Fig. 8 shows that the difference, if any, in the characteristic of a lamp caused by alteration of the charging circuit resistance R is within the limits of experimental error in the tracing of the curves. This result is of importance from the point of

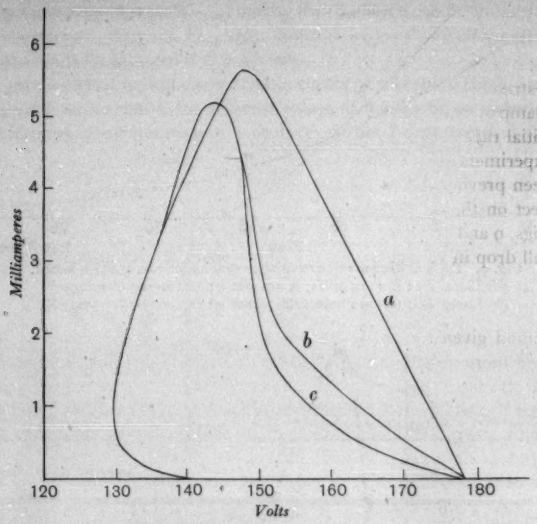


Fig. 9. Effect of inductance in discharge circuit on characteristic curve.

Lamp beehive N: $C = 0{\cdot}01\ \mu F$; $B = 178$ volts; $r = 5240\ \omega$; (*a*) No inductance in discharge circuit; (*b*) Two No. 400 Igranic coils in discharge circuit; $R = 70{,}000\ \omega$; (*c*) Two No. 400 with iron core in discharge circuit; $R = 60{,}000\ \omega$.

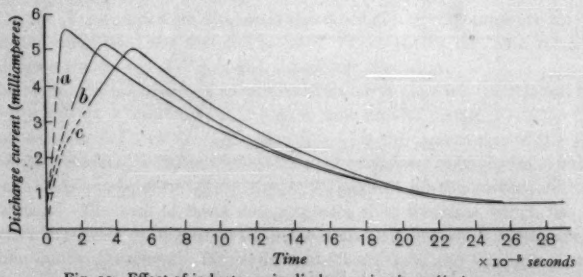


Fig. 10. Effect of inductance in discharge circuit on (i, t) curve.

view of the physiological application of the circuit, since it shows that the rate of flashing can be altered without materially affecting the quantity of electricity passing during each single condenser discharge.

It is clear from the results given, and from a large number of similar observations which have been made, that (1) The range of alteration of voltage across the lamp is decreased, and the shape of the characteristic is altered, when the condenser capacity is raised (see Fig. 5(A) and 5(B)). This range is not affected to any great extent by changes in battery voltage or in series charging resistance—at least within the limits of these experiments, and provided that the applied voltage be greater than the normal striking voltage. (2) With large values of condenser capacity the voltage across the lamp remains almost constant during the greater part of the discharge, after the initial rapid drop. (3) The presence of inductance in the charging circuit is found experimentally to lower considerably the critical resistance for flashing. This has been previously noted[4].

The effect on the lamp characteristic of inductance in the discharge circuit is shown in Figs. 9 and 10. With a large inductance the current may rise very sharply while a small drop in voltage occurs: i.e. the negative slope resistance may be very great.

§4. CONCLUSION

The method given for obtaining characteristic curves of neon lamps involves the use of no more apparatus than the flashing circuit itself and the cathode ray oscillograph. So far it has been applied only to the commercial type of lamp, but it is clear that it could readily be applied to the study of intermittent discharges in pure neon and other gases, under different conditions of pressure and disposition of the electrodes. The use of the cathode ray oscillograph avoids the errors introduced by other types of recorders, such as that used by Anthouard[5] in his work on intermittent discharges through air.

§5. ACKNOWLEDGMENTS

The experiments described have been carried out in the Physics Laboratory of the London School of Medicine for Women. I should like to express my thanks to the Council of the School for grants from the Waller Memorial Research Fund for the purchase of apparatus.

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DISCUSSION

Dr D. W. Dye. I wish to congratulate the author on the execution of a very useful piece of work which has given definite results, while her methods have shown promise of usefulness in allied investigations. I have not had much experience of discharges of this nature in neon, but some years ago I tried such tubes giving intermittent discharges to see if they could be used as sources of discontinuous

oscillations from which high-order harmonics could be selected to serve as radio frequency standards. I was compelled to discard the type of tubes then available because it was found that within one cycle the discharge presented discontinuities which were irregular, and so clouded over the precise selection of a harmonic. It appears, however, from the author's diagrams that the cycle is smooth and regular so that it is difficult to see how the results I obtained could agree with this.

Again recently I tried the use of intermittent discharges from neon tubes to serve as a stroboscopic source of illumination of interference fringes produced between two surfaces, one of which was vibrating rapidly. In this case also no sharpness of definition was possible, but whether owing to luminous persistence of the discharge or to irregular discontinuities I could not say.

Dr A. B. Wood. In her introductory remarks Dr Leyshon mentioned that the object of the experiments was to determine the wave-form of the oscillations in the neon lamp circuit. The voltage/current characteristic, whilst serving this purpose indirectly, does not appear to me to afford the most satisfactory method. Of course if a non-photographic oscillograph, such as the Western Electric type, is used, this method is the only one possible. Some years ago (1924), however, I obtained a series of cathode-ray oscillograph records of the oscillations of an Osglim (neon-filled) lamp, using the circuit shown in Fig. 11. This is the Anson-Pearson flashing circuit in which two condensers C_1, C_2 are used in series to form a potential divider for oscillograph purposes. With this arrangement voltage/time curves of the commencement of oscillations in the circuit were recorded in a single traverse of the cathode spot across a photographic plate in the vacuum chamber of the oscillograph. Such records are shown in Fig. 12. The frequency of oscillation of the circuit in these cases varied from 1000 to 3000~. The following points are of interest: (1) The voltage across the lamp at first rises (during the condenser charge) to a high striking potential, at which point it suddenly falls, recovers, and continues to oscillate. The mean voltage of the sustained oscillation is, as would be expected, considerably lower than the striking voltage. (2) The higher the frequency the smaller the voltage-amplitude of the oscillations. Ultimately it was found that the lamp would not oscillate at frequencies of the order of 100,000 ~ in the circuit shown in Fig. 1. (3) As Dr Dye has already suggested, the initial rise of voltage, before the lamp begins to glow, is not always regular. Some of the records show distinct irregularities in the rising voltage. (4) The wave form is typical of B. van der Pol's "relaxation" oscillations—a succession of slow rises and sudden falls of voltage. I cannot understand why the previous speaker failed to observe the presence of harmonics in such a wave-form as that indicated in the records.

The President. The use of the cathode ray oscillograph in examining intermittent discharges might help in the elucidation of some of the problems of conduction of electricity through gases left unsolved by the more orthodox modes of experiment. I understand that Dr Leyshon is proceeding with these investigations but employing simpler electrode systems, more amenable to calculation than those in the commercial forms of neon lamp.

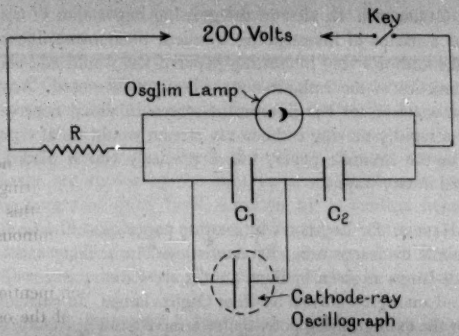

Fig. 11. Anson-Pearson flashing circuit—oscillograph recording voltage/time characteristic.

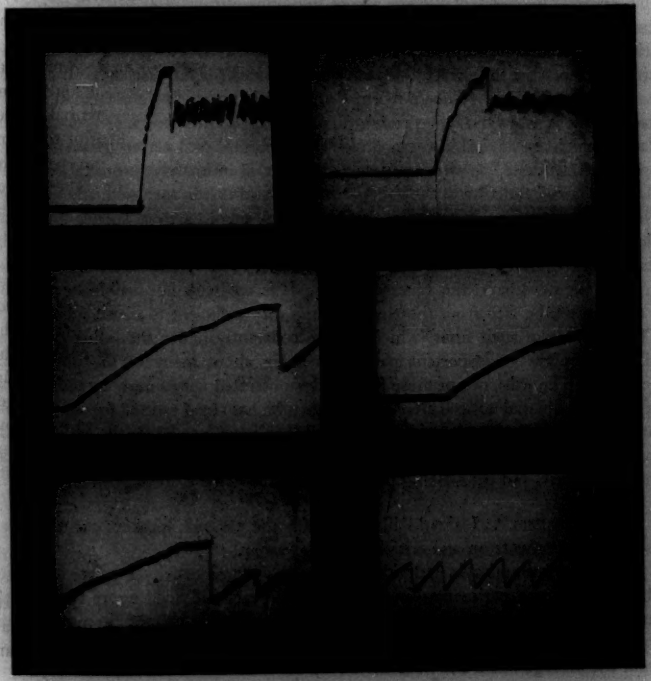

Fig. 12.

Mr L. G. CARPENTER. In view of the growing importance of the cathode-ray oscillograph as a means of investigating transient phenomena, it would enhance the value of the author's very interesting paper if she would add some details as to the characteristics of the cathode-ray oscillograph she used. Any information as to the best material for the fluorescent screen in visual observations of the spot made by a rapidly moving cathode ray stream would be of especial interest. With regard to the bromide paper, where precisely was it placed, and was it specially treated in any way?

Mr N. L. HARRIS. Dr Leyshon's interesting paper sets out a useful method of obtaining dynamic discharge tube characteristics. The striking and extinguishing potentials of the lamps as given by Figs. 4 and 5 are some 15 or 20 volts lower than it is usual to find amongst a normal batch of Osglim lamps. In particular, the value of 115 volts for the extinguishing potential of lamp $B5$ in Fig. 5 (A) is very unusual: possibly the readings of the electrostatic voltmeter are subject to a correction of this order. The constant voltage across the lamp during discharge under certain conditions is probably due to the fact that the cathode fall of potential is "normal," with the cathode incompletely covered with glow. It is well known that in these circumstances the tube voltage is almost independent of the discharge current. In connection with Dr Leyshon's recent demonstration of periodic movements of the cathode glow, and Dr Dye's remarks upon the apparently irregular growth of the flash, it might be advisable to emphasise the important part played by surface films on the electrodes. The striking of the discharge is intimately associated with the electronic work function of the cathode surface, and this in turn with any adsorbed films of monomolecular nature at this surface. A small patch of such a film of an electropositive element is sufficient to initiate a local discharge at a lower potential than that required for the clean electrode surface, and small traces of an electronegative element have the reverse action. It is wellnigh impossible to maintain a discharge tube free from the traces of impurity sufficient to cause such effects. The presence of such films, which change constantly under the influence of the discharge, plays an important part in bringing about many of the phenomena described. The existence of these films is demonstrated very simply by the increase of striking potential noticed after a discharge tube has stood unused for some hours. An adsorbed film of gaseous impurities will then have formed on the cathode, and the striking voltage will be raised. The passage of the discharge cleans the surface, in part at least, and the striking potential falls.

AUTHOR'S REPLY. I should like to thank the various speakers for their encouraging comments on and criticisms of the paper.

The discontinuities in individual discharges mentioned by Dr Dye were not observed in my experiments—no doubt because each recorded figure represented an average of some thousands of discharges. The distinctness of the figures seen showed, however, that the variation of the greater number of individual discharges from the average could not have been very marked—at least after the initial rapid

rise of current. It is possible that the resistance in the discharge circuit may have had a stabilising effect.

The original object of the investigation was to obtain some idea of the nature and duration of the condenser discharges through the neon lamp, these repeated discharges having proved to be a suitable form of stimulus for excitation of nerve preparations.

In Dr Wood's interesting records the voltage changes across the condenser during discharge are shown by the faint lines, which illustrate the difficulty in recording discharges of such brief duration by a method involving a single traverse of the oscillograph spot across a photographic plate.

The explanation for the change in striking voltage which is shown so clearly in Dr Wood's records has been given by Mr Harris in his contribution to the discussion.

The electrostatic voltmeter I used gives readings which correspond closely with those of a Weston instrument and of a Cambridge Instrument Company's voltmeter. The lamp B_5 for which records are given in Fig. 5 (A) certainly has a lower minimum voltage than any other lamp which was examined. I have lately redetermined the minimum voltage for this particular lamp, and have confirmed previous observations. The shapes of the characteristics of this lamp are, however, like those of other beehive lamps.

In reply to Mr Carpenter, the cathode ray oscillograph used was of the standard Western Electric type (No. 224 A). Its sensitivity was about 1 mm. movement of the oscillograph spot for 1 volt potential-difference between the deflecting plates. The bromide (gas light) paper used was not specially treated. It was held down on the top of the oscillograph tube for about half a minute. This method of obtaining photographic records with bromide paper was described by Dr Dye in a paper read before the Physical Society*.

* *Proc. Phys. Soc.* 37, 158 (1925).

A METHOD OF CALCULATING THE NUMERICAL EQUATION OF STATE FOR HELIUM BELOW 6° ABSOLUTE, AND OF ESTIMATING THE RELATIVE IMPORTANCE OF GAS DEGENERACY AND INTERATOMIC FORCES

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ABSTRACT. Gas "degeneracy" (the effect of non-Maxwellian distribution of molecular velocities) is distinguished from gas "imperfection" (the effect of intermolecular forces), and it is shown that specific heat measurements cannot be used to verify theories of the former, unless account is taken of the very rapid increase of the latter at the lowest temperatures and highest pressures. Accordingly a thermodynamic method is used to calculate the sum of degeneracy and imperfection at 4° and 5° Abs. for helium. Theories of degeneracy can then be roughly tested by the extent to which they allow a residual value of the imperfection to be calculated, and determine a consistent extension of the course taken by isothermals down to 15°. The method is applied to Fermi's expressions for degeneracy, and also to Berthelot's equation of state. One of the equations alone satisfies the required condition; it is shown that on this basis degeneracy would comprise 15 per cent. of the total departure of helium from the ideal gas laws at 4° and 5°, the remainder being due to true imperfection or intermolecular forces.

§ 1. STATEMENT OF THE PROBLEM

Let the ideal gas be defined, for the present purposes, by its obedience to the equation

$$p = n\kappa T \qquad \ldots\ldots(1),$$

where p is its pressure in dynes/cm.^{2}, T its absolute temperature, n the number of molecules per c.c., and κ is $1{\cdot}372 \times 10^{-16}$.

Any actual gas departs from this law owing to the finite size of its molecules and the forces of attraction and repulsion between them, and we follow normal usage in referring to this departure as the "imperfection" of the gas. But for many years in thermodynamics (Nernst's heat theorem) and more recently in kinetic theory (the inclusion of translatory motion under quantum restrictions) it has seemed probable that even a gas which had no imperfection in the above sense would nevertheless still show departure from law (1) at high pressure and low temperature. In statistical mechanics this accompanies a departure from Maxwellian distribution of molecular velocities, and has been called the "degradation" or "degeneracy" of a gas.

To include both the imperfection and the possible degeneracy of a gas, its true equation of state would have to take the form

$$p = n\kappa T\{1 + F(T, p) + f(T, p)\} \quad \ldots\ldots(2),$$

where $F(T, p)$ is a function of temperature and pressure representing the effect of molecular dimensions and forces, and $f(T, p)$ represents the effect of departure from Maxwellian statistics.

For brevity we shall refer to these two functions as A and B respectively.

The very large number of empirical equations of state which have from time to time been proposed, beginning with that of Van der Waals, deal exclusively with A, and there is no doubt that B is only likely to become important under the most extreme conditions of pressure and temperature realisable in practice. Attempts to verify the predictions of degeneracy under those conditions made by thermodynamic and kinetic theory have mainly been based on the following argument. Even the most complex equation of state must satisfy the general relations

$$E = p/\tfrac{2}{3}n,$$
$$C_v = (dE/dT)_v,$$

where E is the energy of the n molecules and C_v the specific heat at constant volume. Hence degeneracy must affect C_v, and anomalies discovered in calorimetry may give information about B if the substance can be made to remain in the gaseous phase at low enough temperature. Hydrogen and helium obviously present the most favourable cases, because of their comparatively slight imperfection and consequently low boiling points. At temperatures so low that the contribution of rotational and vibrational degrees of freedom to the specific heat have vanished, both these gases should, in the ideal state, obey the law

$$C_v = \tfrac{3}{2}\kappa.$$

The measurements of Eucken[1], and more recently of Meissner[2], reveal a distinct fall below this value for both gases at very low temperatures, and hence these experiments have often been quoted as experimental verification of degeneracy.

For hydrogen this interpretation is complicated by the recently found difference between the rotational specific heats of the ortho- and para-molecular varieties, at temperatures just above that at which those quantities vanish[3]. However, it is true that any survival or anomalous excitation of rotational degrees of freedom could only increase C_v, and so cannot be responsible for the facts; further, all theories of degeneracy do predict a tendency in the observed direction by their requirement that

$$C_v \to 0 \quad \text{as} \quad T \to 0.$$

Nevertheless, for any such experiments to afford a convincing test of theoretical forms of B, or even a decision as to whether B is large or small under the most extreme practical conditions, the course taken by the function A would have also to be known in this region.

Such a requirement is not easy to fulfil; we shall indicate below some inconsistences produced by extrapolating, to temperatures at which degeneracy is likely

to be important, empirical equations of state known to represent A satisfactorily under less extreme conditions. We are left with the suspicion that effects attributed to B may be due to an unknown increase in A, and any independent clue as to the relative importance of imperfection and degeneracy becomes worthy of enquiry, Hence in the present paper the problem of the possible place of degeneracy in experiment is investigated from a different standpoint, as follows.

§ 2. METHOD OF THE PRESENT PAPER

A method is devised whereby the vapour pressure and latent heat data of the Leiden experimenters are made to yield values of $(A + B)$ through the latent heat equation of thermodynamics. Now direct measurement of helium isothermals has been carried down to 15° Abs. by the Berlin Reichsanstalt. From their data we plot a curve of the departure of the gas from the ideal law between 90° and 15°. This represents the course of $(A + B)$, but it is very probable that, even at 15° and the given pressure, B is small compared with A, and that the curve substantially represents the course of A. We show that the further points secured by our method at 4° and 5° lie on a smooth continuation of this graph of $(A + B)$. But whereas B was probably negligible at 15°, it is not necessarily so at the lower temperatures. Accordingly it becomes of interest to substract from our values of $(A + B)$ any given theoretical value of B; it is then possible to see at once whether the residual value of A is or is not still consistent with the course entered upon by $(A + B)$ at the higher temperatures for which B was negligible. We can in this way make a rough test of any theory of degeneracy, by requiring that the value of A which it extracts from our $(A + B)$ must not lie in such a part of the diagram as to bring obvious discontinuity into the course of that function.

This indirect test is capable of eliminating impossible theories rather than of distinguishing between those which satisfy the conditions, since one extrapolation can be shown to be wrong when another cannot be proved to be right. It does not, however, involve the assumption that an equation of state has equal validity on both sides of the critical temperature, and thus it may have some advantage over the only alternative method by which theories of degeneracy can be compared with experiment.

The method is applied to Fermi's two expressions for degeneracy. A similar test is made of the validity of Berthelot's equation of state in this region.

§ 3. TOTAL DEPARTURE FROM GAS LAWS AT 4·29° ABS. AND 5·20° ABS., DERIVED BY INDIRECT METHOD

If equation (2) be written in the form

$$p = n\kappa T\,(1 + A + B),$$

where A and B are the imperfection and degeneracy functions of T and p, the total $(A + B)$ can be calculated if pressure and density and atomic mass are known, since we have

$$n = \rho/m.$$

Consider the latent heat equation

$$L = (1/\rho_V - 1/\rho_L)\, T\, (dp/dT),$$

where L is the sum of external and internal latent heats, ρ_V and ρ_L are densities in the vapour and liquid phases respectively, and p is the saturated vapour pressure at temperature T. Kammerlingh Onnes[4] determined the density of liquid helium at 4·29° Abs., and the latent heat of evaporation at the same temperature[5]. From the vapour pressure experiments of Onnes and Weber[6] we construct a graph giving us the pressure and pressure gradient at that temperature. From all these sources we obtain the following:

$$\begin{aligned} T &= 4{\cdot}29°, \\ L &= 22 \text{ cal./gm. mol.} \\ &= 2{\cdot}30 \times 10^8 \text{ erg./gm.}, \\ \rho_L &= 0{\cdot}122 \text{ gm./c.c.} \\ dp/dT &= 9{\cdot}37 \times 10^5 \text{ dyne/cm.}^2 \text{ per degree,} \end{aligned}$$

whence we calculate from the latent heat equation

$$\rho_V = 0{\cdot}0152 \text{ gm./cc.},$$

a result which is not inconsistent with the statement from the other cryogenic laboratory[7] that at a slightly higher temperature the density of the vapour is about one-tenth of that of the liquid helium.

For the mass of the helium atom we have

$$m = 6{\cdot}56 \times 10^{-24} \text{ gm.}$$

For Boltzmann's constant

$$\kappa = 1{\cdot}372 \times 10^{-16}.$$

Hence at 4·29°

$$n\kappa T = 1{\cdot}361 \times 10^6.$$

From the curve plotted with the data of Onnes and Weber, at this temperature,

$$\begin{aligned} p &= 810 \text{ mm. Hg.} \\ &= 1{\cdot}082 \times 10^6 \text{ dynes/cm.}^2. \\ \therefore A + B &= -0{\cdot}204. \end{aligned}$$

The total departure from the ideal gas law can again be calculated at 5·20° Abs. This is the critical temperature at which L vanishes. But, using the critical coefficient for helium, we have

$$\begin{aligned} \kappa T_c/p_c V_c &= 3{\cdot}13, \\ p_c &= 1718 \text{ mm. of mercury} \\ &= 2{\cdot}29 \times 10^6 \text{ dynes/cm.}^2. \\ \therefore V_c &= 99{\cdot}4 \times 10^{-24} \text{ cc.} \end{aligned}$$

This is the critical volume for a single atom, since we have used the above "atomic" value for κ. We accordingly combine it with the above value of the atomic mass and obtain

$$\rho = 0{\cdot}066 \text{ gm./c.c.}$$

Hence at 5·20°

$$n\kappa T = 7{\cdot}20 \times 10^8.$$

$$\therefore A + B = -0{\cdot}682.$$

§ 4. TOTAL DEPARTURE FROM GAS LAWS AT HIGHER TEMPERATURES

These indirect determinations of the total departure from the ideal state at 4·29° and 5·20° must be compared with direct determinations at the lowest temperature available. In 1926 Holborn and Otto[8] at the Reichsanstalt investigated isothermals for helium down to 15° Abs.; we calculate from their data the course taken by $(A + B)$ at the two pressures we have been considering, obtaining two curves over the range 90° to 15°. Beyond this range no results are obtainable except by indirect methods. The positions of points representing our values at 4° and 5° are therefore of some interest; we enquire later whether their fit on to the curves is improved or spoilt by assuming any given theory of degeneracy.

Below 60° Abs. the change in the helium isothermals is so rapid that considerable care has to be taken in choosing any way, graphic or algebraic, of enabling their course to be shown without ambiguity. The usually adopted method of plotting a first-order correction term to Boyle's law is not adequate to this case. We take (Table 1) the data of Holborn and Otto from their empirical expression,

$$pv = A + Bp + Cp^2 + Dp^3 + Ep^4.$$

Table 1. Coefficients in Holborn and Otto's equation.

Absolute temperature	A	$B \times 10^3$	$C \times 10^6$	$D \times 10^6$	$E \times 10^9$
90·1	0·32992	0·62286	0·735	—	—
65·1	0·23847	0·55080	2·384	−0·0141	—
20·3	0·07460	−0·16424	18·529	−0·1105	—
15·1	0·05558	−0·79740	54·370	−0·7513	3·796

After unsuccessful attempts to make an unambiguous extrapolation of each coefficient separately, logarithmically and otherwise, it was found more definitive to calculate the integrated value of pv and subtract it from the normal value of the ideal gas. We plot the differences $(pv - RT)$ as a function of temperature for the two conditions with which we have been concerned, namely, 810 mm. and 1718 mm. The result, transferred into the units of Holborn and Otto, is shown in Fig. 1, the unit of pressure being one metre of mercury, while the unit of volume is the volume of one gram of gas at that pressure and 0° C.

From the numerical equation of state which we have derived at the lower temperatures, we determine, transferring our values to the units of Holborn and Otto,

comparable expressions for $(pv - RT)$ at 4·29° and 5·20° and the pressures 810 mm. and 1718 mm.

It is seen that these points (+ in Fig. 1) fit a not unreasonable extension of the curves beyond the lowest temperatures at which direct data are available. Since the curvature of both graphs is increasing rapidly at 15°, more than one set of values at 4° and 5° might be regarded as agreeing with their extrapolation. Here again logarithmic plotting was not found to remove possible ambiguity, and the rigorous identification of any extrapolation would require an empirical equation involving

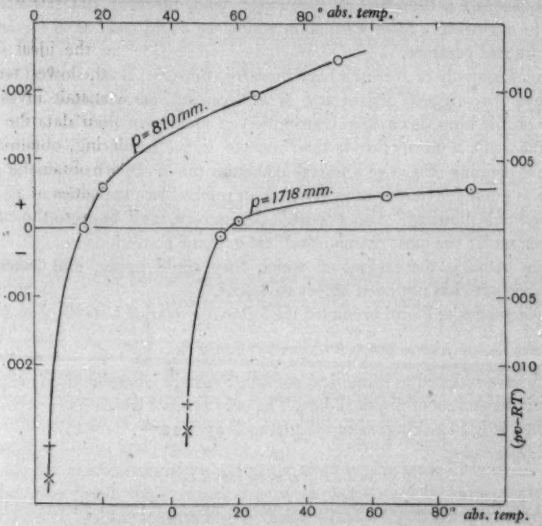

Fig. 1. Gas imperfection, degeneracy and temperature.

higher order terms to which no clue is available at less extreme temperatures. Hence arguments from these curves are only valid in particular cases where decision has to be made between very divergent alternatives.

So long as this restriction is regarded, we can say that, if a single curve from 4° to 90° represents $(A + B)$ and over the upper half of it B is negligible, a broad limit can at once be set to the allowable degeneracy. Either B at the lowest temperature must be small enough not to alter our extension of $(A + B)$ appreciably, or else if B is large A must be supposed to have increased, with opposite sign, enough to prevent their algebraic sum from departing noticeably from the curve. In the particular case discussed below, the numerical values are such as to permit decision between the alternatives.

§ 5. THEORETICAL VALUES OF DEGENERACY

We turn to theoretical expressions for the function B, of which the basis has been twofold.

(*a*) Degeneracy arises from the application of Nernst's heat theorem to non-condensed systems, and is there inseparable from Nernst's view of the existence of zero-point energy. Treated thermodynamically, the most complete exploration of its consequences has been that of Bennewitz, who, however, did not succeed in deriving in quantitative form a function which we could treat as B for any finite temperature and pressure.

(*b*) Independently of Nernst's heat theorem, Einstein, Bose, Dirac, Fermi and others have investigated degeneracy as a necessary consequence of statistical theorems on the non-Maxwellian distribution of molecular velocities. Confidence in these theorems is mainly due to their success, in the hands of Sommerfeld and others, in explaining otherwise intractable phenomena in other branches of physics. We select as a case for treatment by the present method two approximations derived by Fermi[9] for "strong" and "weak" degeneracy, and expected to describe phenomena under the most extreme and less extreme conditions respectively. The border-line between the regions to which they could apply, and indeed their validity anywhere, has not been otherwise tested.

Strong degeneracy: Fermi predicted the following relation between p and n, κ, T,

$$p = 0{\cdot}077\, h^2 n^{5/3}/m + 7{\cdot}6\, m\, n^{1/3}\kappa^2 T^2/h^2,$$

in which I have reduced to numerical constants certain elements in his expression which are independent of physical data. The only symbol which has not previously been defined is h, Planck's constant, equal to $6{\cdot}55 \times 10^{-27}$.

Weak degeneracy:

$$p = n\kappa T\{1 + h^3 n/16\,(\pi m\kappa T)^{3/2}\}.$$

We insert in these equations numerical values appropriate to the cases we have considered, namely,

$$T = 4{\cdot}29^\circ \qquad \rho = 0{\cdot}0152,$$
$$T = 5{\cdot}20^\circ \qquad \rho = 0{\cdot}066,$$

and compute the degeneracy function for an otherwise perfect gas in the form

$$p = n\kappa T\,(1 + B).$$

If weak degeneracy occurs,

$$B = +\,0{\cdot}031 \quad \text{at } 4{\cdot}29^\circ$$
$$= +\,0{\cdot}10 \quad \text{at } 5{\cdot}20^\circ.$$

If strong degeneracy occurs,

$$B = +\,3{\cdot}48 \quad \text{at } 4{\cdot}29^\circ$$
$$= +\,1{\cdot}30 \quad \text{at } 5{\cdot}20^\circ.$$

§ 6. INFERRED VALUES OF "IMPERFECTION"

Subtracting these theoretical values of B from the values of $(A + B)$ determined by our indirect method, we find that:

weak degeneracy being assumed,

$$A = -0{\cdot}235 \quad \text{at } 4{\cdot}29°$$
$$= -0{\cdot}78 \quad \text{at } 5{\cdot}20°;$$

strong degeneracy being assumed,

$$A = -3{\cdot}68 \quad \text{at } 4{\cdot}29°$$
$$= -1{\cdot}98 \quad \text{at } 5{\cdot}20°.$$

Applying the method of test outlined previously, we see that strong degeneracy, if existent in this case, would cause the imperfection to be 18 times and 3 times the *total* departure from the ideal law at 4° and 5° respectively. Now the rates of increase of the curves drawn from the data of Holborn and Otto, regarded as substantially graphs of A, are such that our values might still appear as extrapolations thereof, even after adjustments by, say, 50 per cent. or even 100 per cent. But adjustment by factors of 3 and 18 would introduce into them a character having no connection at all with their courses at 15°. Further, it would involve the arbitrary assumption that our values of $(A + B)$ are due to a remarkable coincidence, the degree of neutralisation of $+3{\cdot}48$ by $-3{\cdot}68$ having the same proportional effect on the one curve as the degree of neutralisation of $+1{\cdot}30$ by $-1{\cdot}98$ had on the other. This would be a very forced explanation, compared with the simple one that B is small compared with A. Again, if strong degeneracy held at 4° and 5°, the above figures show that it would make the value of A at 810 mm. much greater than at 1718 mm. Reference to the curves, which are on suitably different scales because of the greater order of magnitude of A at 1718 mm. than at 810 mm., shows the extreme unlikelihood of this being so. The higher-pressure curve increases much faster than the lower, and we have no right to demand that they should converge again, since the mechanism of imperfection must be the same in both.

On the other hand, the expression for weak degeneracy introduces a correction to $(A + B)$ which is more convincing, in that it enables the Holborn and Otto curves to be extended in the same relationship to one another which they had already shown. The rate of increase of B relative to that of A is then similar on each curve. In fact at each pressure the degeneracy amounts to about 15 per cent. of the total departure from the behaviour of an ideal gas. The degeneracy correction to the pressure is about 3 per cent. and 10 per cent. at 4·29° and 5·20°, the pressures being rather over one and two atmospheres, while for the imperfection correction the corresponding figures are of the order of 20 per cent. and 80 per cent. and of opposite sign. In Fig. 1 the points × represent A, assuming weak degeneracy.

Owing to this opposite sign, if conditions are reached at which the strong approximation becomes valid, we may expect a cancellation of imperfection by degeneracy, and a second "Boyle" point, at which again the behaviour approximates to that of the ideal gas through the condition

$$A = -B.$$

§7. EXTRAPOLATION FROM BERTHELOT'S EQUATION

It has been stated above that in this type of problem it is unsafe to assume that any equation of state is valid when approaching the absolute zero, however well it may fit the facts in another range. It is of interest to see what results would be obtained if this precaution were neglected and A calculated by extrapolating some equation known to be a good approximation at temperatures high compared with the critical temperature. Berthelot's equation is the most successful of these; it may be stated in the form,

$$pv = RT\left\{1 + \frac{9}{128}\frac{T_c}{T}\frac{p}{p_c}\left(1 - \frac{6T_c^2}{T^2}\right)\right\}.$$

We calculate the values of A corresponding to those already deduced, inserting the appropriate values of T_c and p_c and taking $T = 4{\cdot}29$ and $5{\cdot}20$ and $p = 810$ mm. and 1718 mm., and obtain

$$A = -0{\cdot}314 \quad \text{at } 4{\cdot}29^\circ$$
$$= -0{\cdot}351 \quad \text{at } 5{\cdot}20^\circ.$$

As might be expected for an equation designed for less extreme circumstances, this predicts a less rapid increase of departure from the ideal law than that found by the indirect method, whether degeneracy is assumed or neglected.

§8. CONCLUSION

Since an unknown increase of gas imperfection at very low temperatures might be responsible for calorimetric effects attributed to gas degeneracy, the need arises for indirect methods of checking theories of degeneracy and of estimating the numerical importance which any of them would bear, relative to imperfection, in the most general equation of state. The method outlined in the preceding sections allows the total departure from ideal gas laws to be computed from vapour pressure and latent heat data for helium at 4° and 5° Abs.

In comparison, Berthelot's equation of state fails to give a sufficiently rapid increase of imperfection. Fermi's "strong" approximation to the degeneracy function also fails to justify the assumption that it can occur at these temperatures and pressures, since it would require too rapid an increase in the imperfection function, and would require the more rapid increase to take place at the lower pressure. On the other hand, Fermi's "weak" approximation provides a degeneracy consistent with curves constructed from direct data above 15° and indirect data below 15°.

The proof that Fermi's weak degeneracy is not inconsistent with the known facts does not, of course, establish its existence. If, however, it is regarded as worth consideration on this basis, we show that the share taken by degeneracy and imperfection would be as follows:

at $4{\cdot}29^\circ$ and 810 mm. $\quad p = n\kappa T\{1 - 0{\cdot}235 + 0{\cdot}031\}$,

at $5{\cdot}20^\circ$ and 1718 mm. $\quad p = n\kappa T\{1 - 0{\cdot}78 + 0{\cdot}10\}$,

where the two last terms represent respectively the effect of intermolecular forces and of non-Maxwellian distribution of molecular velocities.

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DISCUSSION

Prof. W. Wilson. I should like to ask the author how these low temperatures have been estimated. I had the impression that some equation of state such as that of van der Waals was assumed at the outset in order to determine them. It is interesting to note that Sommerfeld has recently applied the new statistical methods to the electron theory of the conduction of heat and electricity in metals. The classical theory gave far too big a specific heat to the metal. This is now corrected, I believe, by the degeneracy of the electron gas in the metal.

Mr J. H. Awbery. Professor Wilson has mentioned the use that Sommerfeld made of the theory of degeneracy. If I remember correctly, Sommerfeld supported his argument that electron gas should be degenerate at all temperatures known terrestrially, by comparing its degeneration quantitatively with that of hydrogen and helium as found experimentally (using the well-known molecular weight and temperature relation). His only exact evidence of degeneracy at that time was, of course, from specific heat data which, as mentioned in Mr Johnson's paper, give results that can only be attributed to degeneracy and not to imperfection. The fact that such uses have been made of the theory only increases the importance of an estimate such as Mr Johnson has made, and I should like to congratulate him on getting so much out of such scanty data.

Prof. J. E. Lennard-Jones. The new statistical theories developed by Bose and Einstein on the one hand, and Dirac and Fermi on the other, diverge most from classical statistics at low temperatures or high pressures, and calculation shows that it is only in the case of helium near its critical point that its behaviour is likely to be perceptibly different according as it obeys the classical, or the Bose-Einstein, or the Fermi-Dirac statistics. It is, therefore, a matter of great theoretical interest to determine the deviations of helium from the perfect-gas law near its critical point and to analyse the effect of the statistics and of the interatomic fields. Mr Johnson has made an interesting attempt on this problem, but there are one or two points in his treatment which seem to me to require investigation in any further work.

In the first place, the assumption is made that the effects of interatomic fields and of the statistics are simply additive, equation (2). As, however, the calculation

of the effect of interatomic fields must itself depend on the statistics assumed, this simple separation of the two effects requires justification. In the second place, the author considers only the Fermi-Dirac statistics, whereas theory indicates that helium atoms should obey the Bose-Einstein statistics. It would add to the value of his work if the author could consider the effect of the latter statistics on helium near the critical point.

AUTHOR'S REPLY. Gas thermometry, at temperatures where imperfection and degeneracy are in question, is still accurate to a first approximation if the thermometric gas is at low pressure: this condition can be maintained while the gas whose temperature is measured may be at high pressure. Nevertheless, the question of Prof. Wilson and the first point raised by Prof. Lennard-Jones force us to recognise two or more stages of approximation in these problems. First assume that the gas laws hold and from the temperature readings deduce the amount of the consequences in imperfection and degeneracy. An equation of state can thence be constructed which may be used to correct the original temperature reading. With this corrected temperature a slight adjustment of the imperfection and degeneracy may then be made, according to the method, familiar in other branches of physics, of assuming that a certain error is zero in order to obtain a first approximation to its finite value, and using this to remove the assumption in a second approximation. Similarly, to the first approximation the imperfection and the degeneracy must be regarded as additive in order to determine their order of magnitude. Now that Leiden and Berlin are both obtaining data at the same low temperatures, the second approximation suggested by Prof. Wilson's remarks may soon be practicable, though I think the data are not yet accurate enough for the other second approximation which Prof. Lennard-Jones points out as ultimately necessary.

THE MAGNETOSTRICTION CONSTANT FOR ALTERNATING MAGNETIC FIELDS

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ABSTRACT. A magnetostriction constant K for the Joule effect is defined by the equation $p = KH$ where p is the alternating mechanical stress produced by a small alternating magnetic field H superposed on a steady magnetic field H_0. It is shown that the alternating intensity of magnetisation I produced by an alternating strain $\delta l/l$ is given by the equation $I = K\delta l/l$, K being the same constant in both equations. These equations are used to calculate the motional impedance of a laminated ring toroidally wound and vibrating in its fundamental radial mode. K has been measured for various values of H_0 with a ring of the alloy Fe 60 per cent., Ni 40 per cent. It is shown that the magnetostriction effect for steady magnetising fields can be inferred from the measurements with alternating magnetic fields, if the reversible permeability and the differential permeability of the alloy are known.

§ 1. INTRODUCTION

The need for accurate and constant substandards of frequency has led to the use of oscillating valve circuits controlled by very resonant mechanical vibrators. The valve-maintained tuning fork* is available for sonic frequencies and the piezo-electric crystal † for radio frequencies. These vibrators are not convenient for frequencies between 5000 and 25,000 ~ and Pierce‡ has developed the magnetostriction oscillator to cover this range. The theory of the valve-maintained tuning fork has been given by Butterworth§ and others and that of the piezo-electric crystal by Dye‖ on the assumption that the vibrators can be considered as mechanical systems having one degree of freedom. This assumption is equally valid for the magnetostriction oscillator, and the theory appropriate to it must be closely analogous to that for the tuning fork and the piezo-electric crystal. It is essential in the three cases to know the nature and magnitude of the coupling between the mechanical vibrator and the electric circuit. This coupling is defined by the piezo-electric equations and the piezo-electric constant for the crystal and there must be similar equations and a similar constant for the magnetostriction oscillator. It is the object of this paper to define this magnetostriction constant and to show how it can be measured.

* W. H. Eccles, *Proc. Phys. Soc.* **31**, 269 (1919).

† W. G. Cady, "Piezo-electric standards of high frequency," *Journ. Opt. Soc. of America*, **10**, 475 (1925).

‡ G. W. Pierce, "Magnetostriction Oscillators," *Amer. Acad. Proc.* **63**, 1–47 (1928).

§ S. Butterworth, "The maintenance of a Vibrating System by means of a Triode Valve," *Proc. Phys. Soc.* **32**, 345 (1920).

‖ D. W. Dye, "The Piezo-electric Resonator and its Equivalent Electrical Circuit," *Proc. Phys. Soc.* **38**, 399 (1926).

The magnetostriction oscillator used by Pierce* and Vincent† is generally a thin rod and the coupling to the electrical circuit is obtained by placing it in a solenoid carrying a small alternating current superposed on a steady magnetising current. The stresses arising from the magnetostriction effect set the rod into resonant vibration in its fundamental longitudinal mode, with a node at the centre and antinodes at the ends. The mechanical stress is greatest at the centre and falls to zero at the ends, with the result that the intensity of magnetisation must vary along the rod. There is a further complication due to the demagnetising effect of the magnetic poles appearing at the ends of the rod. The theory is much simplified by consideration of a magnetostriction oscillator in the form of a closed circular ring which is magnetised by a uniform toroidal winding. A ring has a mode of vibration in which all parts of it move radially with the same amplitude and phase and, if this mode is excited by the passage of an alternating current of suitable frequency through the toroidal winding, the magnetisation and the mechanical stress cannot vary along the circumference of the ring. There is, however, a variation in these quantities over the cross-section of the ring, since eddy currents are induced in it. If the ring can be laminated this variation can be much reduced and the stress and magnetisation become practically uniform throughout the material. Before investigating the motion of the ring and its reactions on the electrical circuit, it is necessary to define the magnetostriction constant in a form suitable for the study of alternating effects.

§ 2. THE MAGNETOSTRICTION EQUATIONS

The Joule effect is usually defined as the steady mechanical strain produced by the application of a steady magnetic field. This definition is inadequate for a study of the alternating strains produced by a small alternating magnetic field superposed on a steady magnetic field, for these strains depend on the frequency of alternation and the possible resonance of the vibrating body as well as on its magnetostriction properties. It is preferable to define the Joule effect as a relation between the alternating magnetic field and the alternating mechanical stress produced by it. If the laminated ring described above is imagined to be rigidly constrained so that no motion is possible, the alternating magnetic field from the toroidal winding will produce the same alternating mechanical stress at all frequencies and we may write

$$p = KH \qquad \text{.....(1)}$$

where p is the stress in dynes/cm.^{2} H (or $H_0 \sin \omega t$) is the alternating part of the magnetic field in gauss, w (or $2\pi \times$ frequency) is the pulsatance and K is a magnetostriction constant. There is a steady stress, produced at the same time by the steady magnetising field, with which we are not concerned. On removal of the constraints, the ring vibrates and we may regard the stress p as the force maintaining the vibration. The equation of motion can now be derived in the usual form for a vibrating system with one degree of freedom. Let ξ, $\dot{\xi}$ and $\ddot{\xi}$ represent the instantaneous

* *Loc. cit.*

† J. H. Vincent, "Experiments on Magnetostrictive oscillators at Radio Frequencies," *Proc. Phys. Soc.* **41**, 476 (1929); *Nature*, **120**, 952 (1927); *Electrician*, **101**, 729 (1928); **101**, 11 (1929).

outward radial displacement, velocity and acceleration at time t. Let r be the radius, σ the mean density and S the cross-section of the ring and let C be the velocity of sound in it and E the modulus of elasticity defined by the equation $C^2 = E/\sigma$. The circumferential stress p is equivalent to a total force, tending to expand the ring, of $2\pi Sp$ or $2\pi SKH$. The equation of motion is therefore

$$2\pi SKH = 2\pi rS\sigma\ddot{\xi} + k\dot{\xi} + 2\pi SE\xi/r \qquad \ldots\ldots(2),$$

where the terms on the right-hand side represent in the order given the forces necessary to overcome the inertia, the damping and the stiffness. The symbolic solution of this equation for continuous and steady motion is

$$\dot{\xi} = \frac{2\pi SKH}{k + jw\,(2\pi rS\sigma - 2\pi SE/r\omega^2)} \qquad \ldots\ldots(3)$$

where $j = \sqrt{-1}$. The displacement is a maximum, that is the ring resonates, at a pulsatance w_0 given by

$$2\pi rS\sigma - 2\pi SE/r\omega_0{}^2 = 0,$$

from which

$$\omega_0 = \sqrt{E/\sigma} \times 1/r = c/r$$

and

$$2\pi rf_0 = c \qquad \ldots\ldots(4).$$

Resonance occurs at the frequency for which the wave-length of sound in the material is equal to the perimeter of the ring.

The velocity at resonance is

$$\dot{\xi} = 2\pi SKH/k = 2\pi SK\,(4\pi ni)/k \qquad \ldots\ldots(5)$$

where n is the number of turns per cm. in the magnetising winding and i is the alternating part of the magnetising current.

The motion of the ring having been found, it remains to calculate the electromotive force induced in the toroidal winding as a consequence of the alternating Villari effect. An alternating mechanical strain ξ/r is accompanied by a change I in the intensity of magnetisation, where

$$I = K'\xi/r \qquad \ldots\ldots(6),$$

K' being the magnetostriction constant for the Villari effect. The electromotive force v induced in the winding is equal to the rate of diminution of the magnetic linkage through the winding, that is

$$\begin{aligned} v &= -\,2\pi rnS \times 4\pi\dot{I} && \text{from (6)} \\ &= -\,2\pi rnS \times 4\pi K'\dot{\xi}/r && \text{from (6)} \\ &= -\,(2\pi)^2\,(4\pi)^2\,n^2S^2KK'i/k && \text{from (5)} \\ & && \ldots\ldots(7). \end{aligned}$$

This expression for v shows that it is in opposite phase to the current i, and the electrical power required to maintain the vibration is therefore

$$(2\pi)^2\,(4\pi)^2\,n^2S^2K'Ki^2/k \qquad \ldots\ldots(8),$$

which must be equal to the power consumed by the dissipative forces in the ring, that is, to $k\dot{\xi}^2$. Equation (5) shows that $k\dot{\xi}^2$ is equal to

$$(2\pi)^2\,(4\pi)^2\,n^2S^2K^2i^2/k \qquad \ldots\ldots(9).$$

A comparison of (8) and (9) shows that $K = K'$ and therefore one magnetostriction constant is sufficient for the alternating Joule effect and the converse phenomenon, the alternating Villari effect. The magnetostriction equations become

$$p = KH \qquad \text{for the Joule effect} \quad \ldots\ldots(10)$$

and

$$I = K\xi/r \qquad \text{for the Villari effect} \quad \ldots\ldots(11).$$

This reciprocal relation between the two effects can be derived from a general theorem given by J. J. Thomson*; it has been proved here from first principles in order to present a physical interpretation of the phenomena associated with the vibrating ring.

An additional equation is required for the interpretation of the measurements described below. Equation (3) shows that the velocity of vibration is half that at resonance at two frequencies given by

$$\pm\sqrt{3}\,k = w\,(2\pi rS\sigma - 2\pi SE/rw^2) \qquad \ldots\ldots(12).$$

If the ring is very resonant, we may write $w = \omega_0 + \delta w$ where $\omega_0/2\pi$ is the resonance frequency given by (4). On substituting this value above and neglecting $(\delta\omega/\omega_0)^2$, and higher powers, we find that

$$\delta w = \pm\sqrt{3}\,k/4\pi rS\sigma \qquad \ldots\ldots(13).$$

Apart from the use of the reciprocal relation, this investigation differs from that of Pierce† in its definition of the modulus of elasticity. He expresses the magnetostriction force‡ in the form

$$p = aS\,(Bi + B'),$$

where Bi is the induction due to the periodic current i, B' is the induction due to the reverse magnetostriction effect, that is the Villari effect, and a is a magnetostriction constant for the Joule effect. The term aSB' is a force opposing the motion which is included in Young's modulus for the material and should not appear in his equation of motion§ as a term to be subtracted from Young's modulus. In fact a new modulus of elasticity which he introduces later‖ is actually Young's modulus; the discrepancy is of little practical importance and does not affect his subsequent analysis.

§ 3. THE MAGNETOSTRICTION CONSTANT

A quantity of the nickel-iron alloy containing 40 per cent. nickel was available in thin sheets 0·002 in. thick and, Pierce¶ having reported that it vibrated well, an attempt to measure its magnetostriction constant was decided upon. A number of circular rings of the alloy of 11·05 cm. and 8·55 cm. in external and internal diameter respectively, and a number of similar paper rings 0·0005″ thick were prepared. The metal rings were cleaned, covered on both sides with a thin layer of paraffin wax and piled alternately with the paper rings. The composite pile was clamped between two metal plates which were sufficiently hot to melt the wax and, after slow cooling,

* J. J. Thomson, *Applications of Dynamics to Physics and Chemistry*, ch. 5. (Macmillan and Co., 1888.)

† *Loc. cit.* p. 35 and p. 40. ‡ *Loc. cit.* p. 35 (2). § *Loc. cit.* p. 36 (5).

‖ *Loc. cit.* p. 36 (7). ¶ *Loc. cit.*

the finished laminated ring was taken from the press and superfluous wax cut away. The ring was placed on three rubber supports inside a circular wooden channel over which two toroidal coils of 227 and 215 turns were then wound (Fig. 1). The 227-turn winding carried a direct current supplied by accumulators through a large inductance and a variable resistance, and the other winding completed one arm of the alternating current bridge with which the measurements of motional impedance were made. The ratio arms of the bridge were non-inductive resistances of 100 ohms, and the remaining arm contained a calibrated inductance in series with a calibrated resistance, both continuously variable. Alternating current was supplied to the bridge from a coil inductively coupled to a 20-watt valve oscillator, the frequency of which was continuously variable. An earthed wire grid interposed between this coil and the valve oscillator screened the bridge from the electric field of the

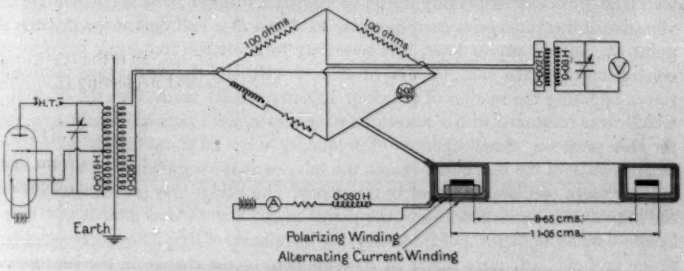


Fig. 1. Circuit for measurement of motional impedance.

oscillator. The detector was a thermionic voltmeter reading from 0 to 1·5 volts and connected to the bridge through a tuned transformer. These simple arrangements were quite adequate for the measurements of impedances up to 100 ohms at 18,000~. The stray capacities which existed between the elements of the bridge were certainly less than 50 $\mu\mu$f. and, since this corresponds to a reactance of 178,000 ohms at the frequency used, the resulting error in the balance of the bridge was negligible.

Some care was required to bring the laminated ring to a reproducible magnetic state. The ring retained some of its magnetism when the magnetising current was broken and the intensity of magnetisation therefore depended upon its previous magnetic history. It was essential to demagnetise it before applying the required magnetising field. Residual magnetism was easily detected on the alternating bridge by the presence of motional impedance at frequencies in the neighbourhood of resonance and it was possible though not easy to remove it by momentary applications of a magnetising field. The sound emitted by the ring provided another sensitive test for the presence of residual magnetism, for this sound could be easily heard when any magnetism remained. The completely demagnetised ring vibrated at twice the frequency of the alternating current, and the sound emitted was, of course, far above the audible limit.

Measurements of the effective impedance of the alternating-current winding

were made over a range of frequencies in the neighbourhood of resonance. Near the resonance frequency, a small continuous drift in the frequency of the valve oscillator, as the filament battery discharged, was accompanied by a comparatively rapid change in the impedance and, as two adjustments were necessary to obtain a balance, the latter was somewhat elusive. It was simpler to set the variable inductance to a suitable value and to obtain a balance by adjusting resistance and frequency. The measured impedances are plotted in Fig. 2 as points on a rectangular co-ordinate diagram with reactances as ordinates and resistances as abscissae. They lie closely on a circle and the resonance point lies near the end C of the horizontal diameter BC. If BA and BD are drawn so that

$$\angle ABC = \angle DBC = 60°, \quad AB = DB = \tfrac{1}{2} BC,$$

we know, from the elementary theory of the circle diagram, that the amplitude of vibration at the frequencies corresponding to A and D is half that at the resonance point C. As will appear later, it is necessary to know the frequency increments corresponding to the arcs AC, DC in order to calculate the mechanical frictional forces opposing the motion of the ring. The frequencies marked on the diagram, which were measured with a resonance wave-meter, were not sufficiently accurate for this purpose. Small changes of frequency were more accurately found by measurement of the beat note between the valve oscillator supplying the bridge and another valve oscillator constant in frequency. The bridge was set successively to the impedances corresponding to the points A, C, and D, and after it had been balanced by an adjustment of frequency, the increments of frequency corresponding to the arcs AC, DC were found by measurement of the change in the beat note. It was found that the arcs AC, DC corresponded to a frequency change of $21{\cdot}5 \pm 1 \sim$.

The circle diagram of Fig. 2 provides a sensitive test for the presence of eddy currents in the laminated ring. Pierce* and Black† have shown that the effect of eddy currents is to depress the diameter BC, drawn through the resonance point, below the horizontal. The angle of depression was about 80° for a solid rod and about 40° for a nickel tube split longitudinally to reduce eddy currents. The motional impedance of the laminated ring at resonance is seen to be practically a pure resistance and it may be concluded that the eddy currents were too small to be appreciable.

The resonant ring thus complies with the assumptions in the theory given above, which can therefore be used to interpret the experimental results. The motional impedance at resonance corresponds to the diameter BC of the circle diagram and we may write from (7)

$$(2\pi)^2 (4\pi)^2 n^2 S^2 K^2 / k = 15{\cdot}0 \times 10^9 \text{ e.m.u.} \qquad \ldots\ldots(14).$$

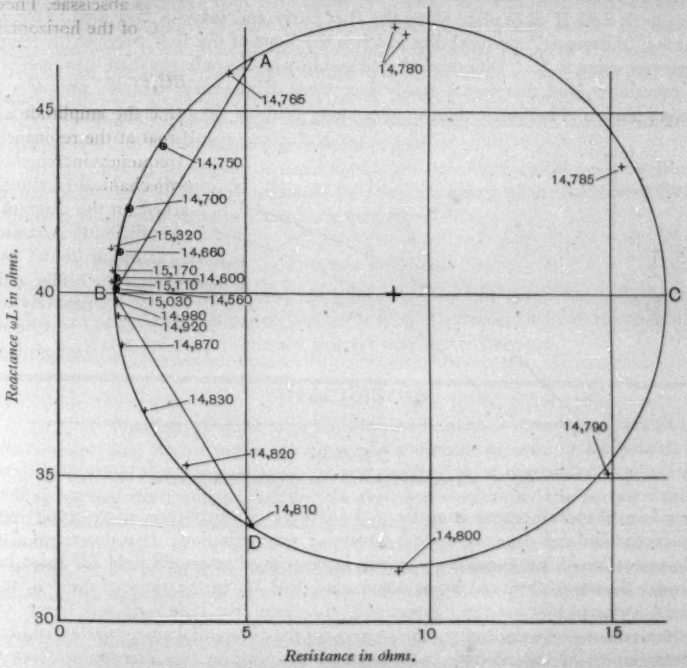
S, the cross-section of the ring, is 0·412 cm.², and n, the number of turns per cm. in the winding, is 7. The damping factor k is found to be 13,700 gm./sec. by substitution of the values $\sigma = 6{\cdot}9$ gm./cc., r 4·9 cm. and $\delta\omega = 6{\cdot}28 \times 21{\cdot}5$ in (13); and thence from (14) it appears that $K = 6{\cdot}28 \times 10^4$. There is a correction for the fact that the laminated ring contains 85 per cent. alloy and 15 per cent. paper and

* *Loc. cit.* p. 37. † *Loc. cit.* pp. 55, 61.

paraffin wax by volume. The value of K is an average over the cross-section and, since the paper contributes nothing to the magnetostriction effect, the value of K must be increased in the ratio of 100:85. The final result is therefore

$$K = 7{\cdot}4 \times 10^4$$

for the nickel iron alloy containing 40 per cent. of nickel and permanently magnetised by a steady field of 14·8 gauss. Measurements with other magnetising fields are given in the table.



Diameter of circle 15·0 ohms. The amplitude is half that at resonance at 21·5~ above or below resonance. Small figures in circle are frequencies in cycles per second.

Fig. 2. Circle diagram for a vibrating ring.

Table.

Magnetising field	3·63	7·27	10·9	14·5	17·8	21·8	25·1	31·0
K for alternating fields ($\times 10^4$)	Very small	1·7	6·7	7·4	7·6	7·6	7·4	5·3
K for steady fields ($\times 10^4$)	Very small	17·3	53	42	30	23·5	19·5	12·2

§4. MAGNETOSTRICTION IN STEADY FIELDS

It can be shown that these measurements are consistent with the changes in length of the same alloy in steady magnetising fields given by Masumoto and Nara*. The alternating magnetic field superposed on a steady field produces changes of magnetic induction represented by an elongated hysteresis loop near the B/H curve and inclined to it, the average slope of which is known as the "reversible" permeability†. The change of induction produced by a steady increment δH of a steady magnetic field H takes place along the B/H curve and may be expressed in terms of the "differential" permeability which is the slope of the B/H curve at the point corresponding to H. "Differential" permeability is always larger than "reversible" permeability, and therefore a steady increment of magnetic field δH produces a larger change of induction than an alternating field $\delta H \sin \omega t$. Now magnetostriction is a phenomenon resulting from the change of orientation of molecules by magnetisation and depends upon intensity of magnetisation. It follows that the change of length produced by a steady increment of magnetic field δH must be greater than that produced by an alternating field δH in the ratio of the "differential" susceptibility to the "reversible" susceptibility. This ratio was found by measurement of the slope of the B/H curve for the differential susceptibility and by measurement of the effective inductance of the ring on the alternating current bridge for the reversible susceptibility. The magnetostriction constants for steady increments of magnetic field are obtained by multiplication of the constants for alternating magnetic fields by the corresponding ratio of susceptibilities, with the result shown in the second row of the table above. These figures are the slopes at a number of points of a curve connecting mechanical stress with the steady magnetising field producing it, and this curve can be found by graphical integration

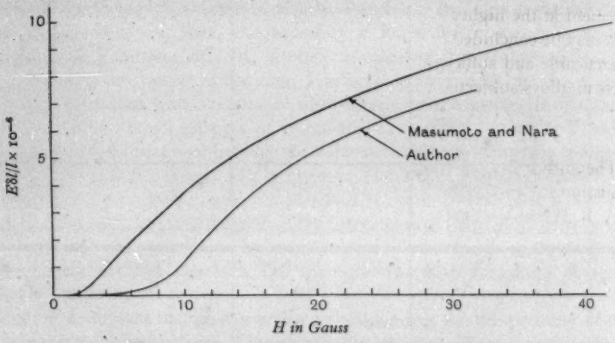


Fig. 3. Magnetostriction in steady magnetising fields.

* Masumoto and Nara, *Sci. Rep. Tohoku Univ.* **16**, 338 (1927).

† Spooner, *Properties and Testing of Magnetic Materials*, p. 11. (The McGraw Hill Book Co. 1927.)

from the slopes. The result is shown in the lower of the curves of Fig. 3. The upper curve is obtained when the changes of length of the same alloy as given by Masumoto and Nara* are multiplied by Young's modulus. The shapes of the curves are similar and they are roughly asymptotic at the stronger magnetic fields. The differences at the weaker magnetic fields can be accounted for by a corresponding difference in the B/H curves. The alloy used by Masumoto and Nara was soft and that used by the author was somewhat hardened by cold working. Soft material magnetises more readily than hard material in weak magnetic fields and it should show a correspondingly greater magnetostriction effect in such fields. In strong fields, however, both materials tend to approach similar values of saturation induction and therefore should have similar magnetostrictive change of length. The relation between the magnetostriction curves for the two samples is in fact qualitatively the same as that between the B/H curves, and, moreover, the quantitative agreement at the higher magnetising fields is good.

It may be concluded that the method of measurement which has been described is practicable and sufficiently accurate for the comparison of the magnetostrictive effects in alloys subjected to alternating magnetic fields.

§ 5. ACKNOWLEDGMENTS

The author desires to express his indebtedness to the Admiralty for granting permission to publish this paper, and to Dr C. V. Drysdale, Dr E. P. Harrison and Dr A. B. Wood for their continued interest and encouragement.

DISCUSSION

Dr J. H. VINCENT. I think that the Physical Society is to be congratulated on the presentation of this paper, which marks a distinct advance in the subject of the application of magnetostriction to the production of mechanical oscillations. I suppose that the formula given for the period of oscillation of an anchor ring in its fundamental radial mode is only approximate. The period of oscillation for an infinitely thin ring is the circumference divided by the velocity of sound in the material. If the anchor ring is supposed to have its orifice filled up with the same material we approach the case of a wire vibrating radially. Whenever a current flows in a wire of magnetostrictive material the radius of the cross-section must change in consequence of the magnetic field which the current produces. This should entail a number of interesting consequences, some of which might be capable of experimental demonstration. I should like the author to tell us a little more about the behaviour of the anchor ring when vibrating under the effect of alternating current in the absence of polarising current.

Dr D. W. DYE. I have read the present paper with much interest and wish to congratulate the author upon a difficult piece of work in a field not hitherto explored. I admire the author's courage in obtaining the results he has worked out, in spite of the admixture of paper, paraffin wax and nickel-iron alloy investigated. These

* Masumoto and Nara, *loc. cit.*

must form a specimen of rather poor vibratory properties. There are three points to which I would like to draw attention. (1) In discussing the Young's modulus of the alloy the author states that Pierce should not have used the term *aSB'* as a term to be subtracted from Young's modulus. Pierce is quite consistent in what he has done; the difference between him and the author is simply one of definition of Young's modulus, which is different in the two cases. One is free to choose either. (2) The dimensions of the built-up ring, as shown in the diagram, indicate a ratio of 1:3 between outer and inner diameters of the ring. This will result in a corresponding ratio of magnetising fields between inner and outer edges. The results obtained are, therefore, those referring to an average magnetisation corresponding to this range of *H*. (3) The values obtained from the circle diagram and by the theory given pre-suppose a pure mode of radial vibration in which the stresses are purely compressional and tensile in the direction of magnetisation. From my experience of quartz specimens of all shapes and sizes I should not be at all surprised to find that a ring of the shape shown had overtone flexural modes such that the vibratory energy was divided up between two or more modes of vibration. This would profoundly affect the magnetostriction constant as here determined. In the analogous case of the quartz plate, the effective piezo-electric constant nearly always comes out at about one-third only of its theoretic value when the motion is complex, simply because energy is divided up between various modes, only one of which reacts piezo-electrically upon the maintaining circuit. I would suggest that a ring of greater thickness should be tried to see if the same result is obtained.

Mr W. A. Benton. I consider the paper of quite outstanding interest and importance. There was one remark, however, made by the author in presenting his paper to which I felt exception might be taken. Mr Smith spoke of the "alternating current producing stresses unaccompanied by changes of dimensions in the magnetic material". It is difficult, if not impossible, to conceive of any stress not accompanied by strain, and, as a matter of fact, the mathematical reasoning would appear to hold good if small dimensional changes accompany the alterations in stress. It may interest Fellows to know that over 50 years ago Dr Du Moncel, and, I believe, others, described telephones based on magnetostriction effects. A rod of magnetic material attached to the centre of a diaphragm reproduced music and speech when acoustically modified currents circulated round the rod. It would not appear, however, that anyone has used the reverse, or Villari effect, as a telephonic transmitter. The very considerable changes of stress which could be produced by acoustic vibrations in a rod or wire made of a magnetic material especially susceptible to the Villari effect could be used to modify the strength of a current and thus form a transmitter.

Author's reply. The simple formula given in the paper for the fundamental mode of radial vibration of a ring is, as Dr Vincent points out, only approximate. An exact calculation which takes into account the finite radial width of the ring shows that the error involved in this approximation is negligibly small for the

comparatively thin ring used in the experimental work. I am unable to give Dr Vincent any further information on the behaviour of a ring subjected to an alternating magnetisation only.

I agree with Dr Dye that the theory given by Pierce for the vibrating rod is accurate provided that Young's modulus for the material is suitably defined. Pierce does not, however, define this modulus in his paper, and the usual simple definition is therefore implied, whereas another rather complicated definition is required if his theory is to be consistent. There is no doubt in my mind that the predominant mode of vibration of the laminated ring is radial. The sound in air emitted by the vibrating ring is audible and it is possible to explore its surface with a small stethoscope. The sound intensity appears to be uniform over the whole of the outside and inside cylindrical edges. The flexural forces in the vibrating quartz disc arise from the fact that the diameter of the disc is usually large in comparison with the wave-length of sound in quartz at the frequency of the vibration. The expansions and contractions of the quartz in the plane of the disc, depending upon Poisson's ratio, are partially prevented by a lateral constraint from the inertia of the disc, and radial stresses are produced. These stresses are not quite symmetrical with respect to the quartz disc because the piezo-electric properties are not, in general, quite uniform throughout the disc, and stresses are therefore produced which can excite flexural modes of vibration. In contrast to the quartz disc, which is usually large in two directions in comparison with the wave-length, the laminated ring is small in two directions in comparison with the wave-length. Expansions and contractions in the directions of the thickness and the radius of the ring, depending upon Poisson's ratio for the material, are practically unconstrained. Moreover, the ring is built up from a large number of thin stampings assembled at random, and any appreciable variation of the magnetostriction constant along the circumference is improbable. It is difficult to imagine how any stresses can arise which might excite a flexural mode of vibration.

The conception of a material in a state of stress without strain, which Mr W. A. Benton finds difficult to accept, can perhaps be most clearly realised in the following case. Let a long thin rod of a material, having the property of magnetostriction, be magnetised so that its length and cross-section change. The rod can be restored to exactly the original dimensions by forces applied to its ends and surface, so that it is completely freed from strain. These forces are in equilibrium with the forces arising from stresses in the material, so that a state of stress without any strain whatever is produced.

FIELD INTENSITY MEASUREMENTS AROUND SOME AUSTRALIAN BROADCAST STATIONS

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Communicated by Professor T. H. Laby, January 17, 1930.
Read and discussed February 28, 1930.

ABSTRACT. The field intensities of wireless transmitting stations have been measured in order to determine some of the factors controlling the propagation of electromagnetic waves, and the effect of the surface conditions on their attenuation. A simple loop, condenser and valve voltmeter circuit has been employed, and careful tests show that this method is available for the measurements of intensities as low as 1 mv./m. The grid rectification voltmeter described is found to measure e.m.f.'s down to 0·01 volt, to be free from frequency error, reasonably stable in calibration, and not to absorb power from the circuit to which it is connected—the last being contrary to current theory. The advantages of this intensity set, which agrees over a wide range of field intensities with a Western Electric set, in respect to simplicity, convenience and portability are pointed out.

The field strength contours of three broadcast stations have been determined, from which the following conclusions have been drawn: (i) Very rapid attenuation of the signal is caused by Australian forest areas; this enormously curtails the areas for which a satisfactory service is provided. (ii) The effective conductivity of the various types of ground surface met with varies from 4×10^{-13} to $0{\cdot}07 \times 10^{-13}$ e.m.u., according to the number of trees in the areas covered. (iii) The use of a longer wave-length gives a marked increase of intensity at distant points beyond forest areas. (iv) For daylight transmission over sea water up to a distance of 85 miles, after the application of curvature-corrections to the intensity, Sommerfeld's formula is correct, to within the limits of experimental error. (v) The efficiency of radiation of the three aerials examined ranges from 48 to 60 per cent.

Finally, the limits of signal intensity required for satisfactory reception under Australian conditions are discussed, and it is suggested that atmospherics and other disturbances are less prevalent in Victoria than in Europe or America.

§ 1. GENERAL DESCRIPTION OF THE METHOD AND APPARATUS

If we have a loop aerial tuned by a condenser, then the ratio of the signal intensity to the voltage which it induces across the loop is constant for all values of the intensity. The value of this ratio depends on the geometry of the loop, its resistance and reactance, and can be determined for a given loop once and for all. Hence if this ratio is known, and if the induced voltage is measured at any point, the field intensity at that point can be deduced.

In the set used by the author the centre point of the loop was earthed, as shown in Fig. 1, in order to preserve the directional effect of the loop aerial by elimination of the "antenna-effect," and the induced voltage across one-half of the loop was

measured with a valve voltmeter. The absolute value, E millivolts per metre, of the field intensity, was determined at this laboratory by an independent method, and the voltage V across half of the field loop was measured at the same place. Hence the ratio E/V for this loop was obtained; it was found to remain constant by similar comparisons carried out at regular intervals. Its value was

$$E/V = 86.$$

The valve voltmeter was calibrated after each reading of V in the field by means of a high-frequency oscillator, a vacuum thermocouple and a non-inductive resistance box.

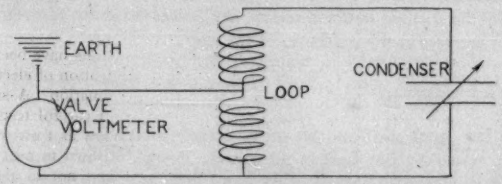

Fig. 1. Circuit diagram.

The measuring and calibrating equipment was mounted on a board in the back of a small motor car, while the fixed loop was carried in a case fastened on to the running board, and was erected on the side of the car for the taking of observations. Measurements made with the apparatus in the car, and on the ground with the car removed, showed that the car had no appreciable disturbing effect.

§ 2. DETAILS OF THE APPARATUS

The valve voltmeter. The circuit is shown in Fig. 2. The points of interest of the voltmeter are: (*a*) the absence of the artificial grid leak resistance, so that the grid of the valve is apparently "free." This greatly increased the sensitivity of the instrument and diminished the power absorbed. There was no sign of instability of the valve, probably because the conductivity of the base of the valve, the valve socket and the valve pinch prevents the grid from blocking. (*b*) The size of the grid condenser, viz. 0·002 μf. (*c*) The balancing-out device for eliminating the steady plate current from the microammeter. This affords a very sensitive means of adjusting the zero of the instrument to any point of the scale. (*d*) The loop tuning condenser; as neither set of plates was at earth potential, in order to avoid body-capacity effects it was necessary to enclose the condenser and voltmeter in an earthed shield connected to the middle point of the loop. Although the batteries and the tuning condenser handle were not shielded, there was no disturbing hand-capacity effect during tuning. The condenser was a double-spaced transmitting condenser of maximum capacity 85 $\mu\mu$f.

The small link between the earth and grid terminals of the voltmeter was found to be of great practical convenience when frequent interchanging of the

connections was necessary. The grid terminal was very susceptible to large fluctuations of potential while connection was being made, causing momentary paralysis of the valve.

Characteristics of a voltmeter. These are: (i) sensitivity and range, (ii) frequency error, (iii) wave form error, (iv) power used, and (v) stability of calibration.

The relative importance of these characteristics will depend on the circuit to which the voltmeter is connected and the quantity that is to be measured. When a small high-frequency current is to be measured by means of the voltage across an inductance tuned to resonance with the applied e.m.f., it is essential that the voltmeter should absorb no power from the circuit. In the measurement of field intensities by the method outlined above, any power taken by the voltmeter will appear as an increase in the resistance of the loop.

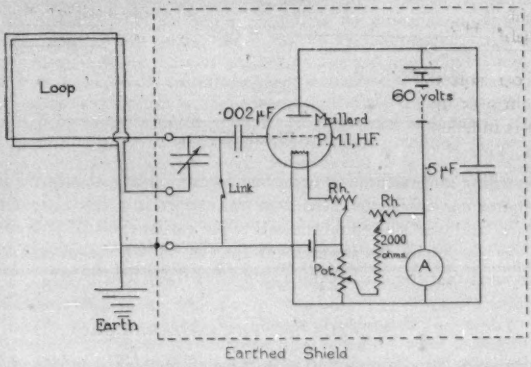

Fig. 2. Valve voltmeter circuit.

Performance of the grid-type valve voltmeter. (i) Sensitivity. With a portable microammeter having a sensitiveness of 10 mm. per microamp., the lowest measurable voltage was 0·01 volt (deflection about 2 mm.) corresponding to a field intensity of 0·86 mv./m. This sensitivity was obtained by the correct choice of valve (Mullard P.M. 1 H.F.), the operating voltages, and the absence of an added grid leak. With a grid-leak resistance of 10 megohms the sensitivity was considerably diminished, especially for small voltages where the extra sensitiveness was most required. (ii) Frequency error. If the voltmeter is to be independent of frequency, the reactance of the grid condenser must be small compared with that of the grid-filament capacity of the valve and its socket. Taking the latter as not greater than 30 $\mu\mu$f., the grid condenser was fixed at 0·002 μf. To check this point, the voltmeter was calibrated at 600 kc. and 900 kc. and the agreement was within 1 per cent. In the field the calibration was carried out at 750 kc., which is about the middle of the broadcast band under investigation. If the grid condenser was larger than 0·01 μf., the grid took an appreciable time to reach its final steady

potential under the action of the incoming signal, and there appeared to be no advantage gained by the use of a larger value than that adopted. (iii) Wave form error. In order that the voltmeter reading should be independent of the wave form of the applied e.m.f., it is necessary that the rectified current be proportional to the square of the applied voltage. For the calibration curve below 0·1 volt this condition is fulfilled, as can be seen from Table 1, which is taken from a typical curve.

Table 1

Rectified current	0·2	0·67	1·45	2·75	4·3	6·15	8·4	11·2	17·1 μa.
Applied potential	0·01	0·02	0·03	0·04	0·05	0·06	0·07	0·08	0·1 volt
$k \times \frac{\text{Current}}{(\text{potential})^2}$	44·7	40·8	40·1	41·5	41·5	41·3	41·4	41·9	41·4

Above 0·1 volt the curve departs from its parabolic form and it becomes straight from 0·15 up to 0·4 volt. Hence for applied voltages below 0·1 volt, the calibration is independent of wave form, while above this point it will depend on wave form. This characteristic is important when the induced voltage due to a modulated signal is being measured. Above 0·1 volt, there were considerable fluctuations of the voltmeter reading due to the modulations of the signal, while below this figure there was practically no sign of such modulation, even though the deflections of the microammeter were as large as 80 mm. As only the carrier-wave intensity was required, it was necessary to wait until the modulation ceased. The short period of the instruments used made this cessation easy to detect, a silent period of 2 seconds being enough to allow the pointer to take up its position for the unmodulated signal. No special precautions were taken to get a pure wave-form for calibrating the voltmeter except that the coupling between the oscillator and the tuned circuit containing the thermocouple and resistance box was kept loose. (iv) Power used. As was mentioned above, the power absorbed by the voltmeter should be small and independent of the applied voltage. A valve which uses the curvature of the grid-current grid-potential curve for rectification must absorb some power, because the action of the rectifier depends on the presence of grid current. In broadcast receivers, the grid condenser is usually connected to the positive terminal of the filament, with a resistance of 1 to 3 megohms across the condenser, and such an arrangement increases the resistance of the input circuit enormously, necessitating the use of reaction to counteract it. This damping of the input circuit increases as the input voltage decreases, and largely counterbalances the gain in sensitivity due to the grid condenser and leak. In the voltmeter described above these defects are reduced to a minimum, while the sensitivity is increased, by the omission of a grid leak. This arrangement is unsuitable for the detector valve in a broadcast receiver. No appreciable effect of the voltmeter on the input circuit could be detected by the following test. Two instruments of identical construction were connected in parallel across a loop tuned to resonance,

and a small voltage was induced in the loop by a local oscillator. Upon the switching out of the filament in one voltmeter, no change was detectable in the reading of the other meter. This test repeated for a series of voltages gave no evidence of power absorption. This only shows that the grid voltmeter has the same effect on the loop circuit whether the valve is alight or not, but bridge measurements at 1000 ~ showed that in both cases the input of the voltmeter with tuning condenser behaved as a nearly perfect condenser of about 30 $\mu\mu$f. capacity. The same test, repeated with an anode-bend valve voltmeter and the grid voltmeter, showed that the former instrument absorbed power. Evidence to the same effect had previously been obtained by other means. (v) Stability of calibration. The voltmeter was calibrated after each reading of the voltage, so that stability of calibration, although it obtained fairly well, was never assumed. The variations during a day were somewhat irregular, but the constancy was usually within 10 per cent. The calibration curves for a single valve over a period of several months were all of the same form, showing an equal percentage change over the full range. Hence one or two points on the curve served to fix the whole of it with sufficient accuracy for the deduction of the signal intensity.

The microammeters. For applied voltages above 0·08 volt, a Paul unipivot galvanometer (1·8 μa. per division) was used; this instrument when shunted also measured the steady plate current. For lower voltages, a portable reflecting Tinsley galvanometer (10 divisions per μa.) was used. The shortness of the period of these instruments made them very suitable for the work. These two galvanometers were mounted together in a box which was suspended by six small coil springs and rested on rubber sponges inside another box. The efficacy of this suspension was shown by the fact that in over 5000 miles of travel, for a large part on rough and unformed country roads, no trouble was experienced with this reflecting instrument. No special mounting was necessary for the rest of the apparatus.

The microammeters showed no instability due to the absence of an artificial grid leak, but there was usually present a gradual creep of the zero of the voltmeter if the batteries and valve had not reached their steady state. No difficulty was experienced in allowing for this creep of the zero, unless it exceeded the rate of 1 mm./sec.

Calibration of the voltmeter. The voltmeter was calibrated by a potential measured, as in Fig. 3, by means of a thermocouple and a non-inductive resistance box calibrated with high-frequency current. Care was taken to minimise stray capacity effects and good agreement was obtained between a high-frequency calibration and a 50 ~ calibration of an anode-bend voltmeter designed to be independent of frequency.

The field loop. In order that the ratio of field intensity to induced voltage, i.e. E/V, may remain constant, it is essential that the loop aerial used in the field should retain its shape and size, and also that its high frequency resistance should remain constant. While it was desirable to reduce this as far as possible to get a more efficient aerial, it was even more important to secure permanence, even at the expense of efficiency. It was therefore decided that solid copper wire should be

used instead of Litz wire. The loop was a square solenoid (side 65·1 cm.) of 18 turns of no. 24 gauge enamelled copper wire, spaced 0·2 in. apart on thin bakelite strips. This construction gave an exact centre point, and should diminish any possibility of change in resistance during the field work which was carried out during fine weather. In the course of 3 months no evidence was obtained for any change as great as 2 per cent. An occasional tightening of the loop was the only attention required.

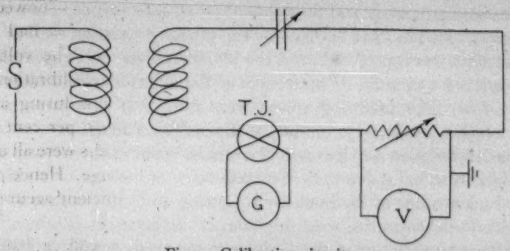

Fig. 3. Calibration circuit.

§ 3. DETERMINATION OF THE RATIO E/V FOR THE FIELD LOOP

The relation between the field intensity E mv./m. and the voltage V millivolts induced between the ends of a tuned loop is

$$V = 2\pi An\omega LE/R\lambda$$

where A is the area of each turn in metres^2,
n the number of turns,
λ the wave-length in metres,
$\omega/2\pi$ the frequency,
L the inductance in henries,
R the resistance in ohms.

All of these quantities can be measured. From the above relation it would seem to be possible to get the value of E/V for the whole loop, and hence for half the loop, but the voltage across the half-loop was found to be more than half that across the whole loop. This result was due to the circumstance that the natural frequency of the loop was comparable with that of the stations under investigation, and difficulties were experienced from the same cause in the measurement of the high-frequency resistance of the loop. While approximate corrections for this self-capacity effect could have been calculated, it was decided to measure the absolute value of the intensity with another loop, at the same time measuring the voltage across half the field loop. Hence the ratio E/V was at once obtained. It was $E/V = 86$. This ratio was checked at intervals during the field work by the same method.

§ 4. DETERMINATION OF THE ABSOLUTE VALUE OF THE FIELD INTENSITY

A standard loop was constructed for this purpose and was kept at this laboratory, the intensity being calculated from the voltage induced across the whole of the loop as measured by the valve voltmeter. It was a square (side 132·3 cm.) of 4 turns of $\frac{5}{8}$ in. woven copper braid, spaced at 4 cm. apart. The inductance and resistance were determined separately and the value of $\omega L/R$ was deduced for a wave-length of 371 metres. This ratio was checked by the capacity-variation method. A special set of resistances was constructed and the measurements were repeated with both the voltmeter and a vacuum thermocouple as the detector in all determinations of R, L, or $\omega L/R$. The following values were obtained: $L = 51{\cdot}9$ microhenries, $R = 0{\cdot}913$ ohms when $\lambda = 371$ metres, so that $\omega L/R = 287$.

By capacity variation $\omega L/R = 291$. The mean value of the step-up is therefore 289, the high value being due to the low resistance of the copper braid. With this loop, the absolute value of the intensity is considered to be accurate to 1 per cent.

To check the absolute value of the intensity as determined with the standard loop, simultaneous readings at the University were taken with it and a Western Electric field intensity set, the property of the Research Laboratory of the Postmaster-General's Department. The mean value of the intensity determined by both sets was 55 mv./m. The standard loop was similarly calibrated for a wave-length of 484 metres, corresponding to that of the other local broadcast station, 3AR Melbourne. A comparison with the Western Electric set gave an intensity of 50, as against 51 mv./m. read by the standard loop. The ratio of E/V for the field loop for this wave-length was $E/V = 107$.

To test if E/V for the field loop was independent of E, a series of readings over a wide range of intensities was taken in conjunction with the Western Electric set. With the value $E/V = 86$ obtained above, Table 2 gives the results obtained:

Table 2

W.E. set	59·5	30·5	13·4	10·4	7·5	2·8	2·15	1·46	0·55 mv./m.
Valve voltmeter set	59·5	30·5	14·0	10·4	7·6	3·0	2·4	1·5	0·5 mv./m.

It therefore appears that this method of measuring intensities is as accurate as is required in this class of work.

§ 5. MERITS OF THE VALVE VOLTMETER FIELD INTENSITY MEASURING SET

The apparatus described will measure satisfactorily field intensities as low as 1 mv./m. This is a field too small for satisfactory reception according to English and American authorities.

Advantages. The following advantages are claimed for the set: (i) Simplicity of construction. This is apparent from the details already given. The voltmeter and

oscillator are made of inexpensive wireless equipment, and the high-frequency decade resistance box, vacuum thermocouple and galvanometers are all easily obtainable. No elaborate shielding, such as is essential on sets making use of loop-injection methods, is required, and considerable weight is thus saved. Other difficulties, e.g. the attenuation box or the carefully balanced input circuit, are also avoided. (ii) Simplicity of operation. The loop tuning condenser is the only tuning control, and with a vernier drive this is easy to operate. The voltmeter deflection for several broadcasting stations can be quickly determined, and a single frequency of calibration serves for all of them. This is an important simplification for rapid and accurate work. (iii) Speed of work. The whole of the field work, including driving, observing, booking and reduction of the results, was done by the author. The observations used for plotting the contour map were obtained in 25 days in the field. In this period 497 individual observations were taken on two stations during their normal daylight working hours, no special transmissions being arranged. The area surveyed was over 10,000 sq. miles, and the distance travelled was 3067 miles. The time taken in an observation on two stations was about 7 minutes, from stopping to starting again. During the 9 months that the apparatus has been in use, not a single delay due to failure of any part of it has been experienced. (iv) Portability. The loop and voltmeter alone can easily be carried by one man without a car, and this enables the apparatus to be set up in places to which a car cannot get. This property enabled the detailed effects of hills on the field intensity to be studied.

Disadvantages. The great disadvantage of this type of set is its inability to measure very weak intensities. The rectifier used should obey a square law, i.e. the rectified current should be proportional to the square of the applied voltage, which holds for this instrument below 0·1 volt. For an intensity of 1 mv./m. the deflection is about 2·5 mm., and half of this intensity would only give 0·6 mm. By slight alteration of the construction of the loop, however, without increase of its dimensions, the ratio E/V was reduced from 86 to 63 and a slightly more sensitive detecting valve (P.M. 12) has been added. The deflection for 1 mv./m. is now 7 mm. No simple way of increasing the range of the instrument further, while still retaining its portability and other advantages, has been found. As there is only a single tuned circuit, lack of selectivity would also be a difficulty under certain conditions.

§ 6. ROUTINE IN THE TAKING OF AN OBSERVATION

For taking an observation, open sites likely to give the undisturbed value of the intensity in that locality were always chosen. The chief disturbing factors were: (i) Telephone wires, especially if present in large numbers. In one case a bank of 50 wires diminished the field intensity for over 50 yards on each side of the line. In another case, two wires doubled the intensity directly under them and produced a directional error of nearly 50°. The field was normal again 15 yards on either side of the wires. (ii) Crests of hills. On a steep slope facing toward the broadcast station, the intensity was always greater than at the foot of the hill or

at a distance behind the crest. (iii) In timbered country, an open place at least 50 yards from any large tree was always selected. The site having been selected, the car is turned side-on to the direction from which the signal is coming. The valve, which is carried separately, is inserted into the voltmeter, and the loop is set up. The loop is tuned and rotated to give the maximum deflection on the microammeter, which is noted. The voltmeter terminals are then short circuited with the link, the loop terminals are disconnected, and short leads from the resistance box are connected. The link is opened, the local oscillator started and the current is adjusted to give a convenient reading on the thermocouple metre. The voltmeter deflection and thermocouple currents for known resistances are noted, and the calibration curve is plotted. In the field this curve is only taken twice a day. For most of the observations, a single point near the observed voltmeter reading or two points on either side of it are sufficient, on account of the similarity of all the curves, as has already been explained. From the calibration curve the voltage due to the incoming signal is found and the intensity is at once deduced.

§ 7. RESULTS OBTAINED IN VICTORIA ON 3LO AND 3AR MELBOURNE

Field intensity map of 3LO *Melbourne.* The observed intensities were plotted on a map, and the 15, 10, 5, 2·5 and 1 mv./m. contours were interpolated, as shown in Fig. 4. Only three observations out of 330 that are plotted are not in agreement with the contours as shown; two of these were suspected at the time of observation to be affected by ground contours but they were taken at the only available sites.

Effects of trees and mountains. The shape of the contours indicates that very marked absorption of the signal takes place in certain directions. In every case, these areas are heavily timbered and are usually associated with mountains or steeply sloping hills. In Victoria mountains and forests usually occur together, and it has not been possible to discriminate completely between them as to the cause of the absorption. In a few cases where the timber is dense while the country is only undulating, the rapid absorption has still been found, but it is not possible to state the effects of bare mountainous country. Only in a few directions does the transmission take place over fairly clear ground and the area given a satisfactory service is far less than was expected from the extent of the 10 mv./m. contour which was located before the present apparatus had been developed. In heavy timber the intensity falls from 10 mv./m. to 1 mv./m. in a distance of only 10 miles. Such areas exist to the east behind Mount Dandenong, to the north-east behind Whittlesea, and to the north-west in the Daylesford forest. Barfield in England* found that the signal was attenuated more rapidly when it passed over forest areas than when it passed over clear country, but in no areas were the effects so pronounced as in the present observations. This discrepancy is due to the difference in size and the number per unit area of the trees, in the two cases, for in the areas dealt with in the present paper the forests cover many hundreds of square miles,

* *Journ. Inst. E. E.* **66**, 204 (1928); **67**, 253 (1929).

while the density of the trees usually exceeds 10,000 per km². In many cases the number is probably greater than 30,000.

The height and leafiness of the trees was found to play an important part in the attenuation. Thus, in the areas mentioned, the trees are mostly over 100 feet high and have a fairly luxuriant foliage. In contrast to this, there is an area about

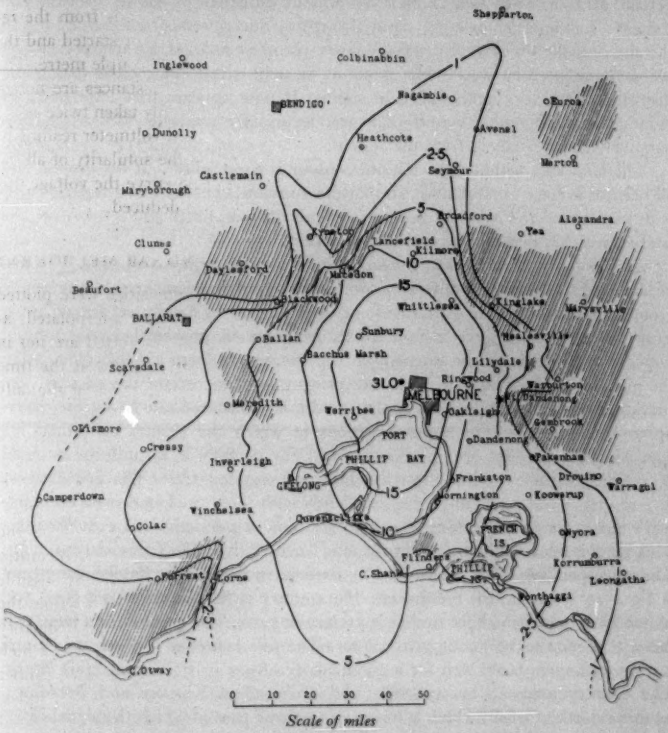

Fig. 4 Field intensity contours of 3 LO Melbourne, wave-length 371 metres, power 5000 watts.* Shaded areas represent mountains and forests.

* This is the power in the plate circuit of the power amplifier coupled to the aerial.

40 to 50 miles west of 3 LO covered with small trees, between 20 and 40 feet high, and with comparatively little foliage. The effect of this belt of timber is plainly seen in the form of the contours in this direction, but it is not so marked as in the former areas. This very rapid attenuation of the signal as it passes over forest areas explains the difficulty experienced in receiving Melbourne broadcast stations,

in the eastern parts of Victoria, where, during daylight, the signal is very weak but of uniform intensity, while, after dark, fading and distortion are so bad as to spoil the programmes completely. These reception difficulties start at Warragul, about 60 miles east of Melbourne, where the daylight intensity is considerably less than 0·5 mv./m. The conditions existing in Gippsland are exactly similar to those in certain areas around New York City and investigated by Bown, Martin and Potter*. In the latter case the rapid absorption due to steel-framed buildings in the city near the transmitter produced areas within 35 miles of the station in which the daylight intensity was as low as 50 μv./m., and within these areas fading and distortion were very pronounced at night. It now appears that large areas of mountainous and timber country can produce exactly similar conditions at comparatively short distances from the station.

The land area within the 1 mv./m. contour is 9400 miles^2. If the ground had all been of average conductivity, so that the 1 mv./m. contour was a regular curve with its limits at the existing contour to the south-east, north and south-west, the land area within it would be over 20,000 miles^2.

Determination of the ground conductivity for various surface conditions. This can be deduced from the field intensity contours by applying Sommerfeld's theory of ground absorption†. The convenient curves published by the British Broadcasting Corporation‡, which give Sommerfeld's formulae in graphical form, make it possible to write down the intensity at a point distant from a transmitter, given the ground conductivity; or, as in the present case, to reverse the process. No curvature corrections have been applied, and it is considered safe to use the curves up to 100 km. only. The three directions in which the ground conditions are approximately uniform are: (i) South-west. The country is as uniform as could be desired, as there are but a few low hills and very few trees. The five contours (15, 10, 7·5, 5 and 3 mv./m.) agree excellently with those predicted from Sommerfeld's theory for a conductivity of 4×10^{-13} e.m.u. or a resistivity of 2500 ohm-cm. This value is taken as the true "bare ground" conductivity for Victorian conditions. The corresponding value determined by Barfield and others for English conditions is $2{\cdot}5 \times 10^{-13}$ e.m.u. (ii) South-east. The country is flat but there is a good deal of low scrub and some light timber is present in parts. The ground for a large part has a tendency to be swampy in winter. The five contours (15, 10, 7·5, 5 and 2·5 mv./m.) agree fairly well for a conductivity of $3{\cdot}3 \times 10^{-13}$ e.m.u. (iii) North. The country tends to become hilly and timbered at Kilmore and Broadford, 35 miles north of 3LO. There is however a narrow pass in which the signal is not rapidly attenuated. The country then becomes flat again, but a considerable amount of scattered timber still remains, a rough estimate giving from 200 to 500 trees per km.^{2} The country is all comparatively dry, and the conductivity deduced from the 10, 7·5, 5 and 2·5 mv./m. contours is 10^{-13} e.m.u. In heavily timbered and mountainous country, the effective conductivity deduced is as low as $0{\cdot}07 \times 10^{-13}$

* *Proc. Inst. Rad. Eng.* **24**, 57 (1926). † *Annalen der Physik.* **28**, 665 (1909).
‡ *The Service Areas of Broadcasting Stations*, by P. P. Eckersley.

e.m.u., which is only 1/50 of its value under favourable conditions. The value given by Eckersley* for this type of country is $0{\cdot}05 \times 10^{-13}$ e.m.u.

All these values are deduced from the contours which are interpolated between readings of the intensity taken at suitable points. More accurate determinations could be made from special observations taken at selected stations, with checks on the station output, but in only a few directions is the ground of sufficient uniformity to justify such an undertaking. In the most favourable direction, viz. south-west, the five points taken along a radial line agree excellently with the value stated.

Recent determinations of ground resistivity made by Mr Bruckshaw, of the Imperial Geophysical Experimental Survey, by Gish and Rooney's method give the value 1500 to 2000 ohm-cm., which agrees satisfactorily with the "bare ground" conductivity determined above.

Table 3

Type of country	Distance d in miles	Product Ed	
		3 LO $\lambda = 371$ metres	3 AR $\lambda = 484$ metres
Flat with not many trees ...	25	100	100
S.W. and S.E. of 3 LO ...	75	65, 55	78, 75
Undulating with some timber	25	100	100
N. of 3 LO	75	30	55
W. of 3 LO	30	100	100
	60	40	62
Sea water	25	100	—
	73	87	—

Shielding effects of the masts at the transmitting station. In the field strength contours around 2 LO London, as shown by Barfield and Munro†, large crevasses occurred owing to absorption in the masts supporting the transmitting aerial. There is no sign of similar effects in the case of 3 LO Melbourne. The masts at the latter station are 200 feet high and are earthed at their bases. The aerial is in the form of Γ with the horizontal "roof" pointing to the north.

Effect of change of wave-length on the intensity at distant points. Information on this matter was obtained from observations on 3 AR Melbourne on a wave-length of 484 metres. This station was "on the air" only during the morning, and results for it are not so complete as for 3 LO Melbourne. The general form of the contours is fairly reliable, and Table 3 summarises the results of 160 observations. In it is shown the decrease of the product intensity × distance with increase of distance for various types of country. For purposes of comparison, all the values are so reduced as to make the product exactly 100 at the shorter distance.

The decrease of Ed in the above table will partly be due to the curvature of the earth, the effect of which at more than 50 miles from the transmitter is appreciable. It has not yet been possible to separate the losses into true-absorption and curvature losses.

* Eckersley, *loc. cit.* p. 8.

† Barfield and Munro, *loc. cit.*

The aerial system at the station 3AR Melbourne does not even approximate to a vertical uplead with a horizontal roof, there being a certain amount of energy directed upwards at an angle of from 30° to 60° in certain directions. The observations contain evidence that some of this energy is returned at distances greater than 40 miles, and so it is not possible to apply Sommerfeld's formulae to deduce the ground conductivities from this station.

Table 4 gives the decrease of intensity due to a small extent of heavily timbered and hilly country.

Table 4

Width of timber in miles	Decrease of intensity	
	$\lambda = 371$ metres	$\lambda = 484$ metres
6	12·4–1·6 mv./m.	14·5–5·7 mv./m.
5	8·6–2·0 ,,	8·0–3·6 ,,
6	7·5–3·7 ,,	6·6–4·1 ,,
13	10·5–2 ,,	10·5–3·6 ,,

All the quantitative values for the conductivity of the various types of ground surface dealt with above and the variation of the attenuation with wave-length are deduced from values of the intensity extending over a wide area of country, in order to obtain the general features of the transmission. More detailed investigation of these values will be undertaken.

Transmission over sea water. In order to investigate the decrease of intensity due to the effects of the curvature of the earth alone, a series of readings was taken over sea water where the surface has a uniform high conductivity (10^{-11} e.m.u.). The readings were taken during the daylight part of the journey from Melbourne to Burnie, Tasmania, which extended to 90 miles from Melbourne. The motion of the boat made the readings of the unipivot galvanometer uncertain of 1 mv./m. in all the readings at distances of more than 40 miles. A large number of readings was taken and the time for each was recorded. The position of the ship at that time was computed from its measured speed over a distance of 30 miles before entering open water. The results are marked with centred circles in Fig. 5, in which log Ed is plotted against d.

If the intensity is corrected for curvature from results given by Ratcliffe and Barnett* for similar wave-lengths, the points marked with crosses are obtained; they lie about a horizontal line. This result indicates that there is no attenuation due to the finite conductivity of sea water up to a distance of 85 miles, and is in agreement with Sommerfeld's formula for transmission over a plane surface of conductivity 10^{-11} e.m.u., which gives the departure from the horizontal line at that distance as less than 2 per cent. To test this point further, readings were taken on 3LO and 3AR on both sides of Port Phillip Bay at the edge of the water. Simultaneous checks on the station output were obtained by separate intensity measurements at a fixed point, and large-scale ordnance maps were used to make

* *Proc. Camb. Phil. Soc.* **23**, 288 (1926).

sure that the measuring sites were in line with the transmitter. Three separate lines from each of 3LO and 3AR were taken, and the length of sea water varied from 31·6 to 35·1 miles.

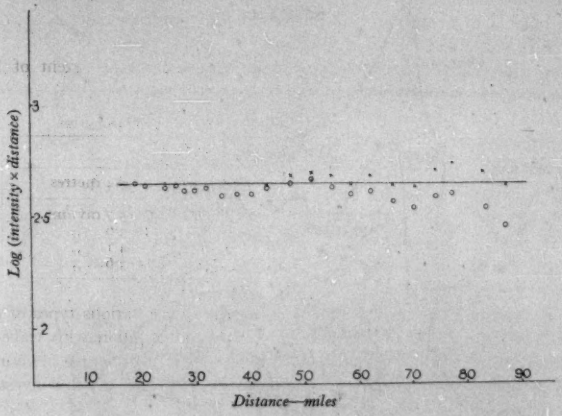

Fig. 5. Transmission over sea.

In every case the product Ed for $\lambda = 371$ metres showed a decrease, the mean being 5 per cent., while for $\lambda = 484$ metres, the product increased, the mean value of the increase being 4 per cent. This increase is attributed to the return of the energy initially radiated upwards from the aerial. It may be mentioned here that a slight increase in the product Ed with a small increase in d has been found frequently, even in the case of flat land transmission, for both stations.

§8. RESULTS OF OBSERVATIONS ON 7ZL HOBART

The wave-length of this station is 516 metres and its rated power is 800 watts. The field-intensity contours for 10, 7·5, 5, 3 and 1 mv./m. are shown in Fig. 6. These are drawn in from about 140 observations. Nearly the whole of Tasmania consists of wooded hills and mountains, and the effect of these is seen plainly in the form of the contours. The effect of Mount Wellington (4000 ft.) to the west of Hobart is very marked. Only to the north of the station does the transmission take place over undulating country without very much timber. The conductivity of the ground in this direction is $= 0{\cdot}5 \times 10^{-13}$ e.m.u. Tasmanian observations are in complete agreement with the conclusions stated for above Victoria.

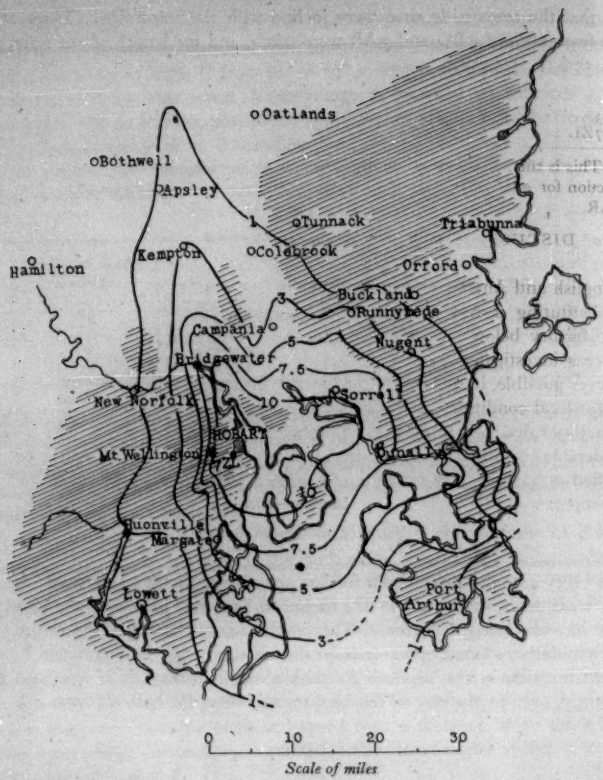

Fig. 6. Field-intensity contours of 7 ZL Hobart. Wave-length 516 metres, power 800 watts.

§ 9. EFFICIENCY OF THE AERIALS OF 3LO, 3AR AND 7ZL

The intensities at points on a circle of small radius d km. from the transmitter are averaged, giving the mean intensity at that distance, for which loss due to absorption will be negligible. If E volt/metre is this mean intensity, the power radiated by the aerial is

$$P = E^2 d^2 10^5/9 \text{ watts.}$$

Table 5 gives this power and the corresponding efficiencies of the three stations investigated.

Table 5

Distance d km.	Mean intensity volt/metre	Power radiated watts	Input power watts	Efficiency
3LO 7·43	0·066	2680	5000	53·6 %
3AR 45[1]	0·0104	2430	5000	48·6 %
7ZL 20·8[1]	0·010	483	800	60·4 %

[1] This is the mean radius of the 10 mv./m. contour for these two stations, and a small correction for attenuation, based on a conductivity of 4×10^{-13} e.m.u., has been applied to 3AR.

§ 10. DISCUSSION OF STANDARDS OF SIGNAL INTENSITY FOR AUSTRALIAN CONDITIONS

English and American authorities mention the value of 10 mv./m. of intensity as constituting a continuously reliable service. Also that reception becomes unsatisfactory below 2·5 mv./m. From information supplied by local people in the areas investigated during the present work, and from personal observation wherever possible, it appears that the service limits quoted above are unnecessarily high for local conditions. Crystal sets were found in use down to 2 mv./m., while satisfactory valve reception was possible below 1 mv./m. Below 2 mv./m., however, considerable variations at night were noted, although not sufficiently large to be classified as "fading" by the local listeners. The signal never became so small as to disappear entirely.

The results of a series of transmission tests held by 3LO and 300 amateur listeners in Victoria, in February, 1928, showed that (i) fading, although not serious, was observed in many districts within 100 miles of Melbourne. The intensity at most of these places has since been found to lie between 1·5 and 0·5 mv./m. (ii) Within this area, atmospherics did not cause serious interference. (iii) Reception was unsatisfactory, in general, at places over 100 miles from Melbourne, except in Gippsland, to the east, where reception was only satisfactory up to 60 or 70 miles. The intensity at these distances is seen from the map to be less than 0·5 mv./m.

We conclude therefore that the area of satisfactory reception extends to the 2·5 mv./m. contour and that no serious interference is present down to 1 mv./m. No measurements of the "noise level" or of the prevalence of atmospherics have yet been made in the south-eastern part of Australia, but the facts quoted above indicate that both these forms of interference are probably less prevalent here than in England or America.

§ 11. ACKNOWLEDGMENTS

The greater part of the cost of this investigation was defrayed by a grant made to the University of Melbourne by the Broadcasting Company of Australia, and some of the equipment was made available by the Radio Research Board of Australia. In conclusion, I wish to express my thanks to Professor T. H. Laby for his advice during the whole course of this work, and to the staff of the Research Section of the Postmaster-General's Department for their co-operation in some of the tests.

DISCUSSION

Mr R. A. Watson Watt (who read the paper in the absence of the author). I have an uneasy feeling that the author has made unjustifiably heavy concessions to portability in his apparatus. In aiming at a high step-up $\omega L/R$ for the field loop he has been driven to working the loop dangerously near its natural frequency, and to using a tuning capacity less than three times that of the voltmeter. I would suggest that this capacity ratio has an additional disadvantage in vitiating the attempt to eliminate antenna effect. The addition of $30\,\mu\mu$f across one half of a loop tuned by a capacity of the order of $85\,\mu\mu$f should have been compensated by an equal capacity across the unused half. The discrepancy between half-loop and whole-loop e.m.f.'s (p. 197) is probably due in part to this omission. It would, too, have been desirable to include provision for measurement of loop resistance in the field, a provision which has been found necessary in most measuring sets. The very wide departure of the E/V ratios from the square of the wave-length ratio is disturbing in this connexion, for $107/86 = 1{\cdot}24$ while $(484/371)^2 = 1{\cdot}70$.

As regards the voltmeter, the unorthodox use of the valve cap and socket as a grid leak leads to a fear that the need for calibration at each measurement may have been mainly due to the variations of this composite resistance of large surface. Further information on the gain in sensitivity as compared with a well-insulating socket and cap and a leak of, say, 10 megohms, would be of interest, as would also information on the mode of action of such a rectifier. Mr W. G. Hayman, of the University of Western Australia, who is at present attached to the Radio Research Station, Slough, has been good enough to make for me some tests on a voltmeter set up in accordance with the author's figures. With a P.M.1.H.F. valve taken at random from stock, an arbitrarily chosen signal of frequency 1 megacycle per second gave a decrease from 0·7 ma. to 0·3 ma. in anode current with the author's free grid arrangement. The grid pin was then isolated by the cutting of the valve cap while the socket was left unaltered, and the anode current change for the same applied signal was now from 0·6 ma. to 0·06 ma. With a 5 megohm leak across the grid condenser the current changed from 0·56 ma. to 0·065 ma. on application of the signal. It would thus appear that it is more important that the grid leak proper should not be swamped by casual leakage than that it should be superseded by such leakage.

The grid current in the case last described was certainly under $0{\cdot}02\,\mu$a. The mechanism of the moderately effective rectification obtained under these conditions, with i_a/e_g and i_g/e_g characteristics as in the accompanying diagram, is not clear. In any case it is difficult to see how a grid-bend rectifier can absorb less power than an anode-bend rectifier properly designed for mitigation of Miller effect; that this is a grid-bend rectifier is shown by the *decrease* of anode current on application of signal.

The phrase "field too small for satisfactory reception" (p. 198) should, of course, be qualified by the insertion of "domestic" before "reception".

It might with advantage be pointed out that the relative attenuations on two wave-lengths shown in Table 3 are roughly consistent with Sommerfeld's analysis and with Barfield's conception of a "pseudo-ground-conductivity" σ, which includes absorption by trees. The ratio of attenuation on the two wave-lengths over the same terrain is, in each case, close to the ratio of the squares of the wave-lengths. Sommerfeld's "numerical distance" is, to a crude approximation, and for considerable distances, inversely as the square of the wave-length. This relation does not, however, hold in Table 4, which might well be expanded from its present somewhat obscure form. § 9 might be amplified to show that a specialised definition of efficiency is implied in the assumption that the polar diagram of radiation is a torus of zero aperture.

I do not think that the author's conclusion as to relative noise levels in Australia and in other places can be supported by the combination of psychological and physical evidence which he adduces. It is probable that the Australian listener is more tolerant in respect of noise level than is the hypothetical listener portrayed in the very stringent specifications of service field strengths used in Great Britain and America.

These criticisms are, however, of but slight importance as against the great value of having such measurements, in another hemisphere, put before us in a form which shows very satisfactory agreement with measurements in Europe. As a worker for the Radio Research Board in Great Britain, I appreciate the privilege which I enjoy to-day of deputising for a colleague working for the Radio Research Board of Australia.

Mr R. L. Smith-Rose. Mr Cherry's paper is interesting, both from the practical point of view in providing data which are useful to engineers responsible for the design of broadcast transmitting stations, and from its more scientific aspect in providing additional knowledge of the electrical constants of the earth at radio-frequencies. The method adopted for making the field intensity measurements appears to depend, among other things, upon an accurate knowledge of the radio-frequency resistance of the receiving loop. It is perhaps doubtful whether this resistance can be measured with sufficient accuracy to warrant the statement on p. 198 that "the absolute value of the intensity is considered to be accurate to 1 %". This criticism does not, however, in any way influence the value of the results recorded in the paper, since a much lower order of accuracy would be adequate to illustrate the results and permit of the various deductions drawn therefrom.

I note that the conductivity obtained from the measurements made for open ground conditions in Australia is of the same order as that obtained by Mr Barfield and myself in some measurements carried out in the south of England a few years ago, by an entirely different method*. This value of the conductivity is decreased when the ground is covered by trees, as was shown by Barfield's later work which is referred to by the author. The much lower conductivity values obtained by Mr Cherry are undoubtedly due to the greater density of trees prevailing in certain

* *Proc. R. S.* **107**, 587 (1925).

parts of Australia. Similar results have been obtained by Lemoine and Rolf in the course of their investigations of the field strength from broadcasting in Sweden.

With reference to Fig. 5 of the paper, it may be pointed out that there appears to be a definite undulation in the course of the results there plotted. This undulation appears to be sufficiently regular to warrant the dismissal of the idea that it is due to unsystematic experimental errors. No reference to this fluctuation is made in Mr Cherry's paper, although I notice that in one of his previous publications† he has drawn attention to this characteristic of the graphical representation of the results of field intensity measurements obtained for transmission over sea water. In the paper referred to the author suggested that the fluctuation was due to the interference of two waves, one of which was transmitted along the earth's surface, while the other was due to reflection at the lower boundary of the Heaviside layer. Since all the measurements were made in daylight, and it is known that the intensity of such a downcoming wave is practically negligible for the wave-lengths in question, it is unlikely that this will provide a true explanation. I should like to suggest as an alternative that the second wave is obtained by reflection from the coast line on one side or the other of Port Philip Bay. At a distance of about 30 miles from the transmitting station, the ship on which the measurements were being made would be somewhere in the neighbourhood of St Leonards, and it is just possible that at this point direct and reflected waves would be obtained with a path difference sufficient to provide the ripple having a "wave-length" of about 5 miles, as indicated in Fig. 5 of the author's paper. At a distance of 50 miles the ship would be well outside Port Philip Bay, and it is likely that the distance between the points at which the two waves come into phase would be greater. This is consistent with the results recorded in Fig. 5, since the distance between the peaks increases to 11 and 16 miles as the distance of transmission is increased from 50 to 80 miles. The possibility of the reflection of waves at a coastal boundary has been pointed out previously in connexion with some experiments on wireless direction-finding carried out in this country*, while confirmatory evidence has also been provided from the results of some experiments in Palestine†.

It is a well-known occurrence in wireless direction-finding that when the path of transmission lies nearly parallel to the coast line, a deviation of the waves may take place of the order of 3° or 4° on wave-lengths in the neighbourhood of 450 metres. Where such refraction effects are observed it is reasonable to suppose that reflection may also take place. The effective coefficient of reflection will be small for the usual values of the conductivity of land and sea water, but this is consistent with the small amplitude of the ripple shown in Fig. 5 of the paper.

If such experimental evidence of coastal reflection could be confirmed by the carrying out of further experiments, a very useful purpose will have been served, since at the present time there is some discrepancy between the experimental data on coastal deviation and the theoretical explanation thereof. Perhaps the author

* *The Daylight Transmission of Wireless Waves over Sea Water* (Broadcasting Company of Australia, 1929).

† *Nature*, **116**, 426 and 498 (1925). ‡ *Nature*, **121**, 35 (1928).

could supply some further information or data obtained from his experiments which would assist in the further elucidation of this point.

Dr D. OWEN said that the last speaker's suggestion was an extremely interesting one, but the effect of coast-line reflection, if real, appeared to be of small magnitude. The fluctuations of intensity shown in Fig. 5 formed only a small percentage of the total intensity.

The PRESIDENT said that wireless engineers would be very glad that this work was being done in Australia. Designers would be helped by the knowledge that the effects on transmission of soils and land surfaces are of the same order in that continent as in England.

The author's reply will be published in a subsequent part of the *Proceedings*.

A METHOD OF EXAMINING STEREOSCOPIC PHOTOGRAPHS

By J. M. NUTTALL, D.Sc. and E. J. WILLIAMS, Ph.D.

Communicated by Prof. W. L. Bragg, October 22, 1929.
Read by Mr T. Smith and discussed January 24, 1930.

ABSTRACT. The paper describes a method of analysing stereoscopic photographs which has been found suitable for the examination of Wilson cloud tracks. The method consists essentially in replacing the photographic plates in the cameras, illuminating them and tracing out the common image which coincides in space with the object originally photographed. A system of movable pin points is used to trace out the contour of the image, the adjustments of the cameras being kept exactly the same as when the photographs were taken. The chief advantage of the method is its simplicity—practically no computation is required and it is not necessary to know the stereoscopic angle, the magnification, etc. The method has been used by the authors to measure the ranges of secondary β-rays and in investigations on the initial directions of emission of photoelectrons, and it has been found convenient and accurate.

§ 1. INTRODUCTION

In papers on "the ranges of secondary β-rays"* and "the spatial distribution of photoelectrons"† and in a letter to *Nature*‡, we have briefly referred to our method of examining stereoscopic photographs. The method was developed originally as the result of a suggestion by Professor C. T. R. Wilson, F.R.S., and is suitable for the examination of Wilson cloud tracks.

Calculations from direct measurements of the photographs have been found to give satisfactory results in the case of α-rays, where the tracks are approximately straight, as is shown by the work of Blackett. In the case of relatively slow β-rays, as, for example, those produced by X-rays, it is well known that the tracks have generally a zig-zag appearance, and calculations from such photographs would be both difficult and tedious. Moreover, in photographing β-ray tracks many observers use two cameras with their axes inclined at a small angle, instead of at right angles, and the difficulties of calculation in such cases are enhanced. Loughridge§ has described a special apparatus, the "stereocomparator," designed for examining such stereoscopic photographs.

During the last few years we have taken a large number of photographs of β-ray tracks. The method we have developed for examining them, which will be described in this paper, has proved both accurate and convenient.

* J. M. Nuttall and E. J. Williams, *Phil. Mag.* **2**, 1110 (1926).
† E. J. Williams, J. M. Nuttall and H. S. Barlow, *Proc. R. S.* A, **121**, 612 (1928).
‡ J. M. Nuttall and E. J. Williams, *Nature*, **123**, 799 (1929).
§ D. H. Loughridge, *Phys. Rev.* **30**, 488 (1927).

§ 2. THE PRINCIPLE OF THE METHOD

Fig. 1 is a plan of the two cameras drawn to illustrate the principle of the method. Suppose that we have a pair of stereoscopic photographs P_1 and P_2 from which we desire to ascertain the relative positions of points on the object photographed or the contour of the original object. The plates, after development, are replaced in the cameras which are adjusted so that they are in exactly the same condition as when the photographs were taken. The plates are illuminated by the lamps M_1, M_2 and are viewed from the right through the camera lenses L_1, L_2. Suppose I_1, I_2 are the images on the plates of a common point T on the object photographed, and let

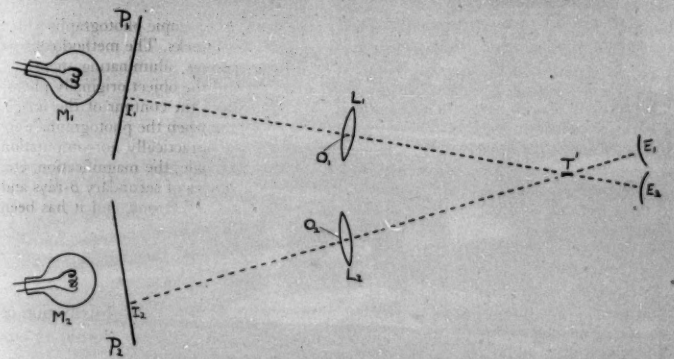

Fig. 1.

O_1, O_2 be the centres of the lenses L_1, L_2. Since the relative positions of the plates P_1, P_2 and the lenses L_1, L_2 are the same as when the photographs were taken, then the point T', which lies on both I_1O_1 and I_2O_2, has the same position in relation to the camera as the original point T of which I_1, I_2 are the photographs. Hence by ascertaining the point of intersection of I_1O_1 and I_2O_2 we can find the position of the original point T in relation to the camera. Similarly the positions relative to the camera of any number of points T_2, T_3, ... on the original object may be obtained, and hence their positions relative to one another. This is the essential principle of the method. If the images on the plates are diffuse through insufficient depth of focus, the method can still be used and will give accurate results provided the centre of the diffuse image be chosen as I.

§ 3. ACTUAL PROCEDURE IN PRACTICE

An arrangement of pin points A, B, C, ..., mounted on arms with universal movements*, is placed in front of the camera (see Fig. 2) roughly in the position of the original object photographed. In order to locate and record the position relative to the camera of a point T on that object (the point T might, for instance,

* Wireless dealers supply these for use with crystal detectors.

be the origin of a Wilson cloud track), one of the pin points is moved so that it coincides with the intersection of I_1O_1 and I_2O_2, where I_1, I_2 are the photographic images of T. The fixing of a point in this way may be done by alternate adjustment of the position of the pin on I_1O_1 and I_2O_2 until it lies on *both*. In practice, however, the fixing of a pin can be done in much less time than this method of successive

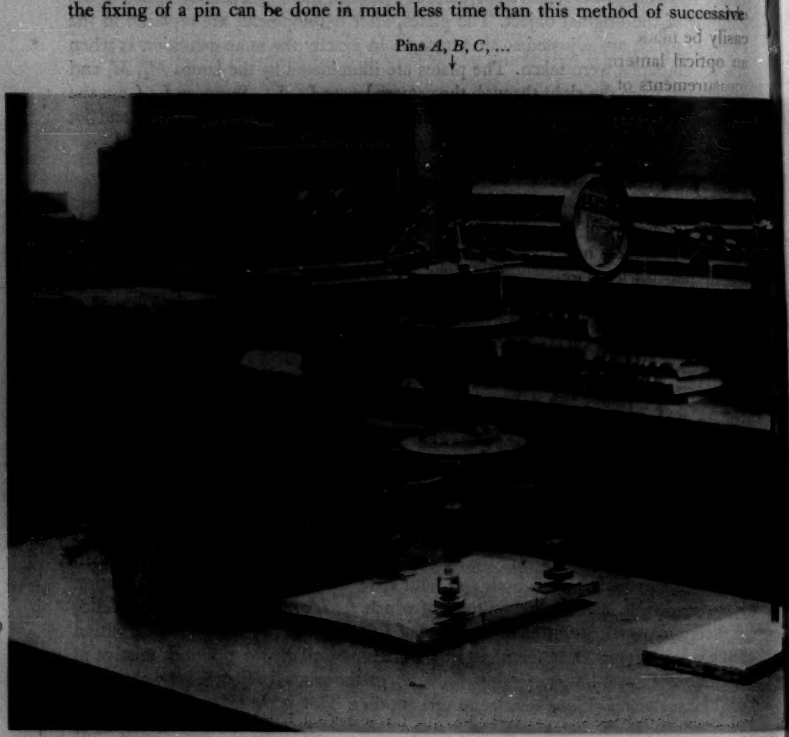

Fig. 2. Apparatus used in fixing the pins.

adjustment would require. Actually, by looking into one lens with one eye E_1 and into the other lens with the other eye E_2, an observer can readily see the photographs in stereoscopic relief, and a pin point can be brought into coincidence with the real image at the intersection of I_1O_1 and I_2O_2 as easily as it could be brought into contact with a point on a real object. The relative positions of a number of points on the original object may be recorded by the use of several pins, each pin recording the position of one point. In this way the contour of the object is mapped (for instance,

the contour of a Wilson cloud track), and when the pins have been "fixed" the essential process of translating the stereoscopic photographs of the original object into a spatial picture of it is completed.

Any information required about the contour of the original object may now be obtained from measurements on the pin points. Such measurements can most easily be made by projection of the images of the pins on to a screen by means of an optical lantern. In this way any desired magnification can be produced, and measurements of distances and angles which would be difficult to make on the pin points themselves become relatively easy. For this purpose it is convenient to have the pins mounted on a rotating table (a graduated spectrometer table serves admirably) so that the line joining two pin points may be brought into any desired plane relative to the direction of projection by rotation of the table*.

Owing to slight variations in the sizes of the photographic plates the fit of a plate in the holder of the camera may be slightly loose, and even though the adjustment of the camera may be the same as when the photographs were taken the plates are not necessarily in exactly the same position as at that time. This defect can easily be corrected if a reference point R, fixed at a known distance from the camera, is photographed at the same time as the object investigated. Then when the plates are being examined a pin point can be placed in the known position of R and the plates moved until the position of the point fixed from the photographs coincides with the known position. The plates are finally clamped in this position. This preliminary adjustment is not only necessary to make the reproduction accurate, but is essential if the photographs are to be seen stereoscopically at all, since stereoscopic vision is impossible if there is any relative displacement of the plates perpendicular to the plane containing the axes of the lenses. This adjustment has always to be made before proceeding with the measurements of the photographs as described previously.

§4. ADVANTAGES OF THE METHOD

The method which we have described in this paper has been used for the measurement of Wilson cloud tracks. The advantages of the method may be briefly summarised as follows: (1) Very little additional apparatus is required apart from the camera used in taking the photographs. (2) It is not necessary to know the stereoscopic angle, the magnification, etc., and the method involves practically no numerical or geometrical computation. Moreover, the effect of any glass apparatus between the camera and object photographed can be eliminated without calculation if such apparatus be simply replaced in the same position relative to the camera when the pins are being fixed. (3) Uncertainties due to slight variations in the stereoscopic angle, arising from the loose fit of the photographic plates in the holders

* Alternatively the contour of the object could be mapped by the use of a single pin point mounted so that it could be moved by graduated screws in any one of three perpendicular directions—a travelling microscope could be adapted to give the necessary movements. One could thus obtain the three spatial co-ordinates of any point by fixing this pin point on the point of the object to be located and reading the co-ordinates of the pin from the graduated screws. This method might be specially suitable for accurate measurements on α-particle tracks.

or looseness of the holders in the cameras, can be eliminated. This is most important when small magnification is used. (4) In view of the fact that the images can be seen in stereoscopic relief, the process of mapping out the contour of an object takes very little time. The zig-zag appearance of the Wilson cloud track of a slow β-ray is well known, and the measurement of its total length in space from stereoscopic photographs would seem a very formidable task. Yet in our work on the ranges of secondary β-rays* the range of a β-ray was measured in about 10 minutes. (5) The accuracy of the method is limited only by the definition of the photographs. In order to test the accuracy of the method we photographed a piece of fine copper wire about 3 cm. long which was bent into a zig-zag shape somewhat resembling a Wilson cloud track of a slow β-ray. The cameras had a stereoscopic angle of about 15°, the focal length of the lenses was 15 cm., the aperture was $f/5{\cdot}8$, and unit magnification was used. The bent wire was placed so that it pointed on the whole away from the camera, in order to test as far as possible the measurements depending on the stereoscopic effect. As the depth of focus was not great the photographs of certain parts of the wire were diffuse, so that the test was a stringent one in every respect. The developed plates were replaced in the cameras and the contour of the wire was mapped out with pin points from the stereoscopic photographs, as described above, and the total length of the wire was determined by measurement on the pin points. The original piece of wire was then straightened out and its length measured directly with a travelling microscope. The length measured indirectly from the photographs agreed within about 1 per cent. with the length measured directly with the microscope.

§ 5. APPLICATIONS OF THE METHOD

Photography with two cameras at right angles is a special case of stereoscopic photography and the method is therefore still applicable in principle. But in the case of photographs taken at right angles the fixing of a pin point cannot be facilitated by stereoscopic vision, and this part of the process will therefore take a longer time.

We have used the method outlined in this paper for the measurement of the ranges of secondary β-rays, the initial directions of emission of photoelectrons, and the nuclear deflections of β-rays. In all cases we have found the method convenient in practice and accurate.

§ 6. ACKNOWLEDGMENT

Finally we wish to thank Prof. C. T. R. Wilson, F.R.S., who originally suggested we should try the method, and Prof. W. L. Bragg, F.R.S., for his help and encouragement in the various researches involving its application.

NOTE ADDED JANUARY 22, 1930

Further checks on the accuracy of the method have been made by one of us in collaboration with Mr F. R. Terroux at the Cavendish Laboratory, Cambridge, and an account of the results was given in a recent paper by these authors†.

* *Loc. cit.*

† E. J. Williams and F. R. Terroux, "The Passage of fast β-particles through Gases," *Proc. R. S.* **126**, 294, 295 (1930).

DISCUSSION

Mr T. SMITH said that it was important that the lenses employed in the method should be free from imperfections, particularly coma, and that the centres of the pupils of the eyes of the observer should be placed and kept in coincidence with the projected centres of the lens apertures. Parallax methods of focussing, though almost universally recommended in text books, are carefully avoided in optical laboratories. A somewhat similar method has been used in air surveying*.

Dr L. C. MARTIN. It is quite true, as remarked by Mr Smith, that problems of this kind have been very thoroughly and successfully solved in connexion with air surveying, particularly in the "stereoplanigraph," an account of which has been published in the *Zeitschrift für Instrumentenkunde*. In this apparatus two perspective projections represented by photographs are mounted in "cameras" in positions related to the camera positions in the original exposures. The instrument then allows of the presentation of a stereoscopic view to the user and with the aid of a "wandering mark" a contour map of the country can then be semi-automatically drawn. Parallax observations are sometimes resorted to by persons who find a difficulty in holding the stereoscopic view; they will open one eye and then the other to see if there is any apparent displacement of the mark with respect to the image, but this is hardly likely to be a very accurate procedure.

AUTHORS' REPLY. Imperfections in the lenses are no more detrimental to our method of *examining* the photographs than they are to the *production* of accurate photographs, and the error arising from this source is of the same order of magnitude whichever method is employed for the examination of the photographs. In fixing a pin the observer uses the central rays through the lenses by adjusting the position of his eye so that the image is in the middle of the field of view. Errors introduced by the use of rays through the periphery of the lenses depend on the ratio of their apertures to their distance apart in the stereoscopic camera. As the diffuseness of the photographic images of point objects is due to the rays coming from different parts of the lenses an error of the same magnitude will be incurred if, in determining the original position in space of the points photographed, measurements are not made on the centres of their diffused images on the plates. The extent to which the conditions for accurate measurement by the present method can be adhered to is shown by the very satisfactory results quoted and referred to in the paper. The method used is in certain respects the same in principle as those used in air survey and it may be regarded as an extension or perhaps a simplification of such methods. In it a full-scale reproduction of the shape of the original object is made, and no optical apparatus in addition to the camera itself is used.

* J. C. D'Almeida, "Nouvel appareil stéréoscopique," *Comptes Rendus de l'Acad. Franç.* **47**, 61 (1858). T. Scheimpflug, "The use of the skioptikon for the production of maps and plans from photographs." *Photographische Korrespondenz*, **35**, 114 (Vienna, 1898). O. von Gruber, "The Carl Zeiss stereoplanigraph," *Zeit. für Instrumentenkunde*, **43**, 1 (1923). J. C. Kapteyn used the principle in determining the parallax of stars; several papers on this work were published about 1890.

A DETERMINATION OF THE COEFFICIENT OF DIFFUSION OF WATER VAPOUR

By W. E. SUMMERHAYS, B.Sc.

Communicated by Prof. S. W. J. Smith, December 25, 1929. Read and discussed February 14, 1930.

ABSTRACT. Water vapour was allowed to diffuse down a vertical tube about 2 in. in diameter, a steady concentration-gradient being maintained from approximate saturation at the top to approximate dryness at the bottom. The gradient was measured with two katharometers at a definite distance apart and the mass of vapour passing in an observed time was weighed. The result given is 0·282 cm.²/sec. at mean temperature 16·1° C.

§ 1. INTRODUCTION

No determination of the coefficient of diffusion of water vapour appears to have been made for thirty years, and I have been unable to find some of the original papers on the subject. In those determinations of which full particulars could be found the methods adopted seemed to be open to criticism, and it was thought that the accuracy attainable with a katharometer* used as a hygrometer would enable a method free from serious error to be found.

Two distinct methods were attempted, (1) a direct method, (2) an indirect method. Whilst the direct method gave consistent results with different experimental conditions, the indirect method had to be abandoned.

§ 2. THE DIRECT METHOD

A steady water vapour pressure-gradient was established along a vertical tube and the mass of water vapour that had passed in a known time was found. The gradient was measured by means of two katharometers inserted in the tube a known distance apart. It was considered essential that the diffusion should take place downwards as water vapour is lighter than air, so the top of the tube was covered with a damp surface and the bottom was kept dry with phosphorus pentoxide.

The apparatus consisted of a brass plate *A*, Fig. 1, 20 cm. in diameter and 1·5 cm. thick. This was placed on the base of a retort stand *R*, the shortened rod of which passed through a hole in the plate *A*: the rod thus constituted a vertical axis about which the plate could be rotated by means of a "tommy-bar" inserted in holes drilled in its edge. Two wells 6 cm. in diameter and 1 cm. deep were cut in the plate. A similar plate *B*, 1 cm. thick rested on *A*, and was prevented from rotating. In plate *B* one hole 4·85 cm. in diameter and one 6 cm. in diameter were cut. The larger hole was normally covered with a flanged brass cap *C*. Over the

* Cf. Daynes and Shakespear, *Proc. R. S.* A, 97, 237 (1920).

smaller hole was placed a copper disc E, 10 cm. in diameter and 0·6 cm. thick. This disc was perforated over an area 5 cm. in diameter with holes 2 mm. in diameter having centres 4 mm. apart. On the copper disc was placed the long tube, 4·85 cm.

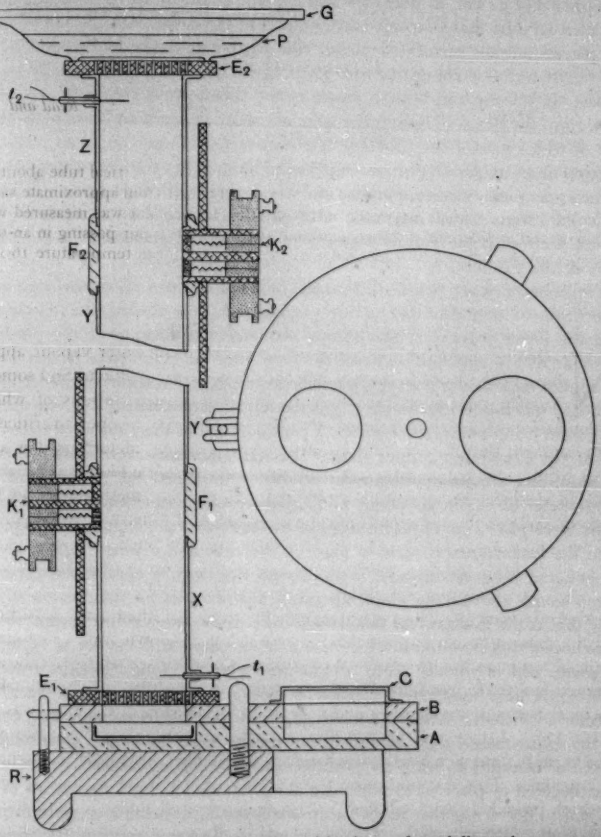

Fig. 1. Left: sectional elevation. Right: plan, with tube, perforated disc and cap removed.

in internal diameter, in which the gradient was established. This tube consisted of three pieces X, Y, Z, connected together by means of two brass collars F_1, F_2 each 4·6 cm. long into which the pieces were screwed. The top and bottom pieces of the tube were each 10 cm. long and were flanged at their free ends. The middle piece

was 12·5 cm. long for half the experiments and 17·5 cm. long for the other half. Through a hole in the side of each collar a katharometer K_1, K_2 was screwed and its position adjusted until its base was flush with the inside of the collar and tube. A copper disc 15 cm. in diameter and 0·3 cm. thick (to act as a cooling fin) was threaded on each katharometer and clamped to the collar. On the top of the tube was placed another perforated copper disc E_2, but this disc had wound round its edge eight turns of manganin wire (No. 29 s.w.g.) kept in position by means of shellac. In the top and bottom pieces of the tube a short side tube was soldered 2 cm. from the flange. These tubes were normally closed with brass plugs through each of which passed one junction of a copper-constantan couple t_1t_2. During an experiment an unglazed, porous soup plate P containing distilled water was placed on the upper perforated copper disc and was covered with a glass plate G. The tube and collars were coated internally with paraffin wax and all surfaces in contact (except the top of the porous plate) were ground and greased with a mixture of beeswax and vaseline.

Preliminary experiments were made, without the use of katharometers, with a short tube of large diameter, and it was found that the cooling due to evaporation from the lower surface of the porous plate and the heating of the phosphorus pentoxide due to absorption caused the bottom of the cylinder to have a temperature higher than that at the top. A small electric heater was put in the water at the top but it was found impossible to get the temperatures equal, the temperature at the bottom being always the higher. A consideration of these facts led to the adoption of the perforated copper discs. The katharometers were designed to take 0·35 amp. and a continuous current of this value raised the temperature of the katharometer blocks in air about 2° C. above room temperature; so the katharometers were placed on opposite sides of the tube and the copper fins were fitted. When the katharometers were in place in the tube and a water vapour pressure-gradient had been established, it was found that with a continuous current the thermo-couple showed that the temperature at the top of the tube was nearly 1° C. above that at the bottom. As this temperature difference was in the right direction it was hoped that it would not give rise to convection, and a series of experiments was made with a middle piece of the tube 17·5 cm. long. Results from these experiments were moderately consistent, giving a value of 0·32 cm.²/sec. for the diffusion coefficient. When the middle piece of the tube was made 12·5 cm. long and the experiments were repeated, the results were no longer consistent. The absence of convection being an essential condition for an accurate determination, it was decided to place in the cylinder along its whole length two strips of metal bent and soldered together to give a cross-shaped cross-section so as to divide the tube into four compartments. This metal was well greased and put in position and the value obtained for the coefficient was 0·28 cm.²/sec., showing that convection had been present before the introduction of the metal strips. It was therefore decided to pass the current through each katharometer for 30 seconds when a reading was required, and to take readings at intervals of not less than half an hour. When this method had been adopted it was found that the temperature at the top

of the tube was 0·3° C. lower than that at the bottom. By the passage of a current of 0·12 amp. through the manganin wire wound round the top copper disc *E* the temperatures at the top and bottom of the tube were sensibly equal (less than 0·1 C. difference).

To prevent the initial drop in e.m.f. that occurs when a current from accumulators is switched on, it was arranged that except during the 30 seconds when a reading was required the current should pass through a resistance approximately equal to that of either katharometer. The room chosen for the experiments was one in which temperature changes were slow, and it was found possible, by taking advantage of suitable meteorological conditions and by manipulating a very small circular window near the ceiling, to keep the room temperature constant within 0·2° C. during the whole experiment. When it was found impossible to keep the temperature within this limit the results of the experiments were rejected.

Calibration of katharometers. The katharometers were calibrated *in situ* by the pushing of air of known vapour pressure through the tube, the porous plate *P* being replaced with a ground glass plate. An aspirator of large capacity was filled with air from outside the building and was then made to push this air through a "saturator"* to the lower side-tube of the apparatus. A short rubber tube connected the upper side-tube to a "bubbler," so that the rate of flow could be observed and regulated. The saturator consisted of a copper plate 16 cm. square, on the under side of which a long spiral copper strip 1·8 cm. wide was soldered edge-on. This plate rested on the top of a large copper beaker containing water which reached half way up the spiral. Air was led into the spiral on the outside, and passed out through a tube in the centre. This central tube passed through a solid copper cylinder soldered to the top of the plate. The temperature of the air as it left the saturator was observed on a thermometer inserted in a hole drilled in the copper cylinder.

The rest of the top of the plate was covered with a thick piece of wood. An ordinary pail was lagged with felt and the copper beaker was placed on three corks on the bottom, the pail being then filled with water or ice. It was found that the temperature as shown by the thermometer could be kept constant within 0·1° C. by means of small pieces of ice dropped into the pail. A temperature about 2° C. below that of the tap water was the highest chosen. Readings of the katharometers were not considered satisfactory until they had remained constant for at least an hour and a half.

A "dry zero" for each katharometer was obtained by the passing of air from the aspirator through phosphorus pentoxide tubes and a long sulphuric acid tube. The dry zero readings are not of great value as it is found that with prolonged drying the katharometers register a gradually increasing dryness. The dry zero points shown on the calibration curves are those obtained after the passage of dry air through the apparatus for three hours, but all values of vapour pressure taken from the curves lie between points obtained with the saturator, or close to one of them. The effect

* This "saturator" was designed by Dr Shakespear and had been used by him in calibrating katharometers for some experiments on humidity.

on the katharometer readings of variations of atmospheric pressure were observed but were found to be very small and were sensibly equal in the two katharometers used; no correction was therefore necessary except in the calibration.

To start the experiment the porous plate *P* containing water and covered with the glass plate *G* was placed on the tube and a Petri dish containing P_2O_5 was put

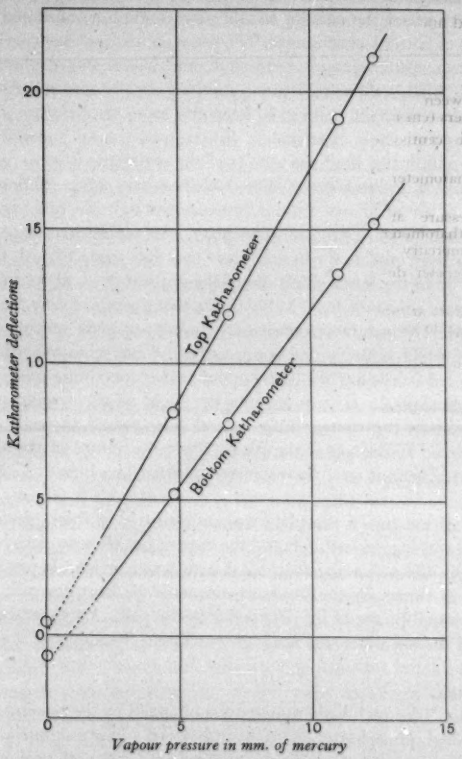

Fig. 2. Katharometer calibration curves for water vapour.

in the well under the tube. Another dish of P_2O_5 was placed in the other well and covered with the cap *C*. When the katharometer readings showed that the gradient was steady (after about two hours) another Petri dish of P_2O_5 was prepared and covered with a greased brass plate of the same diameter, and was carefully weighed. The plate adhered to the dish but could be detached by pushing it with a fine wire

through a small hole drilled close to its edge. The dish of P_2O_5 in the well under the cap *C* was removed and the weighed dish substituted and its cover detached. The cap *C* was replaced and the lower plate *A* rotated until the weighed dish of P_2O_5 came under the tube.

Readings of the katharometers were taken at intervals of about half an hour and the experiment was allowed to run for from about $\frac{1}{3}$ to $5\frac{1}{2}$ hours. The dish was then removed and reweighed and the diffusion coefficient calculated.

Table of Results*

Distance between katharometers (cm.)	15·2	15·2	15·2	15·2	20·0	20·0	20·0	20·0
Temperature centigrade	15·4	15·5	16·1	16·3	15·7	16·0	16·8	17·3
Bottom katharometer deflection	5·0	5·0	5·3	5·4	4·3	4·4	4·8	4·9
Vapour pressure at bottom katharometer in mm. of mercury	4·7	4·7	4·9	5·0	4·1	4·2	4·5	4·6
Top katharometer deflection	16·4	16·5	17·3	17·4	17·3	17·8	18·7	19·0
Vapour pressure at top katharometer in mm. of mercury	9·5	9·6	10·05	10·1	10·05	10·35	10·85	11·0
Difference in vapour pressures	4·8	4·9	5·15	5·1	5·95	6·15	6·35	6·4
Mass of water vapour in m.gm.	21·3	26·2	20·2	20·0	21·6	16·8	32·5	25·4
Time of experiment in min.	221	260	193	194	230	170	330	252
Coefficient of diffusion in cm.^{2}/sec.	0·274	0·281	0·282	0·278	0·284	0·289	0·280	0·285

Mean value of coefficient for 15·2 cm. distance: 0·279.
Mean value of coefficient for 20·0 cm. distance: $0{\cdot}284_5$.
Mean value of coefficient for all experiments : 0·282.
Mean temperature for all experiments: 16·1° C.

The experiments were made at room temperature and the results are for temperatures between 15·4° C. and 17·3° C. Four results were obtained with a distance of 15·2 cm. between the katharometers and four with a distance of 20·0 cm. They depend on the difference in vapour pressures obtained from the calibration curves, and the error possible here is thought to be not greater than 2 per cent.

An attempt was made to weigh the water evaporated as well as that absorbed. For this purpose a small unglazed porcelain disc was cemented near the bottom of a short brass cylinder 5·5 cm. in diameter. Water was placed in the cylinder which was then covered with a greased ground glass plate. The cylinder was placed on another greased plate whilst being weighed and was then placed on the tube after the porous plate had been removed. It was found that the loss in weight was some-

* The mass of 1 litre of water vapour at n.t.p. was taken as 0·806 gm.

times greater and sometimes less than the gain, though the difference was always very small and could be accounted for by variations in the amount of grease adhering to the lower surface of the cylinder. The possible disturbances set up in the tube by the changing of the water vessels led to the abandonment of further work in this direction.

For steady diffusion the coefficient K is given by the equation

$$K = \frac{\text{mass}}{\text{area} \times \text{time} \times \text{concentration gradient}}.$$

For the particular application considered here this equation becomes

$$K = \frac{\text{mass} \times \text{length} \times 760 \times 1000 \times (273 + t)}{\pi \times \text{radius}^2 \times \text{time} \times 0{\cdot}806 \times 273 \times \text{vapour pressure difference}}$$

where t is the centigrade temperature.

§ 3. THE INDIRECT METHOD

The close analogy that exists between thermal conductivity and diffusion suggested the possibility of finding the diffusion coefficient by the use of a mathematical equation previously worked out for conductivity.

It has been shown that if a semi-infinite solid be at a given temperature and its bounding plane be then kept at zero temperature, the time t taken for the temperature at a depth x to reach its half value is given by the equation

$$x/2\sqrt{kt} = 0{\cdot}477^*$$

where k is the coefficient of thermal conductivity. An attempt was made to use this equation to find the diffusion coefficient.

With slight alterations the same apparatus as that used in the preceding method was adopted. The porous plate and perforated copper discs were removed and a tube 52 cm. long flanged at both ends was placed on the top of the original tube and was closed with a glass plate. The bottom piece X of the tube was replaced by a similar piece 20 cm. long. A Petri dish containing water was placed under the tube and left until the bottom katharometer K_1 gave a reading rather less than that corresponding to saturation for the temperature of the room. The dish of water was then removed and the apparatus left until both katharometers gave readings that did not alter for one hour, thus indicating uniformity of vapour pressure throughout the tube. A dish of P_2O_5 was then placed under the tube and the time taken for the bottom katharometer to reach its half value was observed. The experiment was first done before the inside of the tube was coated with wax. The values obtained in this case varied between 0·18 and 0·22. After the coating of the tube with paraffin wax the values were much more consistent with a mean of about 0·23. It appeared that the method was invalidated by the adsorption of water vapour on the inside surface of the tube and also by the unavoidable lag in katharometer indications. It was therefore abandoned.

* Cf. Carslaw, *Introduction to the Mathematical Theory of the Conduction of Heat in Solids*, p. 34 (1921).

§ 4. ACKNOWLEDGMENTS

I have to thank Prof. S. W. J. Smith for giving me the opportunity of doing these experiments. I am very grateful to Dr G. A. Shakespear for suggesting the work, for much help and advice in carrying it out and for the loan of the katharometers and the saturator. I am also much indebted to the kindness of Messrs George Wade and Son of Burslem who supplied the porcelain discs, specially made for the purpose, free of charge; and to Mr W. Holland, of the physics workshop, for his skill in making much of the apparatus.

DISCUSSION

Mr J. H. AWBERY suggested that a third katharometer, intermediate between the two actually used, might have been introduced for the purpose of ascertaining whether the concentration gradient was constant throughout the length of the tube.

Mr R. S. WHIPPLE congratulated the author on the thoroughness of his work and suggested that temperature changes, which are the chief source of difficulty in this kind of investigation, might have been overcome by immersion of the whole apparatus in a bath of constant temperature—a method adopted by the Bureau of Standards.

Mr ROLLO APPLEYARD. With paraffin wax directly exposed to a damp atmosphere—as was found in the tropics with early forms of electrical condensers—there is a falling off in dielectric resistance, indicating the presence of moisture. I suggest that a material better suited to the purposes of the author could be found as a lining to the tube.

Dr D. OWEN. I feel some surprise at the observations of the last speaker. Experiments I have made have shown the small screening effect of an electric field due to the interposition of thin dried and waxed sheets of paper, and confirm the usual view as to the extraordinarily high insulating properties of paraffin wax, even in a moist atmosphere.

AUTHOR'S REPLY. The nature of the problem and the design of the apparatus made the adoption of a bath of constant temperature a matter of very great difficulty. If it had not been found possible to keep the room temperature within 0·2° C. some form of jacketing would have been attempted. In the direct method with a steady water vapour pressure-gradient adsorption of water vapour on the walls of the tube should not affect the results, as a state of equilibrium would be established.

A NEW TYPE OF DEWAR FLASK, FOR USE AS A CALORIMETER

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ABSTRACT. The water equivalent of a new type of Dewar flask, of which the inside was of copper, has been measured at laboratory temperatures, and found to be more constant that the usual type. The precautions necessary in using such a calorimeter are fully discussed.

§ 1. INTRODUCTION

This series of experiments was carried out to determine the order of accuracy to be expected from calorimetric experiments which were to be made with a Dewar flask and an electric heating coil. As previous observers had found considerable variation of the water equivalent of such an arrangement*, experiments were made upon a vacuum flask in which the inside glass vessel was replaced by a similar vessel of copper.

§ 2. PRELIMINARY EXPERIMENTS

Some preliminary experiments were made by Dr H. R. Lang in June 1928 with a vessel similar to that used in the present experiments. A stirrer which had a heating coil wound on it was caused to oscillate in a vertical direction. A charcoal pocket was introduced to keep the vacuum high, and it was found advisable to use the same mass of liquid in successive experiments; a correction was made for the heat due to stirring. It appeared that the water equivalent was related to the amount of electrical power supplied. The writer set out to investigate this relation more completely, with the ultimate intention indicated above. The temperature/time results obtained in these early experiments have been plotted (§ 7) and found unsatisfactory. In the same way, an attempt to formulate the thermal effect of stirring was not very successful, the values of the relevant constant A (§ 8) differing by 50 per cent. The apparent relationship between the water equivalent and the power input is due to (*a*) insufficient stirring and inaccuracy in the evaluation of the stirring correction, and (*b*) the loss of heat due to evaporation on the wetted surface of the oscillating stirrer.

* Experiments made by Dr H. R. Lang and Mr W. Goldsmith on an ordinary glass Dewar flask of about 800 c.c. capacity showed a sporadic variation of water equivalent of more than 20 per cent.

§ 3. THEORY AND OUTLINE OF METHOD

A heating coil is immersed in a liquid contained in the flask, along with a thermometer and a stirrer. The heat communicated to the liquid and calorimeter is evenly distributed by the stirrer, and the thermometer registers the increase of temperature of the calorimeter and its contents.

Elementary theory shows that if

K be the mechanical work done by stirring per sec. in joules,
M the mass of liquid in gm.,
s its specific heat in cal. per gm. deg. C.,
w the water equivalent of the calorimeter in cal. per deg. C.,
$(dT/dt)_m$ the rate of rise of temperature due to stirring only,
$(dT/dt)_e$ the rate of rise of temperature due to stirring and electric heating,
E the potential difference across the heating coil in volts,
C the current through the heating coil in amperes,
θ the observed temperature in degrees C. of the water bath,
T the observed temperature in degrees C. of the calorimeter,
t the time in seconds,
$\mathcal{J}$ the mechanical equivalent of heat in joules per calorie,
ρ_1 the cooling in deg. C. per sec. per deg. C. of temperature gradient during a stirring experiment,
ρ_2 the cooling during an experiment with current flowing,

then

$$K/\mathcal{J} = (Ms + w)\{(dT/dt)_m + \rho_1(T - \theta)\} \quad \ldots\ldots(1).$$

This can be written

$$K/\mathcal{J} = (Ms + w)/D, \text{ where } D = \{(dT/dt)_m + \rho_1(T - \theta)\}^{-1} \quad \ldots\ldots(2).$$

When electrical energy is supplied to the coil, this becomes

$$(K + EC)/\mathcal{J} = (Ms + w)\{(dT/dt)_e + \rho_2(T - \theta)\} \quad \ldots\ldots(3),$$

which can also be expressed thus:

$$(K + EC)/\mathcal{J} = (Ms + w)/F, \text{ where } F = \{(dT/dt)_e + \rho_2(T - \theta)\}^{-1} \ldots(4).$$

Therefore

$$EC/\mathcal{J} = (Ms + w)(1/F - 1/D) \quad \ldots\ldots(5).$$

Now if, during an experiment, $(T - \theta)$ passes from a negative value, through zero, to a positive value, the value of F (or D in the case of stirring) can be found when $T = \theta$. Hence errors due to heat loss should vanish. Even were this not so, if ρ_1 and ρ_2 are very small and almost, if not quite, equal, the error due to the neglect of the difference between the effects in the two experiments is negligible. Therefore from (5)

$$EC/\mathcal{J} = (Ms + w)\{(dT/dt)_e - (dT/dt)_m\} \quad \ldots\ldots(6).$$

§ 4. MEASUREMENTS

(*a*) *Electrical measurements*. The heating current was generated by 24 Chloride batteries, each of 40 ampere-hours' capacity, connected in two banks of twelve cells, and these two banks were then connected in parallel. In series with the battery was a variable manganin resistance, a standard resistance, an ammeter and the heating coil. A manganin dummy coil of the same resistance as the heating coil could be substituted for it by a two-way mercury contact. The current was passed through this dummy coil immediately before each experiment to enable it to become steady. The standard coil was of 10 s.w.g. manganin, immersed in a water-cooled oil bath. The resistance of the coil was 0·9961 ohm at 20° C. and its temperature coefficient negligibly small.

The potential differences across the standard resistance and the heating coil were measured with a calibrated 4-dial potentiometer in terms of standard Weston-cadmium cells. A slab of bakelite, in which were mercury cups, enabled the various connexions to be made rapidly, a connector being placed in three different positions for the three quantities required.

(*b*) *Thermometry*. The temperatures of the water-bath surrounding the Dewar vessel, and of the vessel and its contents, were taken with a platinum thermometer in conjunction with a Callendar-Griffiths bridge, which was carefully calibrated in terms of the largest coil. The thermometer had a fundamental interval of 4·775 ohms. The temperature of the laboratory was taken with a mercury thermometer, graduated in half-degrees; the ice and steam points were found to be at + 0·7° C. and 100·1° C. respectively, when corrected; readings of the mercury thermometer have been corrected accordingly.

(*c*) *Weight and volume*. The volume of water necessary to fill the calorimeter up to 2 cm. from the top was found by trial, and a file mark corresponding to this volume was made upon the neck of a flask. The flask was weighed with calibrated brass weights when filled to the mark and also after the contents had been emptied out. The volume of water used was 559·2 cc. at 17° C. It was found that, at the same temperature, the same mass of water could be measured off by volume to 0·2 gm. In most cases, the water was measured off in this way.

(*d*) *Time*. The rate of rise was measured as described below (§ 6). The standard clock marking seconds on the chronograph was rated against the broadcast Greenwich time signals. It was found to have a variable rate, from zero to a loss of 20 seconds a day. This error, though large for such a clock, would still not be sufficient to affect the results. Other times were taken with a ship's chronometer.

§ 5. THE CALORIMETER

The Dewar flask was about 50 cm. long and 5 cm. in diameter, and had a capacity of about 800 cc. The usual inside of glass was replaced by a copper vessel of similar shape and size, the joint being made air-tight with sealing wax. The glass was made by the National Glass Industry of London. The joint (*R*, Fig. 1) was about 1 cm. from the top of the flask and an equal distance above the level *V*

of the water inside, when the apparatus was completely assembled. The exhausting tube at the bottom *A* was connected to a Hyvac pump and to a discharge tube and charcoal pocket. The space was first evacuated by the Hyvac pump, and more completely by the charcoal pocket in liquid air. In less than 15 minutes a discharge

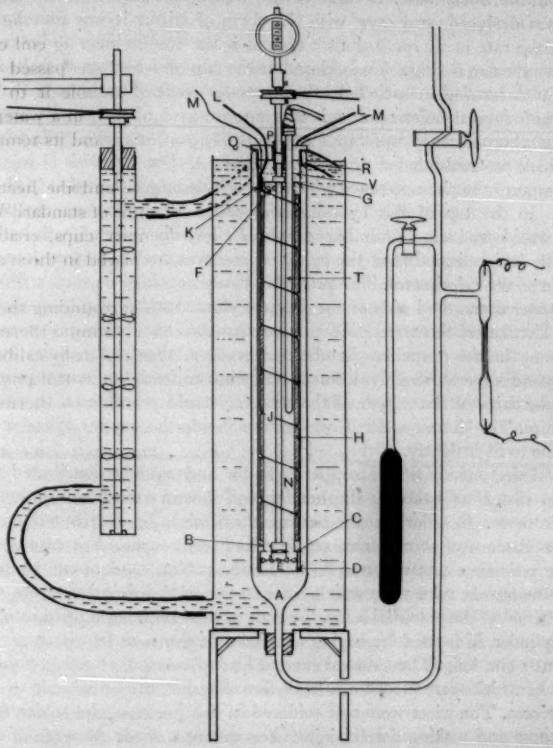


Fig. 1. Dewar flask with copper inner vessel, arranged as calorimeter.

coil capable of giving a 1 cm. spark in air was unable to produce a discharge across the discharge tube. This tube was about 16 cm. long and was of the usual type.

The modified Dewar flask was almost immersed in a lagged water bath, about 2 cm. being above the surface. The water was made to circulate by means of a pump driven by an electric motor. The temperature of the jacket did not change by more than 0·5° C. per hour on any occasion, and the variation was usually less than this.

The narrow and comparatively long shape of the calorimeter placed considerable limitations upon the form of the heating and stirring arrangements. Particularly was it difficult to find a piece of apparatus of the required compass which would ensure that the water was thoroughly mixed and the heat distributed uniformly throughout the whole mass. Several months were spent before an efficient stirring system was designed, and even with this form of stirrer it was found necessary to stir at the rate of 35 revolutions a second if the results were to be consistent. A copper cylinder B (Fig. 1) was closed at the top and bottom with brass plates G and C. A bearing in each held the propeller shaft. The bottom disk C was pierced with several holes so that it was a mere grid of metal. Equally spaced around its circumference were four supports D, portions of a cylindrical shell, inside which the multibladed propeller E revolved. The top disk G was coarsely serrated around the circumference which was a little larger than that of the copper cylinder. In the top of this cylinder, and about 1 cm. beneath the top baffle plate G, were bored a large number of holes F. The diameter of both baffle plates was a little larger than that of the cylinder, and was such that they loosely fitted the inside of the calorimeter. The propeller E was revolved in such a direction as to suck water down the inside of the copper cylinder and pump it up the outside. In each circulation the water had to pass through two baffle plates and suffer churning up in the propeller chamber. Moreover, should the water inside the copper cylinder have a slightly different temperature from that surrounding it, the high conductivity of the copper of the cylinder would promote an equalisation of temperature. The thermometer T was placed inside the copper cylinder through holes made to fit it tightly.

Around the outside of the copper cylinder and equally distributed were six long, thin ribs H of bakelite; the heating coil J was wound non-inductively in notches in these. In order that the heating effect might be distributed over a large volume, a distance of about 2 cm. separated adjacent turns. For ease in working later, the resistance was adjusted to be nearly 1 ohm, and about $1\frac{1}{2}$ metres of 20 s.w.g. manganin wire were employed. In order to minimise errors due to a wrong position of the potential leads, the ends of this coil were slipped into a hollow copper cylinder K formed by boring a hole to fit the wire in pieces of 12 s.w.g. wire about 1 cm. long. The external current leads L were of 16 s.w.g. copper wire, the ends of which were filed down until they fitted in the open ends of the connecting pieces. The wires were soft-soldered in this position, the solder filling the inside spaces and making a solid joint. The potential leads M were of 28 s.w.g. silk-covered copper wire. The covering was removed from that portion of the wire which was inside the calorimeter, and the wire was soldered to the end of the connecting piece K nearest the heating coil. All the bare wire in the calorimeter was then coated with an alcoholic solution of shellac and a current of some 5 amperes was passed through it so as to bake the shellac. Three coats were put on in this way.

The shaft of the stirrer N was in two parts connected by a bakelite sleeve P. This served to diminish the conduction loss along the shaft.

A lid of box-wood Q was pierced with holes to take the four leads, the stirrer

shaft and the thermometer. The holes for the current leads tightly fitted the wires, the rigidity of which kept the lid in place. The lid fitted the top of the calorimeter closely, and evaporation during an experiment must have been small. The stirrer was driven by a shunt wound motor, a controlling resistance being in series with the armature.

§ 6. METHOD OF MAKING AN EXPERIMENT

The pump circulating the water in the jacket was set going about 20 minutes before the experiment proper. The heating current was adjusted to the required value while passing through the dummy heating coil, and the measuring flask was filled with distilled water and cooled. The charcoal pocket was next immersed in liquid air, and by this time the temperature of the water bath was steady and its value could be determined, the time shown by the chronometer being noted. The temperature of the laboratory was observed immediately afterwards.

The measuring flask was removed from its cooling bath and the contents, which were about 10° below room-temperature, were emptied into the Dewar vessel. The stirring mechanism was inserted and the leads were connected to a row of four conveniently placed terminals. The platinum thermometer was inserted after drying, a pulley and collar were screwed on to the stirring shaft, and the revolution counter was fitted to the collar. The experiment was not proceeded with until the nature of the vacuum had been tested and found to be "black."

The stirrer was started and its speed adjusted by trial to the required value. At first some trouble was experienced in keeping the speed of stirring reasonably constant, until the original motor was exchanged for another of a modern type, made by the General Electric Company, of 1/10 h.p. This ran fairly constantly, the total variation at the slowest speeds being no greater than 3 per cent. and at higher speeds very much less. This variation was probably due in some measure to the fluctuating voltage of the mains from which its current was derived. The stirrer shaft revolved about three times as fast as the motor.

The current was switched over from the dummy heater, and the chronograph was set going. At some definite time, as read from the chronometer, the experimenter made a mark on the chronograph drum by tapping the key and allowing it to rise in unison with the half-second beats of the chronometer. In this way any particular record on the chronograph could be referred to events happening at the same time in other determinations.

The bridge was set to be balanced at a temperature corresponding to 15 cm. of bridge wire (about 3° C.) below the jacket temperature, and the chronograph key was depressed as the galvanometer spot passed through zero, which was set to be at the position of balance. The galvanometer had excellent zero-keeping properties and these were carefully preserved by fairly frequent reversal of the current when the deflection was at all large. Any change of zero was carefully noted at the end of each experiment; but this occurred to so small an extent as to render any error due to it very small. Such errors, unless they were progressive, which is doubted, were eliminated in the method of reducing the results. In this way, a

mark was obtained for each 5 cm. of bridge wire until the calorimeter had reached a temperature corresponding to 15 cm. above the jacket temperature (as observed at the beginning of the experiment).

Meanwhile, at intervals of a minute, the p.d. across the standard resistance and across the heater were determined alternately by a second observer. The reading of the counter was taken periodically. At the end of an experiment, the vacuum was tested again, and the jacket and laboratory temperature were redetermined, the time being noted.

The water in the Dewar flask was sucked out by a filter pump. The flask was then dried with a mop, some methylated spirits were poured in and mopped out, and the inside was finally dried by the blowing in of air until it appeared dry by inspection and there was no smell of methylated spirits.

§ 7. REDUCTION OF RESULTS

After removal of the record from the chronograph the whole minutes, indicated by a special mark made by the clock, were numbered from zero. By reference to the mark indicating a known chronometer time, the chronometer time corresponding to the zero minute mark was known. This was called "zero time." Hence if the chronometer time corresponding to any other mark was required, it could be obtained instantly by adding zero time to the chronograph time of that record.

In a similar way, the marks corresponding to each 5 cm. of bridge wire were given serial numbers commencing with 1, and these numbers were denoted collectively by the symbol G. The chronograph time for each of the marks was read off to the nearest tenth of a second, a procedure which was simplified by adjusting the chronograph so that 1 cm. of record corresponded to 1 second of time. The time interval between each successive pair of marks was obtained by subtraction, and the intervals were denoted by the symbol F. (In stirring experiments, the symbol D was used because the range of temperature between two marks was 1/10 that used in the main experiments.)

The intervals F as ordinates were plotted against the corresponding mean values of G as abscissae. The points were found to lie on a straight line (referred to as a G line) as required by the approximate expansions of equations (2) or (4); for G bears a linear relationship to T. Sometimes the points were very far from being collinear; this was almost invariably found to be the case when the stirrer was rotating at a speed lower than 35 revolutions a second. In most of the experiments described, the stirring was faster than this; other experiments were taken into consideration only if the G line was satisfactory. This linear relationship seemed to be a crucial test of efficient stirring, and was so regarded in these experiments. Typical G lines for (*a*) good, and (*b*) fair, stirring conditions are shown in Fig. 2.

The mean jacket temperature, during the period when records were being made, was calculated by simple proportion, and the G value corresponding to this temperature was worked out. This value of G when corrected for lag was that at

which the jacket and calorimeter temperatures were the same and was denoted by G_0. The corresponding value of F (namely F_0) was read from the G line.

But from equation (4)

$$F_0 = 1/(dT/dt)_e,$$

whence $(dT/dt)_e$ is known. Stirring experiments giving the value of $(dT/dt)_m$ were worked out in the same way. The reciprocals of F_0 were calculated and the stirring correction was subtracted. The difference was converted to platinum degrees and then to the gas scale, the mean correction over the range of temperatures used in

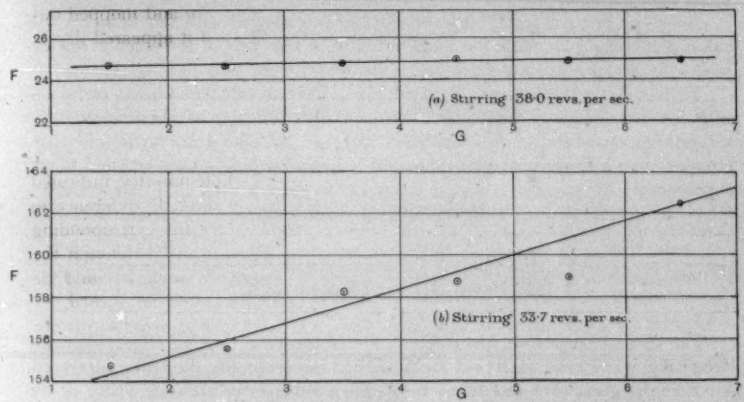


Fig. 2. (*a*) Above critical rate of stirring; (*b*) below critical rate of stirring.

the experiment being adopted. The electrical power supplied was calculated from the potentiometer readings on the assumption that the e.m.f. of the cells was 1·0183 volts. The change of e.m.f. with temperature over the relevant range was so small that a correction was not necessary. This fact and the value of the e.m.f. were verified experimentally by comparison with N.P.L. standards. The temperature variation of the heat-capacity of the volume of water used was allowed for, the weight being corrected for the buoyancy of the air. The mechanical equivalent of heat was taken as 4·180 joules per calorie.

§ 8. STIRRING

Experiments on the correction for stirring were carried out in the same way as the main experiments, except that the temperature interval over which the time to rise was taken was diminished to 1/10 or 1/20 of that in the main experiments. Much greater difficulty was experienced in obtaining consistent results for the rate of rise with stirring than was experienced when electrical energy was used. The rate of rise in cm. of bridge wire per second at the mean jacket temperature

was derived from G lines. It was found that this rate $1/D_0$ was connected with the rate of stirring by the law

$$A = D_0 r^n;$$

for, provided that the stirring was fast enough to mix the liquid thoroughly, when $\log D_0$ was plotted against $\log r$ the points were on a straight line, from the slope of which the value of n could be obtained. It was found that, in general, when the stirring was below 35 rev. per sec. (i) a G line could not be drawn accurately, if at all, and (ii) the stirring did not obey the law quoted or any other ascertainable law, being quite inconsistent.

Above 35 rev. per sec.*, however, the law was obeyed with fair accuracy, the value of n being 2·20 while the mean value of A was $3{\cdot}03 \times 10^5$ when r was expressed in rev. per sec. and D_0 in cm. of bridge wire per sec.

Table 1 shows the accuracy obtained, the correction calculated being compared with that obtained experimentally. Owing to the difficulty of obtaining reliable values experimentally, it is considered that the calculated correction is more accurate than a single observed value and it has accordingly been applied to the experiments.

Table 1. Stirring corrections

Rate, r.p.s. ...	31·0	33·8	34·4	35·2	38·5	43·0
Observed	0·0064	0·0068	0·0079	0·0089	0·0099	0·0131
Calculated	0·0063	0·0076	0·0079	0·0083	0·0102	0·0129

The stirring correction amounted to 1/5 of the total heating effect when the electrical power was 20 watts; but diminished in importance as the power increased. Accordingly, great care was taken to obtain a sufficiently accurate value of the correction.

§ 9. SOURCES OF ERROR

Heat loss. Errors due to this cause were reduced to a minimum by the careful maintenance of a high vacuum between the vessels of the Dewar flask, and by details in the design of and method of working the apparatus already described.

Lag of the thermometer. The platinum thermometer had an appreciable lag, though encased in the thinnest glass tube available. This lead to two errors in the value of F_0 (or D_0) as read from a G line: (i) when the thermometer registered the same temperature as the jacket, the temperature of the calorimeter was higher than this: hence the condition $T = \theta$ was not satisfied; (ii) since the lag was assumed to be proportional to the rate of rise of temperature, the amount of lag would change with F and the G line would be turned through an angle.

The first effect was found to cause an error of about 1 per cent. on the value of the water equivalent, but the error due to the second cause was about 0·1 per cent. and was therefore negligible.

The thermometer was cooled and then placed in the water bath which was at room temperature, and was continually stirred. A series of readings was obtained

* Occasionally satisfactory stirring conditions obtained at lower speeds than this.

with the galvanometer and chronograph. These were plotted on a time/temperature curve to test their regularity.

Elementary theory shows that

$$dT/dt = (\Theta - T)/\lambda \qquad \text{.....(7),}$$

where Θ is the temperature of the liquid,

T the temperature of the thermometer at any time t,

λ a constant (with dimensions of time).

We have also

$$\Theta = \Theta_0 + mt, \text{ where } m = d\Theta/dt.$$

By integrating equation (7) we have

$$\lambda = t/\log_e \{(T_0 - \Theta)/(T - \Theta)\} \qquad \text{.....(8).}$$

The values of t and $\log_e \{(T_0 - \Theta)/(T - \Theta)\}$ were plotted and λ was found to be 15·0 sec. The value of m was obtained from the appropriate G line and hence the product $m\lambda$. This gave the lag of the thermometer in centimetres of bridge wire, and was subtracted from the values of G_0 before the corresponding F_0 values were read from the G line.

§ 10. SUMMARY OF RESULTS

The values obtained for the water equivalent are given in the seventh column of Table 2, which gives also the experimental results from which the water equivalent was calculated.

Table 2. Water equivalent of Dewar flask

1	2	3	4	5	6	7
76·6	0·01240	35·70	29·2	18·23	19·0	130
77·4	0·01223	35·15	29·4	17·96	18·4	129
24·8	0·03976	114·7	38·0	18·93	19·4	131
33·7	0·02887	83·51	37·2	18·13	18·4	133
34·2	0·02840	82·12	37·5	18·25	18·6	133
44·0	0·02121	61·05	40·7	18·44	19·6	130
57·8	0·01614	46·41	38·8	19·24	20·4	129
70·6	0·01271	36·21	37·6	18·30	20·5	123
109·5	0·00764	21·99	36·2	19·60	20·4	130
65·2	0·01431	41·15	34·0	19·10	18·7	129
87·4	0·01023	29·43	34·5	19·29	19·5	129
71·6	0·01231	35·88	39·2	20·74	21·5	138
51·9	0·01809	52·43	36·7	19·46	18·3	134
40·0	0·02395	69·42	37·5	19·09	18·8	134

Column 1 gives the value F_0 as obtained from the G curve, in sec. per cm. of bridge wire; 2, the rate of rise of temperature due to electrical energy in deg. C.; 3, the electrical power used in watts; 4, the rate of stirring in rev. per sec.; 5, the mean water bath temperature in degrees C.; 6, the mean laboratory temperature in degrees C.; and 7, the water equivalent in gm. calories per degree C.

11. COMPARISON WITH RESULTS OBTAINED BY OTHER OBSERVERS

Griffiths* in his experiments on aniline found that the stirring correction was about 1/25 of the total heating effect. When water was used, he experienced trouble due to electrolysis; but his German silver wire was uncovered and there was a considerable p.d. across his heating coil. The resistance of the heating coil in the experiments herein described was constant to 0·001 ohm.

Griffiths† experienced considerable difficulty in his experiments upon J. His observation that the thermometer readings were not steady and that the water equivalent was inconsistent with low-speed stirring are confirmed by the present experiments. He used a water-motor to stir his calorimeter, and was troubled by its variable speed. In a somewhat smaller degree, the author experienced this difficulty, and this fact may account for the difficulty in obtaining really good stirring corrections. In the J experiments the rate of stirring used was of the order of 2000 rev. per minute, and the heat due to it varied from 1/6 to 1/10 of the heat due to electrical energy only. The heat due to stirring under the same conditions varied by about 5 per cent. Griffiths attributes this to the variation in the heat generated at the bearings. This may have occurred in the present experiments also, though there is no direct evidence of it.

W. R. and W. E. Bousfield‡ used a Dewar flask of much larger capacity, about 3000 c.c. A much larger amount of electrical energy was supplied to heat this up, yet only a single blade propeller revolving at 340 rev. per minute was used. The author has found that such apparatus does little else than cause the mass of water to circulate *en bloc*. Its large diameter renders it very susceptible to loss of heat by evaporation and similar causes, and thus makes for some uncertainty in the results. Indeed, the experimental evidence obtained by both Griffiths and the author casts considerable doubt upon the efficiency of the stirring and hence upon the accuracy of the results obtained. The water equivalent in the Bousfields' experiments was 1/6 of the thermal capacity of the calorimeter and its contents, and the greatest discrepancy in the water equivalent was about 1 per cent.

§ 12. DISCUSSION OF RESULTS

The experiments herein described show how important it is to ensure that the stirring is really mixing the contents of the calorimeter thoroughly. It appears that the water equivalent is independent of any of the quantities measured, and that the mean value of a number of determinations will accordingly be the most probable value. The experiments serve to demonstrate that, provided the necessary precautions be taken, the water equivalent can be measured with an accuracy of about 1 per cent., so that calorimetric experiments should be accurate to 0·5 per cent. or, better, the accuracy depending upon what fraction the water equivalent is of the whole.

* *Proc. Phys. Soc.* **13**, 234 (1894).
† *Phil. Trans.* **184 A**, 361 (1893).
‡ *Phil. Trans.* **211 A**, 199 (1911).

§ 13. ACKNOWLEDGMENTS

The author desires to express his gratitude to Dr H. R. Lang for much helpful assistance and advice, and to Mr R. Jessel, who assisted in the observations throughout the experiments. Most of the apparatus was made in the physics workshop, and the author's thanks are due to the Superintendent, Mr W. J. Colebrook, and the members of his staff, for help in design and construction.

DISCUSSION

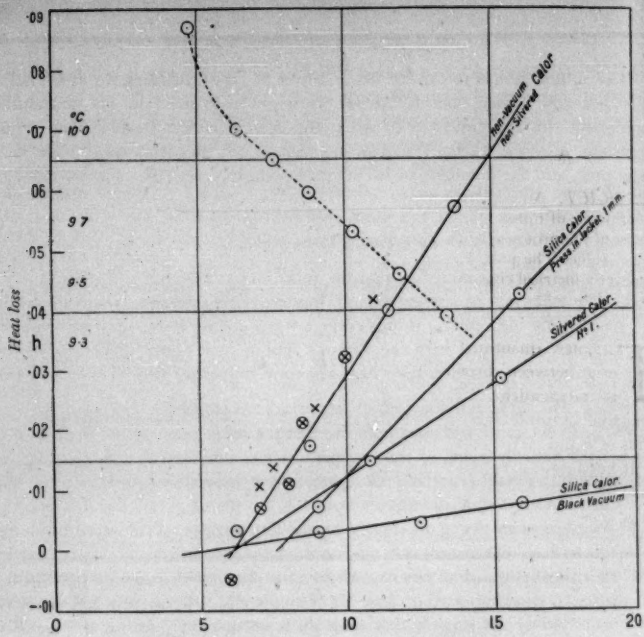
Mr R. S. WHIPPLE congratulated the author on a thorough and useful piece of work. The efficiency of stirring was an important requirement which the author had successfully met; failure in this respect was a common source of error in such experiments. He agreed with the author's finding that Dewar flasks are of little use for calorimetry; further, their heat-retaining properties vary very widely from one flask to another.

Dr H. R. LANG. I wish to emphasise that the inner vessel is not copper-lined, but is itself actually made of thin copper. It was intended that the sealing wax joint *R*, Fig. 1, should eventually be replaced by a fused copper to glass joint, such as is made for large wireless valves.

In many experiments on the specific heat of oils, made of recent years in America, Dewar flasks have been used, and, despite the frequent remarks by other observers as to the vital importance of efficient stirring, insufficient attention has been paid to this matter. I would go further than Mr Whipple and not only urge greater attention to the point, but suggest that some such tests as the plotting of "G" lines should be carried out in every case so as to ensure that the stirring is satisfactory.

Mr J. H. BRINKWORTH. Thanks are due to the author for this work which adds very materially to our knowledge of the vagaries in behaviour exhibited by vacuum vessels. May I emphasise the importance of always having an arrangement whereby the quality of the vacuum can be continuously observed during experimental measurements. To illustrate this I show some figures relating to heat losses observed in the determination of the specific heat of steam near 100° C. by the continuous flow method. The diagram indicates that in Callendar's experiment the glass vacuum jackets always cracked. I experienced the same kind of trouble when using glass calorimeters myself, and in consequence I designed the silica flow tube. It should be noted that the effect of silvering is quite unimportant when there is no vacuum.

With more special reference to the paper under consideration, it would be very interesting to know if sporadic variations in the value of the water equivalent would be observed if experiments were made on all-glass vessels in which the perfection of the vacuum was maintained: and would the water equivalent vary with the depth of water in the inner tube?



× Prof. Callendar's experimental values. Unsilvered calorimeters.
⊗ " " " Silvered "
⊙ Brinkworth. Mean values.

Diagram illustrating Mr Brinkworth's remarks.

AUTHOR'S REPLY. I agree with Dr Lang's remarks concerning the importance of actually testing the efficiency of stirring. Wherever possible, a quantitative test should be applied, for I have found that visual tests, such as the dropping of ink into water which is being stirred, are unreliable. It was because of Mr Brinkworth's work upon the heat loss from vacuum jackets that I maintained a high vacuum between the walls of the calorimeter. My experiments have shown the necessity for constantly testing the perfection of the vacuum. It is possible that the variation of heat-retaining properties among flasks of the same type, observed by Mr Whipple, may be due to variations in the degree of perfection of the vacuum.

I have no reliable evidence concerning the effect of the depth of water in the vessel upon the water equivalent, since this depth was fixed by the stirring apparatus. So far as I am aware, no reliable experiments have been made with an all-glass flask in which a high vacuum has been maintained.

THE MASSES OF THE PROTON AND ELECTRON

By H. T. FLINT, University of London, King's College.

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ABSTRACT. An application of a principle proposed by the author and described as the principle of minimum proper time is made to obtain an equation for the ratio of the masses of the proton and electron. The result obtained is in agreement with the experimental value. The possibility of explaining the asymmetry in regard to the masses by means of a metrical consideration is discussed.

The new quantum theory in its later developments indicates that relations exist between certain physical quantities which have hitherto been regarded as independent. This has led to a search for these relations and we have as a notable example Eddington's attempt to express the fundamental charge e in terms of h and c. We will consider here another relation.

It has been suggested* that the world line of an electron is to be regarded as made up of fundamental units of length of magnitude h/m_0c, where m_0 is the electronic mass; similarly the world line of the proton is composed of units of length h/M_0c, and that in the study of these fundamental masses no shorter length associated with their world lines will ever be revealed.

Perhaps the most convenient expression of this theory is by means of a principle of minimum proper time which states that in association with the electron (or proton) no proper time less than h/m_0c^2 (or h/M_0c^2) will be observed. This is, of course, different from the Bohr-Heisenberg uncertainty principle which states that the position of an electron can be determined as accurately as we wish but that the momentum can then be determined only to an order of accuracy given by the equation $\Delta q \Delta p \sim h$, where Δq denotes the error in the determination of position, Δp that in momentum. It has, however, been shown that in atomic dimensions the application of the principle leads to no contradiction to the uncertainty principle.

In a recent paper Fürth† arrives at the principle by using the uncertainty relation and modifying it by assuming that the electron cannot be located as accurately as we please. By means of a number of rather arbitrary assumptions he arrives at a value of the ratio M_0/m_0 and it is the determination of this ratio that we make the subject of the present paper. We shall approach it purely in the light of the principle of minimum proper time.

Fürth states that the value h/m_0c is to be regarded as the radius of the electron, but as this value is 2000 times the accepted value he inclines to the belief that the principle can apply only to neutral masses. There is no reason to regard h/m_0c as the electronic radius nor to limit it to neutral masses. The establishment of the

* *Proc. R. S.* A, **117**, 630 (1928). † *Phys. Zeit.* **30**, 895 (1929).

principle of minimum proper time shows that it is true for charged or uncharged masses and there is no suggestion that the fundamental length means more than a length along the world line. Fürth's results are, however, very interesting and we can remove his assumption.

Let τ_0 denote the minimum proper time h/m_0c^2. If it were possible for the mass m_0 to be converted to radiation we could presumably calculate the frequency of that radiation by means of the equation $m_0c^2 = h\nu_0$ and if the proper time is not less than τ_0 the radiation arising from this transformation of matter will have ν_0 as a maximum frequency. The proper time is the ordinary time in the system in which the electron is at rest, so that the frequency is calculated in that system.

It is not possible to contemplate this transformation in the case of an electron alone, since this would require the disappearance of charge, but we may suppose it to occur in the case of an electron and a proton. In this case we consider the two bodies as a single one of mass $(M_0 + m_0)$. The new quantum theory teaches that it is necessary to consider phenomena as possessing a double-sided character, one aspect being that in which we contemplate particles and in the other we contemplate waves; so that we can consider the problem as that of a particle of mass $(M_0 + m_0)$ and as that of radiation of a certain wave-length. The principle of minimum proper time applied to this union of electron and proton shows that the maximum frequency possible is ν_0 where

$$\nu_0 = (M_0 + m_0)\, c^2/h,$$

or the minimum wave-length is λ_0 where

$$\lambda_0 = h/(M_0 + m_0)\, c.$$

Now the wave-length in radiation is a unit corresponding to the unit along the world line of the particle. If we enquire what this unit could correspond to in the case considered, the suggestion at once arises that it must correspond to the distance between the centres of the electron and proton. Our method of approach to this point is rather different from that of Fürth but this suggestion is substantially due to him.

The older theories give as the value of the radius of the electron $r_0 = ke^2/m_0c^2$ and of the proton $R_0 = ke^2/M_0c^2$. The value of k depends upon the distribution of the charge, and the values $k = \frac{1}{2}$ and $k = \frac{2}{3}$ have been considered in the classical theory. If, however, the charge is regarded in the light of the new quantum theory it becomes difficult to consider an electron or proton as a sharply defined structure, and we can only speak of a radius as an "equivalent" one. This Fürth does, and with a rather arbitrary choice of equivalent radius he finds $k = 15/32$. We can, however, still speak of a distance between the centres of the two bodies and we may write for it the value:

$$d_0 = k\frac{e^2}{c^2}\left(\frac{1}{m_0} + \frac{1}{M_0}\right),$$

since the dimensions of the charges appear to be proportional to $e^2/c^2 \times$ mass.

We have thus:
$$\frac{h}{(M_0 + m_0)\,c} = \frac{ke^2}{c^2}\left(\frac{1}{m_0} + \frac{1}{M_0}\right)$$

or writing
$$\mu = M_0/m_0$$
$$\mu + (1/\mu) + 2 = hc/ke^2.$$

The occurrence of hc/e^2 is noteworthy. It occurs in Eddington's calculation of e and in the determination of the number of chemical elements by the application of the principle of minimum proper time; and we note that it is a pure number, having no dimensions.

We cannot proceed further to examine whether the equation gives the experimental value for μ unless we know k accurately. If we take the classical view and write $k = \frac{1}{2}$ or $\frac{2}{3}$, μ is of the right order but the value is too small. The result is, however, striking, for we have an equation in μ thus reducing the number of fundamental constants by one and the failure to obtain the correct value is explained by our ignorance of k. If we accept Fürth's assumption that the equivalent radius must be calculated on the assumption that it is the radius of the sphere within which all the charge must be enclosed so that it produces the same moment as the charge in the actual distribution assumed in the new quantum theory, $k = 15/32$ and the value obtained for μ (1838·2) is then in agreement with the experimental value (1838·3).

One of the most interesting and puzzling things in atomic physics is the asymmetry with regard to mass in the case of proton and electron. The question is, why is the positive charge associated with a mass so widely different from that associated with the negative charge? In theoretical considerations the asymmetry is very striking. In one line of attack, that of the five-dimensional theory, we meet with a constant α which may have the values $\pm e/m_0c$. Thus the theory seems to account for the occurrence of positive and negative charges, but there is no suggestion that m_0 has more than one value. It would be a great step if we could in some way relate m_0 and M_0, and this makes it worth while to consider attempts in this direction such as that given above.

We will consider the problem from another point of view, though the asymmetry is replaced by another. Nevertheless a change in the point of view may be useful. It has been suggested that atomic phenomena are indicative of a special kind of metric preferred in nature. In this we are following the way opened up by Weyl and Eddington in their inclusion of electromagnetism with gravitation into a space-metric system. We take the view that it is not in electromagnetic phenomena that the metric is to be found, but in quantum phenomena. Now, if we take the view that there is one standard of measurement for the proton and another for the electron, we can do without the introduction of a second mass, and retain one mass for both electron and proton. But we introduce two scales of measurement and so do not alter the number of constants. Some may prefer to introduce different masses directly rather than different scales, but there is more in the change than this, for the metrical view has certain other advantages. We can only decide which assumption to adopt when we discover that one idea is more fruitful than the other.

We know from the general theory of relativity that the space near a proton must be much more strongly curved than that near an electron and we can readily appreciate that the metric of space also might be markedly different in the two cases.

This point may be explained by reference again to the world lines of the proton and electron. These may be regarded as made of elements of lengths h/M_0c and h/m_0c. We have, as it were, two scales, one more finely divided than the other. Now the view under consideration is that this difference in magnitude is only apparent, the length h/M_0c in the proton space being regarded as equivalent physically to h/m_0c in the electron space. We have a kind of squeezing together of space in the proton relatively to that in the electron. The reason for this statement of equivalence of these two lengths is that in a parallel displacement in the proton region from one end of the element h/M_0c to the other we have the same change in length per unit length as in the electron space from one end of the element h/m_0c to the other.

If we judge the phenomena concerning electrons and protons from the same point of view and apply the same metrical considerations to each we shall conclude that a proton moves more slowly than an electron under the same conditions and so estimate its acceleration at too low a value and attribute to it greater inertia. We ought to regard the unit in the proton space not as of length h/M_0c but as h/m_0c, since the unit of length means physically so much more in that space. Thus our constant α is $\pm e/m_0c$, but with the change of sign we must change our metric.

There is another point. If this view is right the proton space is a kind of miniature of the electron space, and we should expect the radii of the two bodies to appear in the ratio of the units of the scales, i.e. inversely as their masses. This is in fact what is assumed about these bodies for, as we have seen,

$$r_0 = k\frac{e^2}{m_0c^2} \quad \text{and} \quad R_0 = k\frac{e^2}{M_0c^2}.$$

This view is offered as no more than a suggestion of a new way of looking at one of the problems occupying us nowadays.

DISCUSSION

Prof. W. Wilson: Dr Flint's paper contains a further application of a principle which he proposed some time ago; he has already made some important applications of it, one of the most interesting being an estimation of the upper limit for the atomic numbers of the elements. I think it is no mere coincidence that this turns out to be about 97, while the atomic number of uranium is 92. It is hardly surprising that the distance h/m_0c should have a special significance. A similar expression, namely h/mv, where mv is the momentum of the electron, is well known to represent the experimentally determined value of the wave-length of the waves associated with the electron.

As Dr Flint remarks, his principle differs from the Bohr-Heisenberg uncer-

tainty principle which asserts that the errors, $\triangle q$ and $\triangle p$, in the determination of the position and momentum of one electron, are governed by the relation

$$\triangle q \, \triangle p \fallingdotseq h.$$

I feel some doubt as to whether the latter principle is correct. It is true, of course, that any device for accurately determining the position of an electron at some instant will cause it to suffer a violent change of momentum; but what reason prohibits us from employing a device which gives us accurately the velocity (only) of the electron and, *while this is in operation*, from employing some other device, which is adapted to determine its position accurately?

The asymmetry in the masses of the electron and proton is one of the most provoking of physical facts. It is one of the things we should like to be able to trace back to some still more fundamental fact, and Dr Flint's tentative suggestions are of great interest.

Mr J. E. CALTHROP questioned whether the author was justified in adding together the masses of the proton and electron in his calculation, since there would be a change of energy involved in their near approach.

Mr I. O. GRIFFITHS. I listened with interest to Dr Flint's account of his deduction of a fundamental length connected with the electron. In Fürth's paper, to which he refers, a value of a certain expression is obtained which is 2000 times the accepted value of the radius of the electron. As this expression involves the mass of the electron linearly, it was perhaps natural to introduce the atomic mass in addition to the electronic mass, thus introducing a factor of approximately 1840 which goes some way to counterbalance the factor of 2000. The agreement which is claimed would however have been more striking if it could have been obtained with a simpler fraction than 15/32 for k.

Dr W. N. BOND. I should like to express my appreciation of this further attempt by Dr Flint to relate theoretically experimental constants that were formerly unrelated. The comparative smallness of the number 1838, where numbers of the order 10^{10} and 10^{-10} may be concerned, indicates that the relationship might be expected to be calculable theoretically. The calculation given in the paper deduces the relationship in a very interesting way, which I am not in a position to criticise. As regards the numerical value, according to Birge* the ratio M_0/m_0 has two values. In any case, numerical agreement to nearer than one part in 1000 cannot be significant, as it is limited by the experimental errors in the value of hc/e^2. Apparently the experimental value $2\pi\,(137{\cdot}29 \pm 0{\cdot}11)$ has been used by the author, not the value $2\pi \times 137$ suggested by Eddington. Personally I feel that there is now enough evidence to accept the latter value. However, all lack of exact numerical agreement could be attributed to the uncertainty as to the value of k; and the doubt about this value seems the only inelegant part of the paper.

* R. T. Birge, *Phys. Rev. Supp.* 1, 47, 48 (1929).

Capt. C. W. HUME. In attributing the difference in mass between the proton and electron to a change of metric, the author has invoked the increase in curvature of space-time in the neighbourhood of the proton. But is not the difference of mass already implicit in the difference of curvature? The proposed change seems to be only a verbal one.

Mr T. SMITH said that he felt the greatest admiration for those developments of modern physics which skirted the border-line between the physical and the psychical, but he felt that a dictionary of the terms employed was badly needed. The author has used a phrase "physically equivalent," for instance, which the speaker was quite unable to understand.

AUTHOR'S REPLY. It is not necessary to go into detail in this case with regard to the process of union of an electron and proton to form radiation. We must for our present purpose take account of the whole mass concerned, wherever it may be distributed.

The reference to the curvature in the neighbourhood of the proton was only by way of illustration; it is not desired to draw any conclusion from it, but only to point out that, as there are such great changes in the geometry in this region of space, we need not be surprised to find a metrical change also.

The meaning intended in the use of the term "physically equivalent" was that any change taking place in the electron region over the space of one unit length appropriate to this region may be regarded as equal to that which takes place in the protonic region along the unit appropriate to it. We may think of an observer attached to the electron and compare his experience with that of an observer attached to the proton. In the same electromagnetic field the experience of each would be the same along the units appropriate to them.

A HARMONIC ANALYSER

By J. HARVEY, A.R.C.S., B.Sc., City and Guilds Engineering College

Communicated by Dr E. Mallett January 28, 1930. *Read and discussed March* 14, 1930.

ABSTRACT. The paper describes a device for finding a given number of the Fourier components of a function by means of successive weighings.

§ 1. INTRODUCTION

FOURIER'S theorem states that if a function $f(x)$ is periodic in intervals of x equal to $2l$, then $f(x)$ can be expressed as the series

$$a_0 + a_1 \cos \frac{\pi}{l} x + a_2 \cos \frac{2\pi}{l} x + \dots$$

$$+ b_1 \sin \frac{\pi}{l} x + b_2 \sin \frac{2\pi}{l} x + \dots,$$

where $a_0, a_1, a_2, \dots b_1, b_2 \dots$ are constant coefficients.

If the form of the function $f(x)$ is not given, but the lengths $y_0, y_1, y_2, \dots y_{2n-1}$ of the $2n$ ordinates of the curve $y = f(x)$ for the values $0, \frac{l}{n}, \frac{2l}{n}, \dots (2n-1)\frac{l}{n}$ of x are known, a finite series

$$a_0 + a_1 \cos \frac{\pi}{l} x + a_2 \cos \frac{2\pi}{l} x + \dots + a_n \cos n \frac{\pi}{l} x$$

$$+ b_1 \sin \frac{\pi}{l} x + b_2 \sin \frac{2\pi}{l} x + \dots + b_{n-1} \sin (n-1) \frac{\pi}{l} x$$

of $2n$ terms can be found representing a function $\phi(x)$, which has the same values $y_0, y_1, y_2, \dots y_{2n-1}$ as $f(x)$ for the corresponding values of x.

Then
$$a_0 = \frac{1}{2n} [y_0 + y_1 + y_2 + \dots + y_{2n-1}],$$

$$a_r = \frac{1}{n} \left[y_0 + y_1 \cos \frac{r\pi}{n} + y_2 \cos \frac{2r\pi}{n} + \dots + y_{2n-1} \cos (2n-1) \frac{r\pi}{n} \right],$$

where
$$r = 1, 2, \dots (n-1).$$

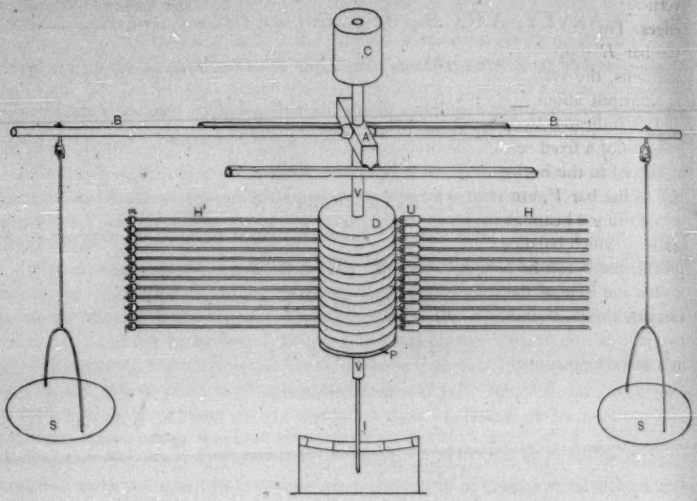
$$a_n = \frac{1}{2n} [y_0 + y_1 \cos \pi + y_2 \cos 2\pi + \dots + y_{2n-1} \cos (2n-1) \pi],$$

$$b_r = \frac{1}{n} \left[y_1 \sin \frac{r\pi}{n} + y_2 \sin \frac{2r\pi}{n} + \dots + y_{2n-1} \sin (2n-1) \frac{r\pi}{n} \right],$$

where
$$r = 1, 2, \dots (n-1)^*.$$

* See Whittaker and Robinson, *Calculus of Observations*.

By means of the harmonic analyser here described the summation of the series for the coefficients when $2n = 12$ can be effected. On the principle employed an instrument may be constructed for evaluating the series for the coefficients corresponding to any value of $2n$.



§ 2. THE INSTRUMENT

From a bar A, supported on horizontal knife-edges, a bar V is suspended which can rotate in a vertical plane called the principal plane, perpendicular to the knife-edges. Along the bar V there is a group of twelve ($= 2n$) circular discs D coaxial with the rod and supported by a plate P rigidly fixed to V. Each disc is capable of rotation about V. Fixed to each disc and projecting radially in opposite directions are two similar bars H and H'. The construction is such that any one disc may be turned about V into a position in which its bars H and H' make with the principal plane any angle which is a multiple of 30° ($= \pi/n$)*.

* There are several methods of achieving this. One such is as follows: On the upper face of the plate P and of each disc, twelve radial grooves at angles of 30° with each other are cut. In a small cylindrical hole parallel with V cut in each disc upwards from the lower face, a plunger plays against a fine spring at the upper end. The lower end of the plunger is wedge-shaped and can enter any one of the twelve grooves in the disc beneath and give a slight check to the rotation of one disc with respect to the other at every 30°. The check is just sufficient to keep the discs, with their bars when loaded, relatively fixed during free oscillations of the whole system about the knife-edges. The plungers are placed in such positions in the discs that all the bars H and H' can be brought together into the principal plane. The rotation of a bar from this plane through an angle which is a multiple of 30° can then be made.

On the bar H attached to each disc is a "unit mass" U symmetrically disposed about the axis of the bar, and having sufficient friction with the bar to keep it in any position in which it has been placed. When a unit mass is situated in a certain position near the disc, as in the diagram, called the zero position, the bar V being vertical, the force of gravity on the unit mass produces a moment about the knife-edges. This moment is counter-balanced by that produced by a mass m fixed to the bar H' opposite. Hence when the unit masses on all the bars are at zero positions, the system with the bars in any relative horizontal positions will be in equilibrium about the knife-edges and V will be vertical. The vertical position of V is indicated by the coincidence of the prolongation of V, as an index I, with the zero of a fixed scale.

Fixed to the horizontal bar A carrying the knife-edges, at right angles to them and to the bar V, are two similar arms B of the same length, opposite each other, constituting a beam as in a balance for weighing. From these arms are hung scale-pans, S, which balance with respect to the knife-edges. A counterpoise C movable on the bar V can be adjusted in position so as to give sensitivity to the system.

On the base of the supporting frame there is a mechanism by which the bar V carrying the discs and bars is fixed or released: by a horizontal movement of a lever two jaws are opened or closed on the bar, while a vertical movement of the lever in a certain position locks or unlocks the jaws.

§ 3. DETERMINATION OF THE COEFFICIENTS

The twelve unit masses are moved along the bars, taken in order, from the zero positions through distances $y_0, y_1, y_2, \dots y_{11}$, corresponding to the lengths of twelve equidistant ordinates of the curve $y = f(x)$ in the range $x = 0$ to $x = 2l$. The bars are then brought into the principal plane so that lengths representing positive ordinates are in one sense from V while those representing negative ordinates are in the opposite sense. The bar V will then, in general, be deflected from the vertical position, positively or negatively. The vertical position is restored by placing mass in one of the scale-pans. The moment of the weight of this mass about the knife-edges is equal to the sum of the moments of the weights of the unit masses, which is

$$y_0 + y_1 + y_2 + \dots + y_{11}.$$

Hence the mass added to the scale pan is a measure of a_0.

In general, to obtain a coefficient a_r, the bars in the order referred to are turned so as to make angles $0°$, $r \times 30°$, $r \times 60°$, $\dots r \times 330°$ respectively with the principal plane. Mass is then added to one of the scale-pans to maintain the bar V in the vertical position. The weights of the unit masses now exert about the knife-edges the moment

$$y_0 + y_1 \cos(r \times 30°) + y_2 \cos(2r \times 30°) + \dots + y_{11} \cos(11r \times 30°).$$

Hence the mass in the scale-pan is a measure of a_r.

To obtain the coefficient b_r the bars are kept in the same relative positions as for a_r, and the whole system is turned through a right angle about the bar V. If V is maintained vertical by the placing of mass in one of the scale-pans, the moment about the knife-edges of the weights of the unit masses is now

$$y_1 \sin(r \times 30^\circ) + y_2 \sin(2r \times 30^\circ) + \ldots + y_{11} \sin(11r \times 30^\circ),$$

and the mass in the scale-pan is a measure of b_r.

On a model actually constructed each unit mass is of 50 grams and has a range of movement of 16 cm. on the bar H. Ordinates may be set off by callipers to within 0·5 mm. Each arm B is 30 cm. long, so that a movement of a unit mass through 0·5 mm. along the bar produces a moment about the knife-edges which can be equilibrated by a mass of 0·08 gram approximately in the scale-pan. The instrument can be made sensitive enough to weigh to 0·2 gram. If the ordinates be measured along the bars in centimetres, the mass placed in a scale-pan to maintain the bar V in the vertical position should be divided by 10 to give the coefficient sought, except in the cases of a_0 and a_6 when the mass should be divided by 20.

§4. ACCURACY

To test the accuracy of the model, the twelve ordinates of the curve

$$y = 3 - \tfrac{3}{2}\cos x - 3\cos 2x + \tfrac{3}{2}\cos 3x + \tfrac{15}{4}\sin x + \tfrac{9}{4}\sin 2x + \tfrac{3}{4}\sin 4x$$

for the values $0, \pi/6, 2\pi/6, \ldots 11\pi/6$ of x were calculated. These ordinates were set off along the bars H of the instrument and the coefficients of the cosines and sines in the given formula estimated. The ordinates were reset and the process repeated. The results are as shown in the table.

Coefficient	By calculation from series	By instrument (1)	By instrument (2)
a_0	3·00	2·94	2·93
a_1	−4·50	−4·44	−4·43
b_1	3·75	3·73	3·68
a_2	−3·00	−2·94	−2·97
b_2	2·25	2·20	2·23
a_3	1·50	1·47	1·48
b_3	0·00	0·03	0·01
a_4	0·00	0·00	0·00
b_4	0·75	0·75	0·77
a_5	0·00	0·01	0·01
b_5	0·00	0·00	0·01
a_6	0·00	0·00	0·01

The instrument should be sufficiently accurate for the analysis of many types of geophysical and meteorological phenomena.

5. EXTENSION TO MORE THAN TWELVE FOURIER COMPONENTS

An instrument consisting of twelve discs and bars may be employed to give the coefficients of the terms of a sine and cosine series for a number $2n$ which is a multiple of twelve, if checks are provided so that the discs can be turned relatively through corresponding angles. Thus when $2n = 24$, twenty-four equidistant ordinates $y_0, y_1, y_2, \ldots y_{23}$ of the given curve over the range of periodicity are measured, and relative movements of one disc over another through multiples of $15°$, or $= \pi/12$, must be made. For the determination of a coefficient a_r, the unit masses on the twelve bars taken in order are moved distances $y_0, y_1, y_2, \ldots y_{11}$ from the zero positions. The bars are then turned through angles

$$0°, (r \times 15°), (2r \times 15°), \ldots (11r \times 15°)$$

respectively from the principal plane. A mass M_1 is added to a scale-pan to maintain the bar V in the vertical position. Next, the unit masses are moved so that they are at distances $y_{12}, y_{13}, \ldots y_{23}$ along the bars taken in the same order as before from the zero positions, and the bars are turned so that they make angles

$$(12r \times 15°), (13r \times 15°), \ldots (23r \times 15°)$$

respectively with the principal plane. Mass M_2 is added to a scale-pan to maintain V vertical. Then $M_1 + M_2$ is a measure of a_r. The coefficient b_r may be obtained similarly from the masses required to maintain V vertical when each of the two systems of bars, as arranged for a_r, is turned through a right angle.

§6. ACKNOWLEDGMENTS

The author acknowledges his indebtedness to Mr E. A. Nehan, Mechanics and Mathematics Laboratory, City and Guilds (Engineering) College, South Kensington, for constructing the model illustrated, and for the valuable advice and help which he has at all times generously given.

DISCUSSION

Mr J. H. AWBERY congratulated the author on his ingenious invention and asked how much time it would require for an analysis, as compared with the time taken by arithmetical methods. The data given in the paper showed that the results were sufficiently accurate for practical purposes.

Mr J. E. CALTHROP. Speaking from the point of view of a teacher of physics, I should like to congratulate the author on a piece of apparatus which should be of great use and interest in a laboratory for teaching purposes. I suppose the use of callipers could be eliminated by suitable graduation of the arms carrying the movable weights.

The PRESIDENT said that the instrument, by its direct attack on Fourier analysis, would give students a very clear view of the principles of the subject. Many persons

must have interested themselves in the problem, but probably the instrument shown at the meeting was the first that had been actually constructed, at all events with such excellent workmanship and accuracy.

AUTHOR'S REPLY. The bars carrying the movable weights could be graduated, but it would, I think, be more difficult to set off the ordinates along the bars by the graduations than by the aid of callipers. The pair of callipers, with its vernier, can be turned in the hand towards the light and the length of an ordinate set off on it accurately and rapidly.

In the estimation of the coefficients of the function given in the table in the paper, the time taken by using the instrument was under twenty-five minutes, a single weighing being made for each coefficient, and I do not think that much time would be saved by this method in comparison with certain arithmetical methods in the case of twelve ordinates.

A METHOD OF COMPARING SMALL MAGNETIC SUSCEPTIBILITIES

By R. A. FEREDAY, B.Sc., East London College.

Communicated February 26, 1930. *Read and discussed March* 14, 1930.

ABSTRACT. A method of the non-uniform field type is described. It possesses the specific advantage that the force exerted on the specimen is constant over a wide region of the field, so that a careful setting to a position of maximum force is rendered unnecessary.

§ 1. INTRODUCTORY

THE present communication gives an account of a method which has been developed at the suggestion of Prof. C. H. Lees, and in consultation with him, for the determination of small magnetic susceptibilities, and departs considerably from general practice in measurements of this type. The apparatus was needed for an investigation in which a high sensitivity was required when a specimen of but a few milligrams was being used, and for this reason, and since relative measurements only were required, a method of the non-uniform field type was chosen.

§ 2. THE NON-UNIFORM FIELD METHOD

The force due to a field H, acting in any direction s, on a small body of volume v and susceptibility κ, that of the surrounding medium being κ_0, is given by

$$f_s = (\kappa - \kappa_0)\, vH \frac{\partial H}{\partial s} \qquad \text{.....(1).}$$

Thus, if two bodies are brought in succession to the same part of the field, and the forces on them are compared, we shall have:

$$\frac{\kappa_1 - \kappa_0}{\kappa_2 - \kappa_0} = \frac{f_1}{f_2} \qquad \text{.....(2).}$$

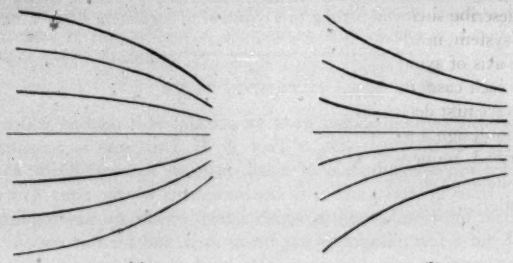
In the derivation of (1), it is assumed that the force on all parts of the body is the same, a condition which it is almost impossible to realise in practice with a specimen of reasonable size. Equation (2) will, however, hold accurately for larger specimens, provided they have the same size and shape, and are brought to the same position in the field*. The method is therefore much better suited for relative than for absolute measurements, but each specimen must be placed at the same point in the field, and this constitutes the chief difficulty of the method.

Curie† and later observers used the magnetic field between two inclined poles; the direction of the field gradient, and hence that of the force exerted on the

* Cf. Stoner, *Magnetism and Atomic Structure*, p. 39.

† *Ann. Ch. Ph.* **7**, 289 (1895).

specimen, being at right angles to that of the field itself. A curve is drawn showing the variation of force along the direction of the gradient, and it is found that at a certain point it passes through a fairly narrow maximum, on each side of which it falls off sharply. In the comparing of susceptibilities, each specimen is placed at this point of maximum force, and to secure this, the electromagnet producing the field is usually mounted on some form of carriage, so that it can be displaced relatively to the specimen until the latter is in the desired position. It is, however, by no means easy to impart micrometric movements to a heavy electromagnet, especially when the specimen is suspended in the interpole gap and must not be unduly disturbed.



(*a*) (*b*)

Fig. 1. Lines of force in converging fields.

Another form of the non-uniform field method was introduced by Wiedemann* but has not found favour with modern workers. In this method, the field, its gradient, and the force to be measured, are all directed along the axis of symmetry of the field, which is produced by the pointed pole of an electromagnet. A suitable field must be of one of the types of Fig. 1, which represents sections of the field by a plane passing through the axis of symmetry.

In the case of a field of type 1 (*a*), both the field intensity and its gradient increase from left to right of the figure; hence the force exerted on a body placed on the axis will be very much greater at the right than at the left of the figure, and if measurements were made in such a field it would be necessary to attend very carefully to the setting of the specimens. With a field of type 1 (*b*), on the other hand, the field decreases as its gradient increases, so that the variation of their product along the axis will be small. If these opposite variations could be made to balance exactly, so that $H \times \partial H/\partial x$ had a constant value at all points of the axis, then the force exerted on a given specimen would be independent of its position so long as it remained on the axis, and the great experimental difficulty of setting the specimen would entirely disappear. It is on this conception that the method to be described is based.

* *Pogg. Ann.* **126**, 1 (1865).

§ 3. THE MAGNETIC FIELD

In an attempt to realise a field satisfying the condition $H.\partial H/\partial x =$ constant along the axis of symmetry OX, the assumption was made that the pole pieces of the magnet could be used with such a field that the permeability of their material would be high; the pole faces would then very nearly be magnetic equipotential surfaces. The problem thus reduces to the determination of equipotential surfaces in a field satisfying the above condition. Further, since the field is symmetrical about an axis, it is only necessary to determine the curves of section of the equipotentials by a plane through this axis, and in the sequel, the term "equipotential" is used to describe such curves. Two systems of co-ordinates are used in the plane, one a polar system, in which the axis of symmetry is $\theta = 0$, and the other a Cartesian system, the axis of symmetry being taken as OX and a perpendicular at the origin as OY. In each case, the centre of the interpole gap is taken as origin. The equipotentials were first determined by a simple semi-graphical method, and the results were used to design a pair of pole pieces. With these pole pieces in the apparatus to be described, numerical data were obtained which served for guidance in the more accurate analysis, which is given in the next paragraph*.

The important constants determined experimentally were:

Field at origin (H_0)	...	...		1000 gauss.
$H.\partial H/\partial x$	...	...	...	3×10^5.

Since the field is symmetrical about the axis, the magnetic potential at the point (r, θ) may be expressed as

$$V = V_0 + A_1 r P_1(\cos\theta) + A_2 r^2 P_2(\cos\theta) + A_3 r^3 P_3(\cos\theta) + \ldots \quad \ldots\ldots(3).$$

For any point on the axis of symmetry, the Legendre polynomial $P_n(\cos\theta)$ is equal to unity, so that the potential at any point on the axis is given by

$$V = V_0 + A_1 r + A_2 r^2 + A_3 r^3 + \ldots \quad \ldots\ldots(4).$$

The field along the axis at the point $(r, 0)$ is therefore given by

$$\begin{aligned} -H = \partial V/\partial r &= A_1 + 2A_2 r + 3A_3 r^2 + 4A_4 r^3 + \ldots \\ &= H_0 + 2A_2 r + 3A_3 r^2 + 4A_4 r^3 + \ldots \quad \ldots\ldots(5) \end{aligned}$$

(H_0 being the field at the origin),

therefore

$$H^2 = H_0^2 + 4H_0 A_2 r + (6H_0 A_3 + 4A_2^2) r^2 + (8H_0 A_4 + 12 A_2 A_3) r^3 + \ldots$$

$$\frac{\partial}{\partial r}(H^2) = 4H_0 A_2 + 2(6H_0 A_3 + 4A_2^2) r + 3(8H_0 A_4 + 12 A_2 A_3) r^2 + \quad \ldots\ldots(6).$$

Now we require to satisfy the condition that $H.\partial H/\partial r$, i.e. $\frac{1}{2}.\partial H^2/\partial r$ shall be

* I am indebted to Prof. Lees for suggesting this method of attack, and for assistance in its development.

independent of r, having a constant value of, say, c. This condition will be satisfied if the coefficients of powers of r in (6) all vanish; i.e. if

$$\frac{\partial}{\partial r}(H^2) = 4H_0A_2 = 2c; \text{ i.e. } A_2 = \frac{c}{2H_0},$$

$$6H_0A_3 + 4A_2^2 = 0; \text{ i.e. } A_3 = -\frac{2A_2^2}{3H_0} = -\frac{c^2}{6H_0^3},$$

$$8H_0A_4 + 12A_2A_3 = 0; \text{ i.e. } A_4 = -\frac{3A_2}{2H_0}.A_3 = +\frac{c^3}{8H_0^5}.$$

Substituting these values of the coefficients A_1, A_2, etc., in (3), we have, as the expression for the potential at the point (r, θ) in a field which satisfies the required condition along the axis,

$$V = V_0 + H_0rP_1(\cos\theta) + \frac{c}{2H_0}r^2P_2(\cos\theta) - \frac{c^2}{6H_0^3}r^3P_3(\cos\theta) + \ldots \quad \ldots\ldots(7),$$

$$\frac{V - V_0}{H_0} = rP_1(\cos\theta) + \frac{c}{2H_0^2}r^2P_2(\cos\theta) - \frac{c^2}{6H_0^4}r^3P_3(\cos\theta) + \ldots \quad \ldots\ldots(7\,a).$$

Substituting the numerical values characterising the experimental apparatus, i.e. $H_0 = 1000$, $c = 3 \times 10^5$, we have

$$\frac{c}{2H_0^2} = 0{\cdot}15; \quad \frac{c^2}{6H_0^4} = 0{\cdot}015.$$

The series will converge least rapidly when r and $\cos\theta$ have their maximum values. The pole gap being, in the apparatus, 3·4 cm., r cannot exceed 1·7, so that when the series of (7 a) is converging least rapidly, its terms will have values

$$1{\cdot}7 + 0{\cdot}43 - 0{\cdot}073 + \ldots.$$

It is thus clear that for an apparatus not too different from the one tested, the value of $(V - V_0)/H_0$ may be expressed by the first two terms of (7 a), without serious error.

We thus have

$$\frac{V - V_0}{H_0} = rP_1(\cos\theta) + \frac{c}{2H_0^2}r^2P_2(\cos\theta) = r\cos\theta + \frac{c}{4H_0^2}r^2(3\cos^2\theta - 1) \quad \ldots\ldots(8).$$

If different values are taken for V in this equation, we have a series of equations to the corresponding equipotentials. It will be convenient to use the same equation, referred to the Cartesian system of co-ordinates; its form in this case is

$$\frac{V - V_0}{H_0} = x + \frac{c}{4H_0^2}(2x^2 - y^2) \quad \ldots\ldots(8\,a).$$

The equation may also be expressed in a third form. Suppose that the equipotential for some particular value of V cuts the axis of symmetry at (X, O). Then we must have

$$\frac{V - V_0}{H_0} = X + \frac{c}{4H_0^2}(2X^2 - 0),$$

so that the general equation may be written

$$X + \frac{c}{2H_0^2} X^2 = x + \frac{c}{2H_0^2} x^2 - \frac{c}{4H_0^2} y^2.$$

If we write $\frac{V - V_0}{H_0} = a$, $\frac{c}{4H_0^2} = b$, then the different forms of the equation to the equipotentials become respectively

$$a = r.\cos\theta + br^2(3\cos^2\theta - 1) \quad \text{.....(8),}$$

$$a = x + b(2x^2 - y^2) \quad \text{.....(8 a),}$$

$$(X + 2bX^2) = (x + 2bx^2) - by^2 \quad \text{.....(8 b),}$$

from which successive equipotentials are determined by giving successive values to either a or X.

The equations just derived define equipotential surfaces of infinite extent. In fashioning a pole face to be an equipotential surface it is, however, necessary to use a limited area. The condition to be satisfied is that if the field is produced between two poles shaped as equipotential surfaces according to the above equations, and separated by the appropriate distance, then their areas must bear such a ratio to each other that the same total flux passes through each.

We shall satisfy this condition by terminating the sections of the equipotential surfaces at points which are on the same line of force. We have the equation to the equipotential (equation 8 b),

$$a = x + b(2x^2 - y^2),$$

$$\therefore\ 0 = dx + b(4x.dx - 2y.dy),$$

$$(4bx + 1)\,dx = 2by.dy.$$

A system of normal curves (lines of force) must satisfy

$$(4bx + 1)\,dy = -2by.dx,$$

$$\frac{dy}{2by} + \frac{dx}{4bx + 1} = 0,$$

$$(2b)^{-1}\log.y + (4b)^{-1}\log(4bx + 1) = \text{const.}$$

$$2\log.y + \log(4bx + 1) = \text{const.}$$

$$y^2(4bx + 1) = \text{const.}$$

Thus, if one equipotential is terminated at the point (x_1, y_1), the other must be terminated at a point (x_2, y_2), related by the expression

$$y_1^2(4bx_1 + 1) = y_2^2(4bx_2 + 1),$$

$$\frac{y_1^2}{y_2^2} = \frac{4bx_1 + 1}{4bx_2 + 1} \quad \text{.....(9).}$$

If the desired interpole gap $(2X)$, and the radius of the larger pole (y_1) are known, then equations (8 b) and (9) suffice to determine a pair of pole pieces for any assumed value of b.

§4. EFFECT OF FINITE SIZE OF SPECIMEN

Having determined a field distribution such that a point specimen placed on the axis experiences a force which is independent of its position on the axis, we have to find to what extent this constancy is affected when a body of finite size is considered.

For a finite body symmetrical about the axis of symmetry of the field, the resultant force will still be directed along this axis, but that portion of it which is due to the effect of the field on the outer parts of the specimen may vary with position along the axis, and thus destroy the constancy of the total force.

We have, from (8 a), the potential at any point (x, y);

$$V = V_0 + xH_0 + \frac{c}{4H_0}(2x^2 - y^2),$$

therefore the field, H_x, along a direction parallel to the axis OX is

$$H_x = H_0 + \frac{c}{H_0} \cdot x,$$
$$\frac{\partial H_x}{\partial x} = \frac{c}{H_0}.$$

Thus the product $H.\partial H/\partial x$, which determines the force on the specimen, should be quite independent of y. This approximation is not close enough for the present purpose, and a more accurate calculation must be made in which a further term of (7) is taken into account. Thus we have

$$\begin{aligned} V &= V_0 + H_0 r P_1(\cos\theta) + \frac{c}{2H_0} r^2 P_2(\cos\theta) - \frac{c^2}{6H_0^3} r^3 P_3(\cos\theta) \\ &= V_0 + H_0 r.\cos\theta + \frac{c}{4H_0} r^2(3\cos^2\theta - 1) - \frac{c^2}{12H_0^3} r^3(5\cos^3\theta - 3\cos\theta) \\ &= V_0 + H_0 x + \frac{c}{4H_0}(2x^2 - y^2) - \frac{c^2}{12H_0^3}(2x^3 - 3xy^2), \end{aligned}$$

$$H_x = H_0 + \frac{c}{H_0} x - \frac{c^2}{2H_0^3} x^2 + \frac{c^2}{4H_0^3} y^2,$$

$$H_x^2 = H_0^2 + \frac{c^2}{H_0^2} x^2 + \frac{c^4}{4H_0^3} x^4 + \frac{c^4}{16H_0^6} y^4 + 2cx - \frac{c^2}{H_0^2} x^2 + \frac{c^2}{2H_0^2} y^2 - \frac{c^3}{H_0^4} x^3 + \frac{c^3}{2H_0^4} xy^2 - \frac{c^4}{4H_0^6} x^2 y^2,$$

$$\frac{\partial}{\partial x}(H_x^2) = \tfrac{1}{2} H \frac{\partial H}{\partial x} = 2\frac{c^2}{H_0^2} x + \frac{c^4}{H_0^3} x^3 + 2c - \frac{2c^2}{H_0^2} x - \frac{3c^2}{H_0^4} x^2 + \frac{c^3}{2H_0^4} y^2 - \frac{c^4}{2H_0^6} xy^2,$$

$$\frac{\partial}{\partial y}\left\{\tfrac{1}{2} H \frac{\partial H}{\partial x}\right\} = \frac{c^3}{H_0^4} y - \frac{c^4}{H_0^6} xy,$$

$$\frac{\partial}{\partial y}\left\{H \frac{\partial H}{\partial x}\right\} = \frac{2c^3}{H_0^4} y\left(1 - \frac{c}{H_0^2} x\right).$$

Substituting numerical values for the present experimental apparatus, we have at the centre of the gap ($x = 0$)

$$\frac{\partial}{\partial y}\left\{H\frac{\partial H}{\partial x}\right\} = \frac{\partial c}{\partial y} = 54000y.$$

As an example, consider a point at 5 mm. from the axis. The mean gradient over the interval is that at $y = 0{\cdot}25$, which is 13500. The change in force is $y.\partial y/\partial x = 6250$. Since the value of c on the axis is $3{\cdot}10^5$, the percentage change in its value at 5 mm. is thus only of the order 5 per cent. Thus, the force exerted on the outer part of a specimen of 1 cm. diameter is 5 per cent. different from that exerted on the centre, so that the force exerted on the whole body will differ by only one or two per cent. from that on a point at its centre, and will thus be constant to within this degree of accuracy as the body is shifted to different parts of the axis.

It must be remembered that in practice the force at a distance from the axis will be seriously affected by "fringing" effects, unless the magnet poles are of large diameter. It is, however, clear from the foregoing analysis that if the specimen is small compared with this diameter it is quite unnecessary to take elaborate precautions to bring it to a definite position in the field, since $H.\partial H/\partial x$ will have a value which is appreciably constant over a very large region of the interpole gap.

§ 5. EXPERIMENTAL REALISATION

As has already been stated, a pair of poles has been designed by a less accurate method; these have been used with a simple torsion balance for susceptibility measurements, as it was found that their shape does not differ seriously from that

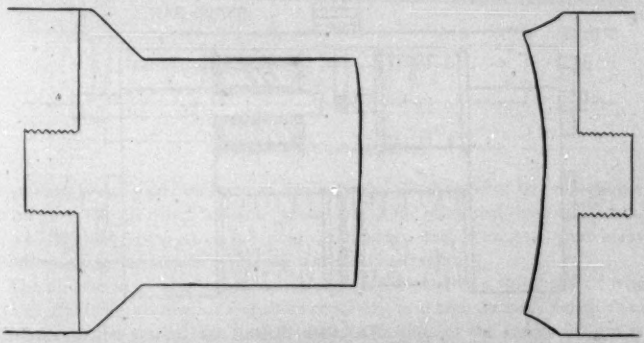


Fig. 2. Section of pole pieces (full size).

indicated by the more accurate theory. The force obtained in the pole gap is not accurately constant, but the constant nature of the susceptibility measurements obtained shows that it does not vary seriously. As a precautionary measure, however, the specimens are set roughly to the same point on the axis when suscepti-

bilities are being compared; for this purpose, a small pointed plug has been let into the centre of one pole face, and the specimens are set to a standard distance from this.

Any attempt to realise the theoretical conditions more accurately has been postponed until experience of the method has been gained from a series of measurements with the present apparatus.

The pole pieces in use are shown in Fig. 2, from which it will be seen that the smaller one has been elongated in order to reduce the disturbing effect of the outer part of the pole core; this is necessary since the poles of the electromagnet are of equal diameters.

§6. APPARATUS

The electromagnet. Details of this are shown in Fig. 3. The coils have about 2500 turns of heavy-gauge copper wire, and can be supplied with current up to 10 amp. from accumulators. As will be seen from the figure, copper tubes, through

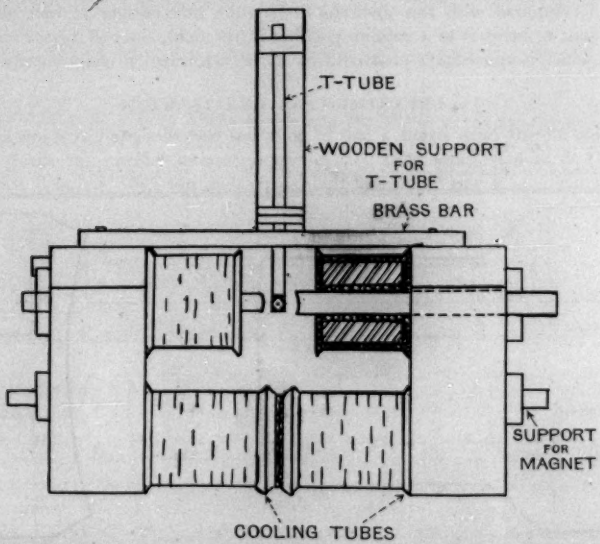


Fig. 3. Electromagnet (partly sectioned) showing method of supporting torsion balance.

which cold water can be circulated, are disposed so as to prevent heating of the poles or the air gap by the windings. A massive brass bar is bolted across the top of the magnet to keep the poles at a fixed distance apart, and this serves as a support for the torsion balance used in measuring the force on the specimen.

The torsion balance. The whole of this system is contained in a T-tube of 1-in. glass tubing, each arm of which is about 12 in. long (Fig. 4); it is held in wooden clamps attached to the brass bar just mentioned. The tube is provided with a brass torsion head H fitting into its upper end, through which passes a brass rod R which is secured by the screw G and can be adjusted vertically to suit suspensions of different lengths. The lower end of this rod is filed down to a diametral plane to which the suspension is attached. To the lower end of the suspension, at O, is fixed the torsion arm A, which is made of glass rod about 0·5 mm. in thickness, except for a short thicker portion at O. The lengths of the two parts AO, OA, of

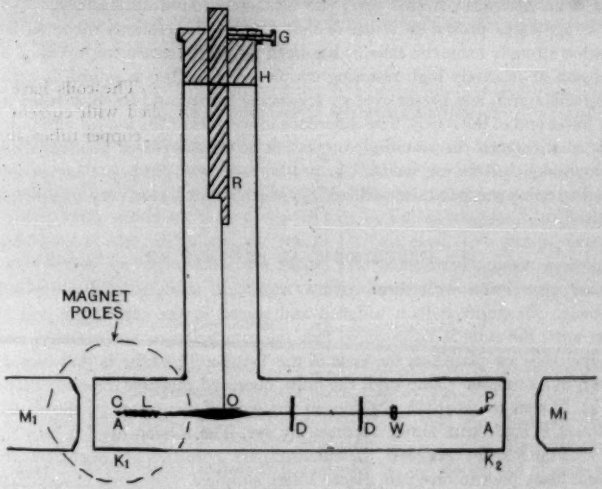


Fig. 4. The torsion balance.

the arm are about 5 cm. and 20 cm. respectively. At the end of the shorter part is a small glass cup C, which takes the specimen; at the other end there is a pointer P of very fine glass, which is used in conjunction with a reading microscope to measure deflections of the system in a manner previously described*.

The difference in weights of the two arms is balanced by a spiral of lead wire L, this material being chosen for its low susceptibility and high density. Small changes in balance of the system due to difference in weights of the specimens are compensated by shifting of the copper wire rider W.

The ends of the horizontal part of the tube are closed by two closely fitting paper caps K_1, K_2, provided with microscope cover slips as windows, through which microscope observations can be made without danger of error.

* *Journ. Sci. Inst.* **6**, 302 (1929).

The substance examined is enclosed in a small glass bulb from 2 to 3 mm. in diameter which fits into the cup *C*. The clamps hold the T-tube at such a distance from the electromagnet that, when the tube is adjusted at the correct height, the cup lies on the axis of symmetry of the field.

Considerable difficulty was experienced in finding a suitable non-magnetic adhesive for the attachment of the suspension and lead counterweight, as any force exerted by the field on the adhesive, especially that used in attaching the suspension to the torsion arm, caused a deflection due to a motion of translation and not to a rotation about the suspension. Shellac varnish, applied thinly and "baked" by means of an electrically heated wire, was ultimately found satisfactory.

The apparatus proved so sensitive that for the observations made so far, on somewhat strongly magnetic salts, it has been found necessary to provide a bifilar suspension of relatively high restoring couple. A silk fibre is joined at each end to the torsion arm, and passes over an accurately turned and freely pivoted pulley at the lower end of the rod *R*. The difference in weights of the loaded and unloaded arm is so slight that the restoring couple can be considered as unaffected by the differing weights of the specimens. The provision of two light paper dampers *D*, *D*, sufficed to damp out pendular oscillations, which at first were very troublesome.

§7. EXPERIMENTAL PROCEDURE

Small glass bulbs with short narrow necks are used to hold the powdered specimens. An empty bulb is weighed and placed in the cup at the end of the torsion arm; the rider is adjusted so that the arm balances horizontally, and the two paper caps are placed on the ends of the T-tube. The tube is then slipped up or down in its wooden clamp until the bulb, observed through the microscope M_1 (Fig. 4), is seen to occupy a position on the axis of symmetry of the field; this adjustment is made with ample accuracy by eye. The torsion head is now turned until, with the magnet energised, the bulb occupies a position at a standard distance from the brass plug in one pole piece. These simple adjustments ensure that the bulb occupies, with sufficient accuracy, a standard position on the axis.

The pointer is now observed through the high power microscope M_2, and from observations of successive turning points of small oscillations, the positions of rest for no field and with the field applied are deduced. The deflections of the pointer never exceed 8 mm., corresponding to a movement of the bulb of 2 mm. These observations give the effect of the field on the empty bulb; they are repeated after the bulb has been filled with a powdered salt, and the effect on the salt alone is deduced by difference. The magnetising current is measured with a high grade moving-coil ammeter, and after adjustment to a standard value of 2·1 amp., the circuit is made and broken several times in order to obtain the true field corresponding to this current.

Generally, the weight of the salt is of the same order as that of the bulb containing it (about 10 mg.) and the deflection in the magnetic apparatus is about 40 times that due to the empty bulb. The weighings are conducted on a Bunge

short-beam balance specially adjusted for high sensitivity in the weighing of masses of the order in question.

With such small quantities of material, it is essential to carry out the work with the utmost care, since a very slight trace of any impurity having a susceptibility differing appreciably from that of the material under examination would suffice to invalidate the reading. This was at first a source of much trouble, but as the technique of the work was mastered the difficulties disappeared. In order, however, to detect any trouble of this nature, susceptibility measurements were made on three or four specimens of each substance, and the mean was only accepted if the individual determinations were in satisfactory agreement. The values of the susceptibility deduced from observations on different specimens of the same substance rarely differed by more than 3 per cent.

§8. RESULTS AND CONCLUSIONS

In all the observations made up to the present time, pure heptahydrated nickel sulphate has been used as a standard of reference for relative measurements. Observations have been grouped, a separate calibration of the apparatus in terms of this salt being made for each group of four or five substances. As illustrating the constancy of the calibration, the results for four successive groups, extending over a period of eleven months, are given. The numbers tabulated represent the susceptibility of the sulphate in arbitrary units characteristic of the apparatus.

October 1927	...	...	750
November 1927	...	...	757
March 1928	...	...	772
June 1928	...	...	761

The extreme values differ by just under 3 per cent., and the constancy over the period of any one group was certainly of a much higher order, since precautions were taken to prevent disturbance of the apparatus over the period of each group, whereas adjustments and minor alterations were sometimes made between calibrations.

As work on the measurements is still in progress, the publication of the full results, comprising measurements on a wide range of nickel salts, is left to a later date; but since in two cases results of another worker are available, they are given here for comparison. Jackson* gives the following susceptibilities:

$NiSO_4.7H_2O$ $= 16{\cdot}0 \times 10^{-6}$ at $292{\cdot}2°$ abs.

$NiSO_4(NH_4)_2SO_4.6H_2O = 10{\cdot}7 \times 10^{-6}$ at $286{\cdot}5°$ abs.

The present measurements give for the susceptibilities of these salts at room temperatures, in arbitrary units, 750 and 517 respectively. Taking Jackson's value for the sulphate as standard, this gives $11{\cdot}0 \times 10^{-6}$ as the susceptibility of the double salt, which is in satisfactory agreement with Jackson's own value.

* *Phil. Trans.* A, **224**, 1 (1923).

The consistency of the results, and the ease with which they can be obtained, have proved to be entirely satisfactory, justifying the further development of the method. Work has accordingly been commenced on the design of an improved apparatus, in which it is hoped to realise the theoretically desirable field distribution more accurately. Attention is also being paid to other experimental details, with the object of developing an apparatus capable of giving precision results with a greater ease than has been possible hitherto.

§ 9. ACKNOWLEDGMENT

The work has been carried out in the Physics Department of East London College, under Prof. C. H. Lees, to whom I am indebted for much valuable advice, and general supervision of the work. I desire also to thank H.M. Department of Scientific and Industrial Research for financial assistance.

DISCUSSION

Dr D. Owen said that the author's method represented a definite advance on previous methods. It was an important achievement to have moved in the direction of making $H.\partial H/\partial x$ constant, for variations in this quantity admittedly gave rise to considerable errors in Curie's work. The speaker expressed a hope that the author's method would be used in a thorough repetition of investigations such as the effect of temperature on magnetisation. For this purpose it would be necessary to measure considerably lower values of the susceptibility than those referred to in the paper, and this appeared to demand that the value of the product $H.\partial H/\partial x$ should be made substantially higher than at present. Would it be feasible to design a magnetic circuit with a rapidly diverging field for this purpose?

Dr J. H. Shaxby: I also should like to congratulate the author on a very beautiful method of using a non-uniform field to give a uniform product. Is it possible to adapt the method to measure the susceptibility of fluids?

Mr J. Guild raised the question whether the treatment of end effects in § 4 of the paper covered all the demagnetising effects of the ends.

Author's reply. In reply to Dr Owen, I would point out that although no measurements have yet been made, with the apparatus described, of susceptibilities smaller than 10^{-15}, this is not due to any lack of sensitivity on account of field design; actually the value of $H.\partial H/\partial x$ is comparatively large, and by a simple modification of the torsion balance it will undoubtedly be possible to attain a very high sensitivity.

In attempting to increase the value of the product $H.\partial H/\partial x$ by making the field diverge more rapidly, one is faced with the difficulty that one of the poles must be made of small diameter; "fringing" effects then become more serious, and the

theoretical conditions cannot be realised so accurately. Also, an increase in $\partial H/\partial x$ by this means, for a constant magnetising current and available gap, reduces H to some extent.

I have not used the apparatus to measure the susceptibilities of fluids, but there is no reason why it should not be used for that purpose.

The treatment of § 4 is intended as an investigation of the field configuration in the absence of the specimen. The correction to be applied for demagnetising effects of the ends of the latter would be of the order of only 0·01 per cent. and is therefore neglected.

THE EQUATIONS OF MOTION OF A VISCOUS FLUID IN TENSOR NOTATION

By C. N. H. LOCK, M.A., National Physical Laboratory.

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ABSTRACT. The tensor notation, which is probably best known in its application in Einstein's four-dimensional theory of gravitation, can be used in its three-dimensional form in obtaining the general equations in many branches of mathematical physics. In the present paper the various steps in the proof of the equations of motion of a viscous fluid are translated into tensor form. An attempt has been made to develop the various formulae in outline from first principles, with references where necessary to a standard work, and also to give whenever possible a geometrical interpretation of the various vector quantities met with in the course of the argument. In particular the fundamental assumptions of Stokes, on which the viscous equations are based, are stated in the form of the relation between the stress vector across an element of area and the vector element of area.

The equations of motion in tensor notation are afterwards translated into ordinary notation for the special case of orthogonal curvilinear coordinates. These can be at once reduced to the commonly useful forms of spherical polars, ellipsoidal coordinates, etc. In a final section it is shown how the tensor equations can be used to deduce the equations of viscous flow in terms of Stokes stream function for motion symmetrical about an axis. The stream function is shown to be identical with the surviving component of the covariant vector potential of which the other two components vanish.

§ 1. INTRODUCTION

In all physical problems of a general nature and in hydrodynamics in particular, a considerable simplification arises if the various vector quantities are represented by a single symbol instead of the three components of the vector in Cartesian coordinates. The result of doing so is to ensure that none but vector expressions occur in the equations; on the other hand, the vector expressions (such as the vector product, divergence, curl, etc.) have to be manipulated by apparently arbitrary rules, many of which must be proved by returning to the Cartesian notation. The tensor notation, on the other hand, has the advantage that it makes use of none but the ordinary rules of algebra and the calculus, and, while the actual components of vectors are dealt with, it is possible by use of suitable notation and by a special convention (the summation convention*) to obtain the same economy in the use of symbols as with the ordinary vector notation.

In the present paper an attempt is made to develop the theory of the motion of a fluid in outline as far as the general equations of viscous flow by the use of the tensor notation in three dimensions. The actual proof of the equations of viscous flow is made to depend on the assumption that the resultant stress across an arbitrary element of area is a function of the first derivative tensor of the velocity vector, is linear in this derivative and vanishes with the element of area. The resulting function must be a vector function, and any properties of the fluid con-

* Appendix A.

tained in it must have the form of scalars, since the fluid can have no directional properties. The customary argument by transformation to the principal axes of the rate-of-strain ellipsoid is replaced by the assumption that the form adopted for the vector function representing the stress is the most general possible subject to the above conditions.

It would have been possible to give the whole of this argument in a form restricted to Cartesian coordinates independent of the particular system, and in fact the form of most of the actual expressions representing physical quantities would have been absolutely unaltered. It seemed however worth while to use the more general form applicable to general curvilinear coordinates, as by this means it was possible to use the results to deduce the equations in general orthogonal coordinates in ordinary notation.

An attempt has been made to develop the tensor calculus from first principles so far as it is required, though such proofs of the various steps as may be found by reference to standard works have for the most part been omitted. Reference is frequently made to *The Absolute Differential Calculus* by Levi-Civita (English translation)*. Definitions of the various physical quantities occurring in the equations of motion are given in some detail; after the proof of the equations of motion, the various tensor expressions, including the expressions for the stresses, are converted to general orthogonal coordinates in ordinary notation.

The method of eliminating the pressures, given in Lamb's *Hydrodynamics*†, is developed in tensor notation and expressed in terms of the vector potential. The result is used to develop the equation for the special case of motion symmetrical about an axis in orthogonal coordinates, in terms of Stokes' stream function ψ, by showing that ψ is identical with the third component of the covariant vector potential, the other two components being zero.

§ 2. THEORY OF TENSORS

The essential feature of the tensor notation is that it applies equally to any possible system of coordinates. Starting with any Cartesian system $X_i, (X_1, X_2, X_3)$, we take as the typical system of coordinates any three arbitrary functions of the X_i,

$$x_j = f_j(X_1, X_2, X_3).$$

These generalised coordinates will be in general both curvilinear and oblique; but the tensor formulae might be restricted if required to the special case of the transformation of Cartesian axes by displacement or rotation, which would lead to important simplifications in detail but would leave the general results unaltered.

In the tensor calculus we are concerned with transformations from any coordinate system x_i to any other system $\bar{x}_a$, where either system is considered as defining the position of an arbitrary point P; the $\bar{x}_a$ are considered either as functions of the x_i or of the original Cartesian system.

The simplest case is that of a function of position ϕ which has a definite value at every point P. It may be considered as a function of the coordinates x_i and is obviously transformed by the relation of "invariance"

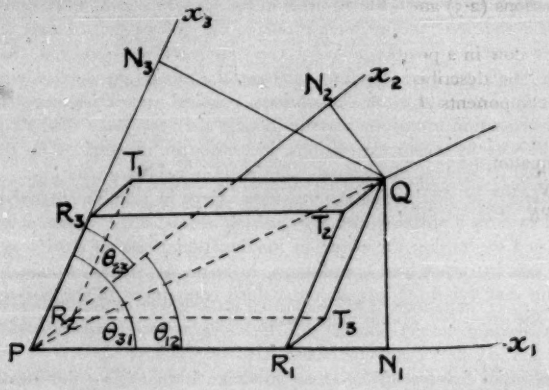
$$\bar{\phi}(\bar{x}_1, \bar{x}_2, \bar{x}_3) = \phi(x_1, x_2, x_3).$$

* Blackie and Son, 1927.

† 4th edition, § 328, equation (8).

Before proceeding to discuss the possible ways of expressing a vector function of position it is desirable to remark that the tensor calculus is only concerned with "infinitesimal" vectors. Every vector used in physical problems can in fact be represented by a directed line of infinitesimal length, with the exception of the straight line representing the position of the point P; the position of P is not considered as a vector in the tensor calculus.

We start therefore by considering the infinitesimal vector **A**, which is a function of the position of the point P, as being represented by the line joining P (coordinates x_i) to a neighbouring point Q (coordinates $x_i + dx^i$)*. Consider also the points R_1, R_2, R_3 whose coordinates are $(x_1 + dx_1, x_2, x_3)$ etc. and T_1, T_2, T_3 whose coordinates are $(x_1, x_2 + dx_2, x_3 + dx_3)$ etc. These eight points form to the first order a parallelepiped which is in general oblique angled.



One method of representing the vector **A** is by means of the three components dx^i (dx_1, dx_2, dx_3) which are proportional to the lengths PR_1, PR_2, PR_3. An equally reasonable method would be to make use of quantities proportional to the projections PN_1, PN_2, PN_3 of PQ on the axial lines PR_1, PR_2, PR_3.

Write ds for the length of PQ and consider the formula connecting ds with the dx^i. ds may be considered as an invariant function of **A** and of the position of P. In Cartesian coordinates

$$ds^2 = dX_1^2 + dX_2^2 + dX_3^2 \qquad \text{.....(2·1)},$$

which may be written $dX_\alpha dX^\alpha$*,

with $dX_\alpha = dX^\alpha$.

By direct transformation we have

$$ds^2 = \frac{\partial X_\alpha}{\partial x_i} \cdot \frac{\partial X^\alpha}{\partial x_j} dx^i dx^j*,$$

* See Appendix A.

or writing $$g_{ij} \text{ for } \frac{\partial X_a}{\partial x_i}.\frac{\partial X^a}{\partial x_j} \qquad \text{.....(2·2),}$$

$$ds^2 = g_{ij}\,dx^i\,dx^j \qquad \text{.....(2·3),}$$

with the relation $g_{ij} = g_{ji}$. By definition g_{ij} are functions of position which depend only on the particular system of coordinates, not on the particular vector **A**.

From the geometry of the parallelepiped, writing θ_{23} for the angle R_2PR_3, etc.,

$$ds^2 = \Sigma PR_1^2 + 2\Sigma PR_2 PR_3 \cos\theta_{23} \qquad \text{.....(2·4).}$$

Comparing equations (2·3) and (2·4), we obtain the equations

$$PR_1 = g_{11}^{\frac{1}{2}}\,dx_1 \qquad \text{.....(2·5),}$$

etc., and $$\cos\theta_{23} = \frac{g_{23}}{g_{22}^{\frac{1}{2}}\,g_{33}^{\frac{1}{2}}} \qquad \text{.....(2·6),}$$

etc. Equations (2·5) and (2·6) connect the g_{ij} with the geometry of the parallelepiped.

We are now in a position to define two types of components of the vector **A**. These will be described as the "contravariant" components A^a and the "covariant" components A_a. The A^a are defined at once by the three equations

$$A^a = dx^a \qquad \text{.....(2·7),}$$

so that equation (2·5) gives $$A^1 = PR_1/g_{11}^{\frac{1}{2}} \qquad \text{.....(2·8).}$$

Again PN_1, the projection of PQ on PR_1, is equal to the sum of the projections of PR_1, PR_2, PR_3 on PR_1 so that

$$\begin{aligned} PN_1 &= PR_1 + PR_2.\cos\theta_{12} + PR_3.\cos\theta_{13} \\ &= g_{11}^{\frac{1}{2}}\,dx_1 + \frac{g_{12}}{g_{11}^{\frac{1}{2}}\,g_{22}^{\frac{1}{2}}}\cdot g_{22}^{\frac{1}{2}}\,dx_2 + \frac{g_{13}}{g_{11}^{\frac{1}{2}}\,g_{33}^{\frac{1}{2}}}\cdot g_{33}^{\frac{1}{2}}\,dx_3 \\ &= \frac{1}{g_{11}^{\frac{1}{2}}}\,g_{1a}\,dx_a\text{*} \end{aligned} \qquad \text{.....(2·9).}$$

We shall define the covariant component A_a of **A** by the equation

$$A_a = g_{ab}\,dx^b \qquad \text{.....(2·10).}$$

It follows from equation (2·9) that

$$A_1 = g_{11}^{\frac{1}{2}}\,PN_1 \qquad \text{.....(2·11),}$$

etc., which give the geometrical definition of A_a in a form similar to equation (2·8) for A^a. Alternative definitions of A^a and A_a may be given with reference to the parallelepiped consisting of planes perpendicular to PR_1, PR_2, PR_3. It appears that A^a and A_a are equally well qualified to be considered as components of the vector **A**.

When the coordinates are orthogonal the point N_1 coincides with R_1, and PN_1 is the "component" of **A** as usually defined. Also, when $i \neq j$, g_{ij} is zero by equation (2·6). It follows from equations (2·8) and (2·11) that

$$g_{11}^{\frac{1}{2}}\,A^1 = PN_1 = \frac{1}{g_{11}^{\frac{1}{2}}}\,A_1 \qquad \text{.....(2·12).}$$

In Cartesian coordinates $g_{11} = 1$ and the distinction between A^a and A_a disappears.

* See Appendix A.

§ 3. SOME FUNDAMENTAL ALGEBRAICAL RESULTS

At this stage it is convenient to define the most general type of tensor and to obtain its laws of transformation, showing that A^i, A_i as defined above are particular cases. We take dx^i as the typical contravariant tensor, which evidently satisfies the law of transformation

$$d\bar{x}^i = \frac{\partial \bar{x}^i}{\partial x^a} dx^a \qquad \ldots\ldots(3{\cdot}1).$$

An alternative definition of a covariant vector U_i is given by the equation

$$\phi = U_i dx^i \qquad \ldots\ldots(3{\cdot}2).$$

It may be shown* that the values of U_i in any other system of coordinates are consistently and uniquely defined by the condition that ϕ is invariant for any arbitrary values of dx^i, and that the U_i so defined satisfy the law of transformation

$$\bar{U}_i = \frac{\partial x_a}{\partial \bar{x}_i} U_a \dagger \qquad \ldots\ldots(3{\cdot}3).$$

Both the laws of transformation (3·1) and (3·3) may be shown to be transitive and also invertible, so that e.g.

$$U_a = \frac{\partial \bar{x}_i}{\partial x_a} \bar{U}_i \qquad \ldots\ldots(3{\cdot}4).$$

The covariant vector U_i has still to be identified with the covariant component A_i, defined in equation (2·10).

In addition to the contravariant and covariant vectors having three components it is possible to define a more general type of tensor having 9, 27, etc. components by an extension of equation (3·2). For example write

$$\phi = A_{ab}{}^{ijk} U_i U_j' U_k'' dx^a dx'^b \qquad \ldots\ldots(3{\cdot}5),$$

so that $A_{ab}{}^{ijk}$ has 3^5 components. It may be shown that components $\bar{A}_{ab}{}^{ijk}$ in any other system of coordinates are consistently and uniquely defined by the condition that ϕ is invariant for arbitrary values of the U_i and dx^a, and that the $A_{ab}{}^{ijk}$ satisfy the law of transformation

$$\bar{A}_{ab}{}^{ijk} = \frac{\partial \bar{x}_i}{\partial x_l} \frac{\partial \bar{x}_j}{\partial x_m} \frac{\partial \bar{x}_k}{\partial x_n} \frac{\partial x_f}{\partial \bar{x}_a} \frac{\partial x_g}{\partial \bar{x}_b} A_{fg}{}^{lmn} \qquad \ldots\ldots(3{\cdot}6).$$

The $A_{ab}{}^{ijk}$ are described as the components of a tensor of the fifth rank having three elements of contravariant and two of covariant character.

As a particular example, a comparison of equation (2·3) with equation (3·5) above, shows that the g_{ij} are components of a covariant tensor of the second rank; it is of fundamental importance, being a function of the coordinate system only.

It may be shown, as a particular case of (3·5), that the "outer product" of any number of vectors or tensors is itself a tensor, e.g.

$$A^i . B_k{}^j = C_k{}^{ij} \qquad \ldots\ldots(3{\cdot}7),$$

is a tensor of the kind indicated.

* Levi-Civita, *The Absolute Differential Calculus*, chap. 4, § 11.

† A simple method of remembering this law of transformation is afforded by equation (6·1) below.

Contraction. It may be shown* that the result of putting a lower suffix equal to an upper suffix in any tensor and performing the indicated summation†, is itself a tensor. Thus $A_{ka}{}^{ia}$ is a tensor obtained from the tensor $A_{kl}{}^{ij}$ by the process of "contraction." In particular the result of contracting the "mixed" tensor $A_j{}^i$ is the scalar quantity $A_a{}^a$.

A further extension of equation (3·5) is that if (e.g.) $B_k{}^{ij}$ is an *arbitrary* tensor then the condition that any product (contracted or not) of $B_k{}^{ij}$ into $A_{mn}{}^l$ is a tensor of the kind indicated, defines $A_{mn}{}^l$ uniquely and consistently in any coordinate system such that it is a tensor of the kind indicated.

A very specialised type of mixed tensor is defined as follows:

$$\begin{aligned} \delta_j^i &= 0 \text{ when } i \neq j \\ &= 1 \text{ when } i = j \end{aligned} \qquad \text{.....(3·8).}$$

The fact that $\delta_j{}^i$ satisfies the correct law of transformation may be verified by direct substitution. By virtue of the above rule of contraction it may be verified that the contracted product of $\delta_j{}^i$ into e.g. a contravariant vector A^k is given by

$$\delta_j^i A^j = A^i \qquad \text{.....(3·9),}$$

so that $\delta_j{}^i$ has the property of substituting one suffix letter for another in any tensor. Other important properties which may be easily verified are embodied in the equation

$$\delta_j^i = g^{ia} g_{aj} \ddagger = \frac{\partial \bar{x}^i}{\partial x^a} \frac{\partial x^a}{\partial \bar{x}^j} \qquad \text{.....(3·10).}$$

Application to geometrical vectors. We may now return to the subject of the representation of geometrical vectors by means of the tensor notation. It follows from a remark in the section on contraction, that the covariant component as defined in equation (2·10) is equivalent to the covariant vector as defined in equation (3·2) and satisfies the same law of transformation, equation (3·3). The three simultaneous linear equations

$$A_i = g_{ia} A^a \qquad \text{.....(3·11),}$$

may be solved for the A^a in the form

$$A^a = g^{aj} A_j \qquad \text{.....(3·12),}$$

where, according to the ordinary rule for solving simultaneous equations, g^{ij} is the cofactor of g_{ij} in the determinant having g_{ij} as elements divided by the value g of the determinant, and it follows from (3·12) that g^{ij} is a tensor. Thus the fundamental tensors g_{ij} and g^{ij} can be used to convert the contravariant components of a vector into the covariant components and *vice versa*.

We are now in a position to show how either the A^i or A_i are convenient for representing the vector **A** in any system of coordinates. The first observation is that the condition $A_i = 0$ involves the vanishing of **A**. Again, equations of the type

$$A_i = B_i,$$

hold in any system of coordinates, and involve the identity of the vectors **A** and **B**.

* Levi-Civita, *loc. cit.* chap. 4, § 9. † Appendix A. ‡ g^{ia} is defined in the next section.

In view of the linearity of the equations of transformation, it follows that the equation

$$K_i = A_i + B_i + C_i \qquad \text{.....(3·13),}$$

holds in any system of coordinates, and it may be shown geometrically that the vector **K** is equal to the sum of the vectors **A**, **B**, **C** according to the parallelogram law.

§ 4. GEOMETRICAL EXAMPLES OF TENSORS OF THE SECOND ORDER

The "outer" product of two vectors. The results of § 3 show that the "outer" product of the components of two vectors **A** and **B**, e.g. A^iB^j is a tensor of the second rank having nine components. Since there are two types of component of each vector, we can construct four tensors of the second rank, A^iB^j, A^iB_j, A_iB^j, A_iB_j (one contravariant, two mixed, and one covariant), which may be considered as being "components" of a tensor (generalised vector) of the second rank.

Linear vector function of a vector. We consider a vector **F** which is a function of the arbitrary vector **A**. **F** $\{A\}$ is described as a linear function if it satisfies the following conditions: (i) if **A** is fixed in direction, then **F** is fixed in direction, and of magnitude proportional to the magnitude of **A**; (ii) if **A** (1), **A** (2) are any two values of **A**, and **A** (3) is their vector sum, then **F** $\{A\,(3)\}$ is the vector sum of **F** $\{A\,(1)\}$ and **F** $\{A\,(2)\}$. Under these conditions it may be shown that the components of **F** are equal to the reduced product of the components of **A** into a tensor T^* of the second rank, e.g.

$$F^i = T^{ij}A_j \qquad \text{.....(4·1).}$$

Proof. Let **A** (1), **A** (2), **A** (3) be three values of **A** having different directions. Substitute these and the corresponding values of **F** in equation (4·1) and we obtain nine simultaneous equations which in virtue of the results of § 3 determine the components of T as a tensor in any system of coordinates. Now any value **A** (4) of **A** may be expressed in the form

$$\mathbf{A}\,(4) = \alpha_1\,\mathbf{A}\,(1) + \alpha_2\,\mathbf{A}\,(2) + \alpha_3\,\mathbf{A}\,(3) \qquad \text{.....(4·2),}$$

where α_1, α_2, α_3 are scalar constants and it follows from the conditions satisfied by **F** that

$$\mathbf{F}\,(4) = \alpha_1\,\mathbf{F}\,(1) + \alpha_2\,\mathbf{F}\,(2) + \alpha_3\,\mathbf{F}\,(3) \qquad \text{.....(4·3),}$$

and therefore **F** (4) and **A** (4) satisfy equation (4·1). Thus equation (4·1) represents the necessary and sufficient condition that **F** is a linear vector function of **A**. The tensor T is of the most general type, unlike the example of the last section (product of two vectors).

§ 5. CONTRACTION OF TENSORS OF THE SECOND ORDER

From any tensor T^* of the second rank we can obtain by processes of contraction a scalar and a vector which may be described as the "scalar contraction" and the "vector contraction" of T respectively.

Scalar contraction. The contracted mixed component T^a_a* of the tensor T is a scalar: the scalar contraction of T.

* By analogy with the outer product we consider any tensor T, of the second rank, to have the four components: T^{ij}, $T^i{}_j$, $T_i{}^j$, T_{ij}. Any three of these may be expressed in terms of the fourth by relations identical with those obtainable by application of equations (3·11) or (3·12) to the outer product. It follows that $T^a{}_a = T_a{}^a$, and this contraction will therefore be written T^a_a.

Vector contraction. Epsilon tensors. It appears on examination that the antisymmetrical tensor $T^{ab} - T^{ba}$ has only three distinct components. It may be reduced to a vector by means of the fundamental auxiliary tensor of the third rank E_{abc}* (in space of three dimensions) defined by the conditions

$$E_{abc} = 0,$$

unless a, b and c are all different, when

$$E_{abc} = \pm g^{\frac{1}{2}} \qquad \text{.....(5·1),}$$

the sign being that of the corresponding term of the fundamental three-row determinant whose value is g. The corresponding contravariant tensor is defined by

$$E^{ijk} = g^{ia} g^{jb} g^{kc} E_{abc} \qquad \text{.....(5·2),}$$

and may be shown to satisfy similar conditions to those of equation (1) but with $g^{\frac{1}{2}}$ replaced by $g^{-\frac{1}{2}}$. An important result which may be easily verified is that

$$E^{ijk} E_{iab} = \delta^j_a \delta^k_b - \delta^j_b \delta^k_a \qquad \text{.....(5·3).}$$

Now consider the vector $\mathbf{L}$ defined by the alternative equations

$$L_i = \tfrac{1}{2} E_{iab} (T^{ab} - T^{ba})$$
$$= E_{iab} T^{ab} \qquad \text{.....(5·4),}$$
$$L^i = \tfrac{1}{2} E^{iab} (T_{ab} - T_{ba}) \qquad \text{.....(5·5),}$$

which may be shown to be all equivalent. Multiplying both sides of (5·4) by E^{ijk} and using (5·3) we have

$$E^{ijk} L_i = (\delta^j_a \delta^k_b - \delta^j_b \delta^k_a) T^{ab}$$
$$= T^{jk} - T^{kj} \qquad \text{.....(5·6),}$$

by equation (3·9). By means of the equations (5·5) and (5·6), the vector $\mathbf{L}$ (the "vector contraction" of T) may be expressed in terms of the tensor $(T^{jk} - T^{kj})$ and *vice versa*.

Some additional identities involving epsilon tensors which will be required below are given in Appendix B.

Scalar and vector products. Examples of these two types of contracted tensor are given by substituting for the tensor T^{ab} the "outer product" $A^a B^b$ of the two vectors $\mathbf{A}$ and $\mathbf{B}$. These are the scalar product ϕ and the vector product $\mathbf{V}$ of the two vectors defined by the equations

$$\phi = A^a B_a \qquad \text{.....(5·7),}$$

and

$$V_i = E_{ijk} A^j B^k \qquad \text{.....(5·8),}$$

similar to equation (5·4). It may be shown that these are identical with the scalar and vector products of two vectors as ordinarily defined.

If we take the scalar product of $\mathbf{V}$ into a third vector $\mathbf{C}$ it is obvious geometrically that the result is equal to the volume of the parallelepiped having $\mathbf{A}$, $\mathbf{B}$, $\mathbf{C}$ as adjacent edges. Alternative expressions for this volume are therefore

$$C_k V^k \equiv E^{ijk} A_i B_j C_k \qquad \text{.....(5·9),}$$

$$\equiv \frac{1}{g^{\frac{1}{2}}} \begin{vmatrix} A_1 B_1 C_1 \\ A_2 B_2 C_2 \\ A_3 B_3 C_3 \end{vmatrix}.$$

* Levi-Civita, *loc. cit.* chap. 6, § 8. He uses ϵ in place of E.

§6. DIFFERENTIATION OF SCALAR AND VECTOR FUNCTIONS OF POSITION

Derivative of a scalar. The change $\delta\phi$ in a scalar function ϕ due to a small displacement from the point P to the point Q considered as a vector $PQ = \mathrm{B}$ is obviously given by

$$\delta\phi = \phi_Q - \phi_P = \frac{\partial\phi}{\partial x_a}\,dx^a = \frac{\partial\phi}{\partial x^a}\,B^a \qquad \text{.....(6·1).}$$

It follows from equation (3·2), that since $\delta\phi$ is invariant, $\partial\phi/\partial x_a$ is the covariant component of a vector. This vector is the gradient of the scalar ϕ as usually defined.

Derivative of a vector. Example of a linear vector function. For an arbitrary vector function of position $\mathbf{A}$ we have to consider the effect on $\mathbf{A}$ of a small displacement from the point P to the point Q, where PQ is considered as an arbitrary vector $\mathbf{B}$ whose contravariant components are dx^i. Let $\delta\mathbf{A}$ be a vector equal to the difference between $\mathbf{A}_Q$ and $\mathbf{A}_P$ or more exactly to the difference between $\mathbf{A}_Q$ and the vector through Q equal and parallel to $\mathbf{A}_P$. Obviously $\delta\mathbf{A}$ is a function of the vector $\mathbf{B}$ and it may be shown geometrically that it is a "linear vector function." Hence it is possible to define the four components $A^i{}_{,}{}^a$; $A^i{}_{,a}$; $A_i{}_{,}{}^a$; $A_{i,a}$ of the second rank "tensor derivative"* of $\mathbf{A}$ by equations such as

$$\delta A^i = A^i{}_{,a}\,B^a = A^i{}_{,a}\,dx^a \qquad \text{.....(6·2).}$$

In general when the coordinates are curvilinear $A^i{}_{,a}$ is not equal to $\partial A^i/\partial x^a$ nor is $\partial A^i/\partial x^a$ a tensor. The following expressions for $A^i{}_{,a}$ and $A_{i,a}$ are obtained in Appendix C, by direct transformation from Cartesian coordinates:

$$A^i{}_{,}{}^a = \frac{\partial A^i}{\partial x_a} + \Gamma_{ab}{}^i A^b \qquad \text{.....(6·3),}$$

$$A_{i,a} = \frac{\partial A_i}{\partial x_a} - \Gamma_{ia}{}^b A_b \qquad \text{.....(6·4),}$$

where

$$\Gamma_{jk}{}^i = \tfrac{1}{2}g^{ia}\left\{\frac{\partial g_{ja}}{\partial x_k} + \frac{\partial g_{ka}}{\partial x_j} - \frac{\partial g_{jk}}{\partial x_a}\right\} \qquad \text{.....(6·5),}$$

is known as a Christoffel symbol of the second kind.

Derivative of a tensor. Similar expressions may be obtained for the derivative of a tensor of any rank, e.g. the tensor A_{ab} has the covariant derivative

$$A_{ab,c} = \frac{\partial A_{ab}}{\partial X_c} - \Gamma_{bc}{}^i A_{ai} - \Gamma_{ac}{}^i A_{ib} \qquad \text{.....(6·6).}$$

It may be shown that the covariant derivatives of the fundamental tensors g_{ij}, g^{ij}, E_{abc} are all zero so that these tensors behave as constants for the purpose of covariant differentiation. It follows that $A_{i,a} = g_{ib}A^b{}_{,a}$.

Divergence of a vector. The "scalar contraction" of the tensor derivative of A is $A^a{}_{,a}$ which is identical with the divergence as ordinarily defined. It may be

* The "comma" notation for the tensor derivative is used throughout the paper. Of the four components of the tensor derivative of a vector (see p. 270, footnote), $A^i{}_{,}{}^a$ and $A_{i,a}$ are defined by equations (6·3) and (6·4), and satisfy the relation $A_{i,a} = g_{ib}A^b{}_{,a}$ while $A^i{}_{,}{}^a$ and $A_i{}_{,}{}^a$ are defined by the relations $A^i{}_{,}{}^a = g^{ab}A^i{}_{,b}$ and $A_i{}_{,}{}^a = g^{ab}A_{i,b}$.

verified that $A^a{}_{,a}$ may be expressed in the following form which does not contain Christoffel symbols:

$$\text{div. } \mathbf{A} \equiv A^a{}_{,a} = g^{-\frac{1}{2}} \frac{\partial}{\partial x_a}(g^{\frac{1}{2}} A^a)^* \qquad \text{.....(6·7).}$$

Curl of a vector. The "vector contraction" of the tensor derivative of **A** (with sign reversed) is identical with the curl of **A** as ordinarily defined and may be written "curl **A**." From equation (5·5) we have

$$\begin{aligned}(\text{curl } A)^i &= E^{iab} A_{b,a} \\ &= \tfrac{1}{2} E^{iab} (A_{b,a} - A_{a,b}) \qquad \text{.....(6·8).}\end{aligned}$$

When the second form is written out in full the Christoffel symbols cancel and we have therefore

$$(\text{curl } A)^i = E^{iab} \frac{\partial A_b}{\partial x_a} \qquad \text{.....(6·9),}$$

a form independent of Christoffel symbols. Since the operation of obtaining the curl changes a vector into a vector it may be repeated any number of times. For this purpose it is convenient to obtain formulae connecting corresponding components of **A** and curl **A**; these are:

$$(\text{curl } A)_i = g_{if} E^{abc} \frac{\partial A_c}{\partial x_b} \qquad \text{.....(6·10),}$$

$$(\text{curl } A)^i = E^{iab} \frac{\partial}{\partial x_a}(g_{bc} A^c) \qquad \text{.....(6·11).}$$

Second derivative. By substituting the first derivative of a vector (equation 6·4) in the formula for the first derivative of the corresponding tensor (equation 6·6) we obtain the second derivative of the vector **A** of which e.g. the covariant component may be written $A_{i,a,b}$. It is important to notice that in Euclidean space with which alone we are concerned, the order of covariant differentiation is indifferent; this fact involves a series of complicated identities among the g_{rs} and their derivatives.

Contracted second derivative ∇^2. The contracted second derivative (contravariant component, $A^{i,a}{}_{,a}$) is the component of a vector which is identical with $\nabla^2\mathbf{A}$. This vector may be reduced to a form independent of Christoffel symbols by using the identity usually written

$$\nabla^2 \mathbf{A} \equiv \text{grad. div. } \mathbf{A} - \text{curl curl } \mathbf{A} \qquad \text{.....(6·12),}$$

since the divergence, curl, and gradient have all been simplified in this way. A proof of this identity by tensor notation is given in Appendix D and an alternative method of simplifying $\nabla^2\mathbf{A}$ in Appendix E.

Reduced second derivative of a scalar $\left(\frac{\partial \phi}{\partial x^a}\right)^{,a}$†. This is identical with the divergence of the gradient of ϕ and is also usually written $\nabla^2\phi$, but it is important to notice that the formulae for $\nabla^2\phi$ and $\nabla^2\mathbf{A}$ only agree in the special case of Cartesian coordinates.

* Levi-Civita, *loc. cit.* chap. 6, § 7.

† See footnote, p. 272.

§ 7. VECTORS AND TENSORS REQUIRED IN HYDRODYNAMICS

Velocity. If a particle of fluid at a point P is displaced to the point Q in a small time interval δt, then PQ is an infinitesimal vector and the velocity $\mathbf{u}$ may be defined as the vector

$$\mathbf{u} = \frac{1}{\delta t} PQ \qquad \text{.....(7·1),}$$

the time δt being considered as an independent variable unaffected by change of coordinates.

Acceleration. We shall adopt the Eulerian convention by which the velocity $\mathbf{u}$ at a fixed point P in space is considered as a function of the time and of the coordinates of P, and shall deal only with the case of fixed axes. In determining the acceleration of a given particle of fluid write $\mathbf{u}_P(t)$ and $\mathbf{u}_P(t+\delta t)$ for the velocities at the point P at the beginning and end of the small interval δt. At the end of the interval the particle of fluid will have moved to the point Q where $PQ = \mathbf{u}_P \delta t$ to the first order and its velocity will then be $\mathbf{u}_Q(t+\delta t)$. Let $\mathbf{f}$ be the vector acceleration of the particle, then

$$\begin{aligned} \mathbf{f}\delta t &= \mathbf{u}_Q(t+\delta t) - \mathbf{u}_P(t) \\ &= \mathbf{u}_Q(t) - \mathbf{u}_P(t) + \mathbf{u}_P(t+\delta t) - \mathbf{u}_P(t) \end{aligned} \qquad \text{.....(7·2),}$$

to the first order; in tensor notation (contravariant components) this becomes:

$$f^i \delta t = \delta u^i + \frac{\partial u^i}{\partial t}\delta t \qquad \text{.....(7·3).}$$

By the results of section 6, δu^i the contravariant component of the difference between the simultaneous velocities at Q and P is given by

$$\delta u^i = u^i{}_{,a}\,\delta x^a \qquad \text{.....(7·4),}$$

where δx^a the contravariant component of the vector PQ is given by

$$\delta x^a = u^a \delta t \qquad \text{.....(7·5).}$$

Hence we have

$$f^i = u^i{}_{,a}u^a + \frac{\partial u^i}{\partial t} \qquad \text{.....(7·6),}$$

and similarly

$$f_i = u_{i,a}u^a + \frac{\partial u_i}{\partial t} \qquad \text{.....(7·7).}$$

A similar formula applied to any vector function of position corresponds to the usual formula for differentiation following the motion of the fluid*.

When $u_{i,a}u^a$ is written out in full it is found that two of the three terms in the Christoffel symbols cancel, leading to the result:

$$u_{i,a}u^a = u^a \frac{\partial u_i}{\partial x_a} - \tfrac{1}{2}u^a u^b \frac{\partial g_{ab}}{\partial x_i} \qquad \text{.....(7·8).}$$

Rate of strain. It is usual in works on hydrodynamics to separate the derivative of the velocity $u_{i,j}$ into two components A_{ij} and B_{ij} given by

$$A_{ij} = \tfrac{1}{2}(u_{i,j} + u_{j,i}) \qquad \text{.....(7·9),}$$

$$B_{ij} = \tfrac{1}{2}(u_{i,j} - u_{j,i}) \qquad \text{.....(7·10),}$$

so that

$$u_{ij} = A_{ij} + B_{ij} \qquad \text{.....(7·11).}$$

* Lamb, *Hydrodynamics*, § 5.

It appears from equation (6·8) that the tensor B_{ij} can be expressed in terms of a vector ξ (the curl of the velocity) given by the equation

$$\xi^i = (\text{curl } u)^i = E^{iqb} u_{b,q} \qquad \text{.....(7·12).}$$

This vector is the "vorticity" as ordinarily defined; the tensor A_{ij} is described as the "rate of strain." The proof given below shows that the stress components are functions of the rate of strain and independent of the rotation so that it becomes unnecessary to make the separation at this stage. The tensor B or the vector ξ can be expressed in a form independent of Christoffel symbols by means of equations (6·10) or (6·11).

For the tensor A it will be found that when A^{ij} is written out in full some of the derivatives of the g_{is} in the Christoffel symbols cancel, giving the result

$$2A^{ij} = u^{i,j} + u^{j,i}$$
$$= g^{ja}\frac{\partial u^i}{\partial x_a} + g^{ia}\frac{\partial u^j}{\partial x_a} + u^a g^{ib} g^{jc}\frac{\partial g_{bc}}{\partial x_a} \qquad \text{.....(7·13).}$$

§ 8. STRESS

Consider an arbitrary small element of area $d\mathbf{S}$ considered as a vector defined as being perpendicular to the plane of the element of area, and equal to it in magnitude, and let $d\mathbf{F}$ be the resultant force of reaction across this area in the fluid. $d\mathbf{F}$ is a function of the vector $d\mathbf{S}$ and it will be shown that it is a linear vector function as defined in § 4. Consider the equilibrium of the element of fluid bounded by any small tetrahedron. The forces $d\mathbf{F}$ on the four sides have zero resultant to the first order since the inertia and external forces are of a higher order of small quantities; the vector elements of area of the four sides have zero resultant by the geometry of the tetrahedron. Again, when $d\mathbf{S}$ is fixed in direction $d\mathbf{F}$ is fixed in direction and proportional to $d\mathbf{S}$. Hence $d\mathbf{F}$ is a linear vector function of $d\mathbf{S}$, and by § 4 a tensor P of the second rank may be defined by an equation such as

$$dF^i = P^{ij} dS_j \qquad \text{.....(8·1).}$$

The stress tensor P then determines $d\mathbf{F}$ for all values of $d\mathbf{S}$.

The scalar contraction of P is P^a_a which may be written

$$P^a_a = \frac{dF_1}{dS_1} + \frac{dF_2}{dS_2} + \frac{dF_3}{dS_3} = \frac{dF^1}{dS^1} + \frac{dF^2}{dS^2} + \frac{dF^3}{dS^3} \qquad \text{.....(8·2).}$$

It is usual to write $-\frac{1}{3}P^a_a = p$ the "mean pressure" in the fluid.

Equation of moments. Symmetry of P^{ij}. An additional relation may be obtained from the equation of moments on the fluid contained in an elementary parallelepiped whose adjacent edges are the vectors $d\xi . d\eta . d\zeta$. If $d\mathbf{F}(\xi)$ is the stress across the face bounded by $d\eta$, $d\xi$, the sum of the moments of the forces on the opposite faces about the centre of the parallelepiped is the vector $\mathbf{M}(\xi)$ equal to the vector product of $d\mathbf{F}(\xi)$ by $d\xi$, so that

$$M_i(\xi) = E_{ijq} dF^j(\xi)\, d\xi^q \qquad \text{.....(8·3).}$$

Again the area $d\mathbf{S}(\xi)$ of the face of the parallelepiped bounded by $d\eta$ and $d\zeta$ is given by

$$dS_i(\xi) = E_{ibc} d\eta^b d\zeta^c \qquad \text{.....(8·4).}$$

Adding the moments of the forces on the other pairs of opposite faces, we have for the whole moment

$$M_i = M_i(\xi) + M_i(\eta) + M_i(\zeta)$$
$$= E_{ijk} P^{jk} dS_k d\xi^a + \dots \qquad \dots\dots(8{\cdot}5),$$
$$= \{E_{ija}E_{kbc} + E_{ijb}E_{kca} + E_{ijc}E_{kab}\} P^{jk} d\xi^a d\eta^b d\zeta^c$$
$$= \tfrac{1}{2} E_{ijk}(P^{jk} - P^{kj}) E_{abc} d\xi^a d\eta^b d\zeta^c \qquad \dots\dots(8{\cdot}6),$$

using the result of Appendix B, equation (B·2). Since all other forces are of a higher order of small quantities, it follows that **M** vanishes and hence from equation (8·6)

$$P^{jk} = P^{kj} \qquad \dots\dots(8{\cdot}7).$$

Equation of motion in terms of stress. It is now possible to write down the equations of motion of the fluid in terms of the stress tensor, by application of d'Alembert's principle to the equilibrium of the fluid contained within the elementary parallelepiped specified in the last paragraph. The difference $\delta dF(\xi)$ between the stresses on the opposite faces bounded by $d\eta$, $d\zeta$ is given by

$$\delta dF(\xi)^i = dF(\xi)^i{}_{,a} d\xi^a$$
$$= P^{ij}{}_{,a} dS(\xi)_j d\xi^a$$
$$= P^{ij}{}_{,a} E_{jbc} d\xi^a d\eta^b d\zeta^c \qquad \dots\dots(8{\cdot}8).$$

Again, the mass acceleration of the element of fluid is

$$\rho f^i E_{abc} d\xi^a d\eta^b d\zeta^c$$

using the expression for the element of volume given in equation (5·9). Equating this to the resultant stress (assuming no external forces), we have the equations

$$\rho f^i E_{abc} d\xi^a d\eta^b d\zeta^c = \delta dF(\xi)^i + \delta dF(\eta)^i + \delta dF(\zeta)^i$$
$$= \{P^{ij}{}_{,a} E_{jbc} + P^{ij}{}_{,b} E_{jca} + P^{ij}{}_{,c} E_{jab}\} d\xi^a d\eta^b d\zeta^c$$
$$= P^{ia}{}_{,a} E_{abc} d\xi^a d\eta^b d\zeta^c \qquad \dots\dots(8{\cdot}9),$$

using the result of equation (B·1). Hence

$$P^{ia}{}_{,a} = \rho f^i$$
$$= \rho \left\{ \frac{\partial u^i}{\partial t} + u^i{}_{,a} u^a \right\} \qquad \dots\dots(8{\cdot}10),$$

which is the required equation of motion.

§9. PROOF OF THE EQUATIONS OF VISCOUS FLOW

In order to obtain the final equations of motion in terms of the velocities it remains to formulate a relation between the stress components and the velocities. Consider the vector $d\mathbf{F}$ representing the stress across an arbitrary element of area $d\mathbf{S}$ in the fluid. The vector $d\mathbf{F}$ must satisfy the following conditions: (i) It must be a linear vector function of $d\mathbf{S}$. (ii) It must be a linear function of the first tensor derivative of the velocity vector **u**. This corresponds to the ordinary assumption on which the theory of viscous flow is based. (iii) The function must contain no additional variables other than scalar functions of the density, temperature, etc. since the properties of the fluid are non-directional.

There is no need at this stage to make use of the additional condition that the results must be independent of the vorticity; it will appear that this follows automatically from the condition proved in equation (8·7) that the stress tensor P is symmetrical.

The vector $d\mathbf{F}$ must therefore be given by an equation of the form

$$d\mathbf{F} = \alpha d\mathbf{A} + \beta d\mathbf{B} + \gamma d\mathbf{C} + \ldots \qquad \ldots\ldots(9{\cdot}1),$$

where $d\mathbf{A}$, $d\mathbf{B}$, $d\mathbf{C}$, etc. are vector functions of $d\mathbf{S}$ and the tensor derivative of $\mathbf{u}$ which satisfy the above conditions, and α, β, γ, etc. are scalars.

It appears on examination that there are only four independent vectors satisfying the above conditions. These are

$d\mathbf{A} = d\mathbf{S}$ $\qquad \ldots\ldots(9{\cdot}2),$

$d\mathbf{A}' = u^a{}_{,a}\, d\mathbf{S},$

$d\mathbf{B}$ = the change of $\mathbf{u}$ in a displacement PQ equal to the vector $d\mathbf{S}$ (i.e. perpendicular to $d\mathbf{S}$ and equal to it). Hence from equation (6·2)

$$dB^i = dS^a u^i{}_{,a} \qquad \ldots\ldots(9{\cdot}3),$$

$d\mathbf{C}$ = the gradient of the scalar product of $\mathbf{u}$ and $d\mathbf{S}$ treating $d\mathbf{S}$ as a constant.

$$dC_i = \frac{\partial}{\partial x_i}(dS_a u^a), \ (dS_a \text{ constant}),$$

$$= dS_a u^a{}_{,i} \qquad \ldots\ldots(9{\cdot}4).$$

Hence we may write

$$dF^i = \alpha dS^i + \beta dS^a u^i{}_{,a} + \gamma dS_a u^{a,i}$$

$$= \{\alpha g^{ia} + \beta u^{i,a} + \gamma u^{a,i}\}\, dS_a \qquad \ldots\ldots(9{\cdot}5),$$

where

$$\alpha = \alpha_0 + \alpha_1 u^a{}_{,a} \qquad \ldots\ldots(9{\cdot}6),$$

and $\alpha_0, \alpha_1, \beta, \gamma$ are scalar functions of the temperature, density etc. only. It is interesting to notice that the last term will contribute nothing to the equations of motion for an incompressible fluid. Comparing with equation (8·1), it follows, since $d\mathbf{S}$ is arbitrary, that

$$P^{ij} = \alpha g^{ij} + \beta u^{i,j} + \gamma u^{j,i}$$

$$= \alpha g^{ij} + \beta\,(u^{i,j} + u^{j,i}) \qquad \ldots\ldots(9{\cdot}7),$$

γ being set equal to β since P^{ij} has been proved to be symmetrical. This proves that P^{ij} involves the pure rate of strain only and is independent of the vorticity.

We might equally well start with equation (9·7), basing it on the assumption that P^{ij} is a linear function of the pure rate of strain. The above development, being based on equations between vectors, appears, however, to be susceptible of a more definite physical interpretation. Thus for an arbitrary element of area, the first term $\alpha d\mathbf{S}$ represents a normal pressure on the element; the second term $\beta d\mathbf{B}$ represents a force parallel and proportional to the rate of change of velocity in a direction normal to $d\mathbf{S}$; the third term $\gamma d\mathbf{C}$ is required to maintain the rotational equilibrium of a volume element of fluid.

We can now write down the equations of motion by substituting the values of P^{ij} from equation (9·7) in equation (8·10). The resulting equation is

$$\rho f^i = P^{ia}{}_{,a}$$

$$= g^{ia}\frac{\partial \alpha}{\partial x_a} + \beta\,\{u^i{}_{,a}{}^{,a} + u^{a,i}{}_{,a}\}$$

$$= g^{ia}\frac{\partial}{\partial x_a}\{\alpha + \beta u^b{}_{,b}\} + \beta u^i{}_{,a}{}^{,a} \qquad \ldots\ldots(9{\cdot}8).$$

It is here assumed that β is independent of position, and in reducing the last term use is made of the condition that the order of differentiation is indifferent.

It is usual to express α in terms of the mean pressure $p\ (= -\frac{1}{3}P^a_a)$ defined by equation (8·2) above. Equation (9·7) may be written

$$P^i_j = \alpha\delta^i_j + \beta\,(u^i{}_{,j} + u_j{}^{,i}) \qquad \text{.....(9·9).}$$

Contracting by putting $i = j$ gives

$$-3p \equiv P^a_a = 3\alpha + 2\beta u^a{}_{,a}$$

or

$$\alpha = -p - \tfrac{2}{3}\beta u^a{}_{,a} \qquad \text{.....(9·10).}$$

Substitution in (9·8) (f^i being written in full and β identified with μ, the coefficient of viscosity) gives*

$$\rho\left\{\frac{\partial u^i}{\partial t} + u^i{}_{,a}u^a\right\} = g^{ia}\frac{\partial}{\partial x_a}\left\{-p + \tfrac{1}{3}\mu u^a{}_{,a}\right\} + \mu u^{i,a}{}_{,a} \qquad (9\cdot11),$$

as the final form of the equations of motion.

The corresponding expression for P^{ij} is

$$P^{ij} = -g^{ij}\,(p + \tfrac{2}{3}\mu u^a{}_{,a}) + \mu\,(u^{i,j} + u^{j,i}) \qquad \text{.....(9·12).}$$

It is important to notice that in virtue of equations (9·10) and (9·6), p may be expressed in the form

$$-p = \alpha_0 + (\alpha_1 + \tfrac{2}{3}\mu)\,u^a{}_{,a}$$

where α_0, α_1 are functions of density and temperature only†.

The equations of motion for covariant components and the expression for mixed and covariant components of the stress tensor can be written down at once from equations (9·11) and (9·12).

§ 10. REDUCTION TO GENERAL ORTHOGONAL COORDINATES

It has been shown that the condition that the coordinate system is orthogonal is that g_{ij} is zero when $i \neq j$. It is convenient to write

$$ds^2 = a^2\,dx^2 + b^2\,dy^2 + c^2\,dz^2 \qquad \text{.....(10·1),}$$

so that

$$g_{11} = a^2, \text{ etc.}, \quad g = a^2b^2c^2, \quad g^{11} = 1/a^2 \text{ etc.} \qquad \text{.....(10·2).}$$

We shall also make use of the ordinary components of velocity, which will be denoted by U, V, W. It was shown in equation (2·12) that they satisfy the relations

$$u^1 = \sqrt{g^{11}}U = U/a, \text{ etc.} \qquad \text{.....(10·3),}$$

and

$$u_1 = \sqrt{g_{11}}U = aU, \text{ etc.} \qquad \text{.....(10·4).}$$

Again if $dS\,(1)$, $dS\,(2)$, $dS\,(3)$ are actual elements of area of the three coordinate surfaces, the three ordinary components of stress across $dS\,(1)$ are written in the ordinary notation $p_{11}\,dS\,(1)$, $p_{12}\,dS\,(1)$, $p_{13}\,dS\,(1)$. In orthogonal coordinates the covariant components of $dS\,(1)$ are $\{a dS\,(1),\ 0,\ 0\}$ and the contravariant components of the stress are:

$$\{(1/a)\,p_{11}dS\,(1),\quad (1/b)\,p_{12}dS\,(1),\quad (1/C)p_{13}dS\,(1)\}$$

by equation (2·12). Hence equation (8·1) gives:

$$\left.\begin{aligned}(1/a)p_{11}dS\,(1) &= aP^{11}dS\,(1)\\ (1/b)\,p_{12}dS\,(1) &= aP^{12}dS\,(1)\end{aligned}\right\} \qquad \text{.....(10·5),}$$

* The equation was given in this form by Milne, in *Proc. Camb. Phil. Soc.* **20**, 344 (1921).

† See Lamb, *Hydrodynamics*, 4th edition, § 358, equation (1), where μ' is written in place of $\alpha_1 + \frac{2}{3}\mu$.

etc., so that the nine components of stress as usually defined are

$$\left.\begin{matrix} a^2P^{11}, & ab\,P^{12}, & ac\,P^{13} \\ ab\,P^{21}, & b^2P^{22}, & bc\,P^{23} \\ ac\,P^{31}, & bc\,P^{32}, & c^2P^{33} \end{matrix}\right\} \qquad \text{.....(10·6).}$$

All the expressions occurring in the equations of motion and in the expressions for the stresses have been reduced to forms which do not involve Christoffel symbols*; it is only necessary to substitute the above values to obtain the equations of motion in orthogonal curvilinear coordinates in full.

For the equations of motion in the ordinary components it is sufficient to multiply the first contravariant equation by a or divide the covariant equation by a; for the stresses, the above expression in terms of the contravariant components or the corresponding expressions in terms of the mixed or covariant components may be used. It is sufficient to work out a single example in full and we shall choose the "curl" which is required in reducing $u^{i,a}{}_{,a}$ by using the formula

$$u^{i,a}{}_{,a} = g^{i\alpha}\frac{\partial}{\partial x_\alpha}(u^a{}_{,a}) - (\text{curl curl } u)^i \qquad \text{.....(10·7).}$$

We have from equation (6·11)

$$(\text{curl } u)^i = E^{ijk}\frac{\partial}{\partial x_j}(g_{kl}\,u^l).$$

Hence for the first component in orthogonal coordinates

$$(\text{curl } u)^1 = E^{123}\frac{\partial}{\partial x_2}g_{33}u^3 + E^{132}\frac{\partial}{\partial x_3}g_{22}u^2$$

(since the summation for the $g_{\alpha\beta}$ disappears) or in the notation of this section,

First component of curl $(U, V, W) = a\,(\text{curl } u)^1$

$$= \frac{a}{abc}\left\{\frac{\partial}{\partial y}c^2.\frac{W}{c} - \frac{\partial}{\partial z}b^2.\frac{V}{b}\right\}.$$

The first of the three equations of viscous flow in general orthogonal coordinates may now be obtained by multiplying the first contravariant equation by a; the resulting equation is:

$$\begin{aligned}\rho\left\{\frac{\partial U}{\partial t} + \frac{1}{a}\left(\frac{U}{a}\frac{\partial}{\partial x} + \frac{V}{b}\frac{\partial}{\partial y} + \frac{W}{c}\frac{\partial}{\partial z}\right)aU - \frac{U^2}{a^2}\frac{\partial a}{\partial x} - \frac{V^2}{ab}\frac{\partial b}{\partial x} - \frac{W^2}{ac}\frac{\partial c}{\partial x}\right\} \\ = -\frac{1}{a}\frac{\partial p}{\partial x} + \frac{4\mu}{3a}\frac{\partial}{\partial x}\frac{1}{abc}\left(\frac{\partial bcU}{\partial x} + \frac{\partial caV}{\partial y} + \frac{\partial abW}{\partial z}\right) \\ - \frac{\mu}{bc}\left\{\frac{\partial}{\partial y}\frac{c}{ab}\left(\frac{\partial bV}{\partial x} - \frac{\partial aU}{\partial y}\right) - \frac{\partial}{\partial z}.\frac{b}{ac}\left(\frac{\partial aU}{\partial z} - \frac{\partial cW}{\partial x}\right)\right\} \qquad \text{.....(10·8).}\end{aligned}$$

The typical equations for the stresses may similarly be derived from equations (7·13) and 9·12) in the form

$$p_{11} = -p - \frac{2\mu}{3}\frac{1}{abc}\left(\frac{\partial bcU}{\partial x} + \frac{\partial caV}{\partial y} + \frac{\partial abW}{\partial z}\right) + \frac{2\mu}{a}\left\{\frac{\partial U}{\partial x} + \frac{V}{b}\frac{\partial a}{\partial y} + \frac{W}{c}\frac{\partial a}{\partial z}\right\} \qquad \text{...(10·9),}$$

$$p_{23} = \mu\left\{\frac{b}{c}\frac{\partial}{\partial z}\left(\frac{V}{b}\right) + \frac{c}{b}\frac{\partial}{\partial y}\left(\frac{W}{c}\right)\right\} \qquad \text{.....(10·10).}$$

* $u_{i,\alpha}u^\alpha$ in equation (7·8); $u^\alpha{}_{,\alpha}$ in equation (6·7); $u^{i,j} + u^{j,i}$ in equation (7·13); $u^{i,\alpha}{}_{,\alpha}$ in equations (6·12), (6·7) and (6·11), or Appendix E.

The equations for any special case of orthogonal coordinates such as spherical polar coordinates may be obtained at once from the expression for ds^2

$$ds^2 = dr^2 + r^2 d\theta^2 + r^2 \sin^2\theta d\phi^2$$

as compared with $ds^2 = a^2 dx^2 + b^2 dy^2 + c^2 dz^2$,

and it is only necessary to substitute

$$x = r,\ y = \theta,\ z = \phi,\ a = 1,\ b = r,\ c = r\sin\theta,\ U = U_r,\ V = U_\theta,\ W = U_\phi$$

to get the first equation, and the proper permutations of a, b, c, U, V, W to get the second and third equations.

§ 11. ELIMINATION OF THE PRESSURES

By the taking of the curl of both sides of the equations of motion (equation 9·11) the pressure p is eliminated by virtue of the obvious result that the curl of the gradient of any scalar is zero. The resulting contravariant equation is

$$\frac{\partial}{\partial t}(\text{curl } u^i)^i + \{\text{curl } (u^j{}_{,b} u^b)\}^i = \nu\,(\text{curl } u^i{}_{,a}{}_{,a})^i* \qquad \ldots\ldots(11\cdot1),$$

where ν is written for μ/ρ the kinematic coefficient of viscosity. Using the relation

$$u^i{}_{,a}{}_{,a} = g^{ib}\frac{\partial}{\partial x_b}(u^a{}_{,a}) - (\text{curl curl } u)^i \qquad \ldots\ldots(11\cdot2),$$

the last term reduces to

$$-\nu\,(\text{curl curl curl } u)^i.$$

The details of the reduction of the second term are given in Appendix F. Writing ξ for curl u, as in equation (7·12), we can show that

$$\{\text{curl } (u^j{}_{,b} u^b)\}^i = u^a\frac{\partial \xi^i}{\partial x_a} - \xi^a\frac{\partial u^i}{\partial x_a} + (u^a{}_{,a})\,\xi^i \qquad \ldots\ldots(11\cdot3).$$

The equation of motion therefore reduces to

$$\frac{\partial}{\partial t}\xi^i + u^a\frac{\partial \xi^i}{\partial x_a} - \xi^a\frac{\partial u^i}{\partial x_a} + (u^a{}_{,a})\,\xi^i = -\nu\,(\text{curl curl } \xi)^i \quad \ldots(11\cdot4)\dagger,$$

with $\xi = \text{curl } \mathbf{u}$.

For an incompressible fluid it is possible to define a "vector potential" $\mathbf{h}$‡ by the relations

$$\mathbf{u} = \text{curl } \mathbf{h}$$

$$h^q{}_{,a} = 0 \qquad \ldots\ldots(11\cdot5),$$

so that $\xi = \text{curl curl } \mathbf{h}$ $\qquad \ldots\ldots(11\cdot6)$,

and the last term on the left hand side of equation (11·4) vanishes.

* The notation (curl $A^q)^i$ stands for a contravariant component of the curl of the vector whose contravariant components are A^i, and is identical with (curl $A)^i$. The former notation is necessary when A^i is an expression such as $u^i{}_{,b} u^b$.

† Lamb, *Hydrodynamics*, 4th edition, § 328, equation (8). ‡ *Ibid.* § 148.

§ 12. MOTION SYMMETRICAL ABOUT AN AXIS

The last result leads directly to Stokes' form for the equation of viscous flow when the motion is symmetrical about an axis. We take for the third coordinate x_3, the angle of azimuth of the point P with reference to the axis of symmetry, and for x_1, x_2 any two-dimensional system of coordinates in planes through the axis. It follows that

$$ds^2 = g_{11}dx_1^2 + 2g_{12}dx_1dx_2 + g_{22}dx_2^2 + \varpi^2 dx_3^2,$$

and that all the $g_{\alpha\beta}$ are independent of x_3, ϖ being the normal distance of P from the axis.

We now assume for the components of the vector potential $\mathbf{h}$ that $h_1 = h_2 = 0$; then it may be verified that $u_3 = 0$ and that u_1, u_2 are functions of h_3 which automatically satisfy the equation of continuity for an incompressible fluid in virtue of the identity

$$\operatorname{div}\operatorname{curl}\mathbf{A} \equiv 0$$

and therefore the above assumption with regard to $\mathbf{h}$ gives the most general motion of an incompressible fluid of the kind considered. It may also be shown for

$$(\operatorname{curl}\operatorname{curl}\mathbf{h}) \equiv \boldsymbol{\xi}$$

that $$\xi_1 = \xi_2 = 0,$$

and similarly that $$(\operatorname{curl}\operatorname{curl}\boldsymbol{\xi})_1 = (\operatorname{curl}\operatorname{curl}\boldsymbol{\xi})_2 = 0,$$

all variables being independent of x_3. On examination it appears that the equations of motion in the form (11·4) reduce to a single equation in the variable h_3.

In order to identify h_3 with Stokes' stream function ψ, we proceed to obtain an expression for the flow across any surface of revolution bounded by a circle about the axis. By Stokes' theorem and the definition of the vector potential $\mathbf{h}$ for an incompressible fluid, this is equal to the line integral of $\mathbf{h}$ taken round the curve, i.e. to $\int h_\alpha dx^\alpha$ taken round the curve. In the present case this reduces to

$$\int_0^{2\pi} h_3 dx_3 = 2\pi h_3$$

which agrees with the definition of Stokes' stream function ψ*.

It is possible to reduce the equations of motion in the form (11·4) to a single tensor equation in the variables $x_1, x_2 : h_3 (= \psi)$, ϖ and ξ_3 being treated as scalars. It is sufficient for the present purpose to consider the special case in which the two-dimensional coordinates are orthogonal.

Orthogonal coordinates symmetrical round an axis. Using the notation of § 10, putting $c = \varpi$, and using equation (6·10), we can verify that

$$u_1 = (\operatorname{curl} h)_1 = \frac{a}{\varpi b}\frac{\partial\psi}{\partial y} \qquad \text{.....(12·1),}$$

$$u_2 = (\operatorname{curl} h)_2 = -\frac{b}{\varpi a}\frac{\partial\psi}{\partial x} \qquad \text{.....(12·2),}$$

$$u_3 = 0,$$

* Lamb, *Hydrodynamics*, § 94.

writing ψ in place of h_3. Hence

$$U = \frac{1}{\varpi b}\frac{\partial \psi}{\partial y} \qquad \text{.....(12·3),}$$

$$V = -\frac{1}{\varpi a}\frac{\partial \psi}{\partial x} \qquad \text{.....(12·4).}$$

Further, we have $\xi_1 = \xi_2 = 0$,

$$\xi_3 = (\text{curl curl } h)_3$$

$$= -\frac{\varpi}{ab}\left\{\frac{\partial}{\partial x}\frac{b}{\varpi a}\frac{\partial \psi}{\partial x} + \frac{\partial}{\partial y}\frac{a}{\varpi b}\frac{\partial \psi}{\partial y}\right\} \qquad \text{.....(12·5),}$$

$$= -D(\psi),$$

say. It is of interest to notice how this expression differs from the expression for $\nabla^2\phi$ in two dimensions, where ϕ is a scalar; this takes the form

$$\nabla^2\phi = \frac{1}{ab}\left\{\frac{\partial}{\partial x}\frac{b}{a}\frac{\partial \phi}{\partial x} + \frac{\partial}{\partial y}\frac{a}{b}\frac{\partial \phi}{\partial y}\right\} \qquad \text{.....(12·6).}$$

The equations of motion (11·4) may now be written down in the form

$$\frac{\partial \xi_3}{\partial t} + \frac{\varpi}{ab}\left\{\frac{\partial \psi}{\partial y}\frac{\partial}{\partial x}\left(\frac{\xi_3}{\varpi^2}\right) - \frac{\partial \psi}{\partial x}\frac{\partial}{\partial y}\left(\frac{\xi_3}{\varpi^2}\right)\right\} = -\nu D(\xi_3) \qquad \text{.....(12·7),}$$

subject to equation (12·5) above. The vorticity ω as usually defined satisfies the relation

$$\omega = \xi_3/\varpi.$$

APPENDIXES

Appendix A. *Summary of notation and conventions.*

A tensor is expressed by the notation

$$A_{jc}^{iab}$$

in which there may be any number of suffixes. The upper or lower position of the suffix is important as denoting contravariance or covariance. If the suffixes are all different it is understood that a suffix may take any of the values 1, 2, 3 (in three dimensions); the above expression therefore stands for 3^5 different components. The only case in which the suffix may be repeated is when there are two identical suffixes, one upper and one lower. By the *summation convention* an expression such as

$$A_{ab}^{iab}$$

stands for $\Sigma_{a=1,2,3}\Sigma_{b=1,2,3}A_{ab}^{iab}$, the signs of summation being omitted for convenience. It follows that the suffixes a and b in the above term are dummies and may be replaced freely by any other letter not occurring in the same term, e.g.

$$A_{ab}^{iab} \equiv A_{jk}^{ijk}$$

but A_{bb}^{ibb} is not a tensor. Apart from repeated suffixes, all terms in a tensor equation must carry the same suffixes in a similar position, for example the equation

$$A^{ij} = B^{ik}C_k^{\ j} + M_{ab}^{\ ia}N^{jb},$$

is legitimate. These rules extend to the partial differential coefficients in a trans-

formation, the coefficient of the variable in the numerator of a partial differential coefficient being treated as an upper suffix, for example

$$\bar{A}_{jk}{}^{i} = \frac{\partial \bar{x}_i}{\partial x_a}\frac{\partial x_b}{\partial \bar{x}_j}\frac{\partial x_c}{\partial \bar{x}_k} A_{bc}{}^{a}$$

and the summation convention still holds.

A vector is represented when necessary by the symbol **A** and its tensor components by A^i, A_i. Expressions containing commas among the suffixes, such as $A_{i,j}$, $A^i{}_{,a}$, $A_i{}^{,a}$, $A_{i,j,k}$, etc., stand for tensor derivatives as defined in § 6 and Appendix C.

Appendix B. *Additional properties of epsilon tensors.*

(1) If $A_k{}^{ij}$ is any tensor of the kind indicated, then

$$A_a{}^{ij}E_{jbc} + A_b{}^{ij}E_{jca} + A_c{}^{ij}E_{jab} = A_j{}^{ij}E_{abc} \qquad \text{.....(B·1).}$$

Proof. If a, b, c are all different, both sides are zero. If, say, $b = c$, right hand side is zero and left hand side is the sum of terms of the type

$$(A_b{}^{ij}E_{jba} + A_b{}^{ij}E_{jab}) = 0.$$

If a, b, c are all different and equal to α, β, γ, which are fixed, i.e. they represent the numbers 1, 2, 3, in some order, then left hand side is equal to

$$A_\alpha{}^{i\alpha}E_{\alpha\beta\gamma} + A_\beta{}^{i\beta}E_{\beta\gamma\alpha} + A_\gamma{}^{i\gamma}E_{\gamma\alpha\beta}, \text{ not summed,}$$
$$= A_j{}^{ij}E_{\alpha\beta\gamma}, \text{ summed.}$$

(ii) If A^{jk} and B^{abc} are arbitrary vectors

$$\{E_{ija}E_{kbc} + E_{ijb}E_{kca} + E_{ijc}E_{kab}\}A^{jk}B^{abc} = E_{ijk}A^{jk} \times E_{abc}B^{abc}$$
$$= \tfrac{1}{2}E_{ijk}(A^{jk} - A^{kj}) \times E_{abc}B^{abc} \qquad \text{.....(B·2).}$$

Proof. Write left hand side $\equiv C \equiv C_1 + C_2 + C_3$. Since both sides vanish if $i = j$, we may put $i = \alpha$, $j = \beta$ where α, β, γ are fixed and all different. Hence $C_1 = 0$ unless $a = \gamma$. For $C_1 \neq 0$, $E_{kbc} \neq 0$: if $b = \gamma = a$, $C_3 = 0$, $C_2 = -C_1$, $C = 0$; if $c = \gamma = a$, $C_2 = 0$, $C_3 = -C_1$, $C = 0$; if $k = \gamma = a$,

$$C = E_{ijk}A^{jk}E_{abc}B^{abc}$$

restricted by the condition $a = \gamma$. Similarly for $C_2 \neq 0$, $C_3 \neq 0$, C has the above value restricted by the conditions $b = \gamma$, $c = \gamma$, respectively. Hence the total value of C is given by

$$C = E_{ijk}A^{jk}.E_{abc}B^{abc}$$

unrestricted,

$$= \tfrac{1}{2}E_{ijk}(A^{jk} - A^{kj}).E_{abc}B^{abc}.$$

Appendix C. *Covariant derivatives.*

It is required to find expressions for the tensors $A^i{}_{,a}$, $A_{i,a}$ defined by the equation

$$\delta A^i = A^i{}_{,a}dx^a \qquad \text{.....(C·1),}$$
$$\delta A_i = A_{i,a}dx^a \qquad \text{.....(C·2),}$$

where A^i, A_i are components of the vector $\mathbf{A}$ as defined in § 2. Let $\bar{A}^a = \bar{A}_a$ be components of the vector in the Cartesian system $\bar{X}_a$. Then

$$\delta \bar{A}_a = \frac{\partial \bar{A}_a}{\partial \bar{X}_b} d\bar{X}^b$$

and therefore

$$\delta A_i = \frac{\partial \bar{X}_a}{\partial x_i} \cdot \frac{\partial \bar{A}_a}{\partial \bar{X}_b} \cdot \frac{\partial \bar{X}_b}{\partial x_c} dx^c \qquad \text{.....(C·3),}$$

and

$$\frac{\partial A_i}{\partial x_c} = \frac{\partial}{\partial x_c}\left\{\frac{\partial \bar{X}_b}{\partial x_i} \bar{A}_b\right\}$$

$$= \frac{\partial \bar{X}_a}{\partial x_c} \cdot \frac{\partial \bar{X}_b}{\partial x_i} \cdot \frac{\partial \bar{A}_b}{\partial \bar{X}_a} + \frac{\partial^2 \bar{X}_b}{\partial x_i \partial x_c} \cdot \frac{\partial x_r}{\partial \bar{X}_b} \cdot A_r \qquad \text{.....(C·4).}$$

Comparing 3 and 4, we have

$$\delta A_i = \left\{\frac{\partial A_i}{\partial x_c} - A_r \frac{\partial x_r}{\partial \bar{X}_b} \cdot \frac{\partial^2 \bar{X}_b}{\partial x_i \partial x_c}\right\} dx^c \qquad \text{.....(C·5),}$$

or

$$A_{i,c} = \frac{\partial A_i}{\partial x_c} - \Gamma_{ic}{}^r A_r \qquad \text{.....(C·6),}$$

where

$$\Gamma_{ic}{}^r = \frac{\partial x_r}{\partial \bar{X}_b} \cdot \frac{\partial^2 \bar{X}_b}{\partial x_i \partial x_c} \qquad \text{.....(C·7).}$$

To express $\Gamma_{bc}{}^a$ in terms of the derivatives of the g_s we have, from the definition in equation (2·2),

$$\frac{\partial g_{ij}}{\partial x_k} = \frac{\partial \bar{X}_b}{\partial x_j} \cdot \frac{\partial^2 \bar{X}^b}{\partial x_i \partial x_k} + \frac{\partial \bar{X}_b}{\partial x_i} \cdot \frac{\partial^2 \bar{X}^b}{\partial x_j \partial x_k} \qquad \text{.....(C·8).}$$

Write down the other two equations obtainable by interchanging ijk, add two together and subtract the third, and we get

$$\Gamma_{ij;k} \equiv \frac{1}{2}\left(\frac{\partial g_{jk}}{\partial x_i} + \frac{\partial g_{ki}}{\partial x_j} - \frac{\partial g_{ij}}{\partial x_k}\right) = \frac{\partial \bar{X}_b}{\partial x_k} \cdot \frac{\partial^2 \bar{X}^b}{\partial x_i \partial x_j} \qquad \text{.....(C·9).}$$

This expression is called a Christoffel symbol of the first kind. Multiply equation (C·7) by

$$g_{sr} = \frac{\partial \bar{X}^b}{\partial x^r} \cdot \frac{\partial \bar{X}_b}{\partial x_s}$$

and we get

$$g_{sr}\, \Gamma_{ic}{}^r = \frac{\partial \bar{X}_b}{\partial x_s} \cdot \frac{\partial^2 \bar{X}^b}{\partial x_i \partial x_c} = \Gamma_{ic;s}.$$

It follows that

$$\Gamma_{ij}{}^r = g^{rk}\, \Gamma_{ij;k}$$

$$= \tfrac{1}{2} g^{rk}\left(\frac{\partial g_{jk}}{\partial x_i} + \frac{\partial g_{ki}}{\partial x_j} - \frac{\partial g_{ij}}{\partial x_k}\right).$$

Similarly it may be proved that

$$A^i{}_{,a} = \frac{\partial A^i}{\partial x_a} + \Gamma_{ra}{}^i A^r.$$

Appendix D. *Proof of the curl curl identity.*

This takes the form

$$(\text{curl curl } A)_i = \frac{\partial}{\partial x_i}(A^a{}_{,a}) - A_i{}^{,a}{}_{,a}$$

or

$$\text{curl curl } \mathbf{A} = \text{grad. div. } \mathbf{A} - \nabla^2 \mathbf{A}.$$

From equation (6·8)

$$(\text{curl } A)_t = g_{ta}E^{abc}A_{c,b},$$

$$(\text{curl curl } A)_t = g_{ta}g_{ck}E^{abc}E^{kst}A_{t,s,b}$$

$$= g^{br}E_{trk}E^{kst}A_{t,s,b}$$

$$= g^{br}(\delta_t^t\delta_r^s - \delta_r^s\delta_t^t)A_{t,s,b},$$

by equation (5·3)

$$= g^{br}A_{r,t,b} - g^{br}A_{t,r,b}$$

$$= \frac{\partial}{\partial x_t}(A^b{}_{,b}) - A_t{}^{,a}{}_{,a},$$

the order of differentiation being assumed to be indifferent.

Appendix E. *Second method of simplifying the last term in the equation of viscous flow.*

The following method does not involve the use of the epsilon tensors, and is therefore more direct than that previously used. The resulting term in general orthogonal coordinates is more symmetrical than in equation (10·8) but is also considerably more complicated. The method is most simply applied to the covariant components, the term to be evaluated being $g^{bc}u_{t,b,c}$.

We have in full

$$u_{a,b} = \frac{\partial u_a}{\partial x^b} - \Gamma_{ab}{}^r u_r$$

and

$$A_{ab,c} = \frac{\partial A_{ab}}{\partial x^c} - \Gamma_{ac}{}^r A_{rb} - \Gamma_{bc}{}^r A_{ar}.$$

Hence, putting $A_{ab} = u_{a,b}$

$$u_{a,b,c} = \frac{\partial^2 u_a}{\partial x^b \partial x^c} - \Gamma_{ac}{}^r \frac{\partial u_r}{\partial x^b} - \Gamma_{bc}{}^r \frac{\partial u_a}{\partial x^r} - \Gamma_{ab}{}^r \frac{\partial u_r}{\partial x^c}$$

$$- u_s \frac{\partial \Gamma_{ab}{}^s}{\partial x^c} + \Gamma_{ac}{}^r \Gamma_{rb}{}^s u_s + \Gamma_{bc}{}^r \Gamma_{ar}{}^s u_s \quad \ldots\ldots(\text{E·1}).$$

Hence

$$g^{bc}u_{a,b,c} = g^{bc}\frac{\partial^2 u_a}{\partial x^b \partial x^c} - g^{bc}\Gamma_{bc}{}^r \frac{\partial u_a}{\partial x^r} - 2g^{bc}\Gamma_{ac}{}^r \frac{\partial u_r}{\partial x^b} + \text{terms in } u_s \quad \ldots(\text{E·2}).$$

The first two terms may be combined by analogy with $g^{bc}\left(\frac{\partial \phi}{\partial x^b}\right)_{,c}$ which may be written in the alternative forms

$$g^{bc}\frac{\partial^2 \phi}{\partial x^b \partial x^c} - g^{bc}\Gamma_{bc}{}^r \frac{\partial \phi}{\partial x^r}$$

and

$$\frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}\left(g^{bc}\sqrt{g}\frac{\partial \phi}{\partial x^b}\right).$$

Hence

$$g^{bc}u_{a,b,c} = \frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}\left(g^{bc}\sqrt{g}\frac{\partial u_a}{\partial x^b}\right) - 2g^{bc}\Gamma_{ac}{}^r \frac{\partial u_r}{\partial x^b} + \text{terms in } u_s.$$

In Euclidean space the terms in u_s (terms of zero order in u_a) may be simplified by the following artifice. It appears that the terms of zero order in equation (E·1) are unaltered by interchanging a and b and hence $u_{a,b,c} - u_{b,a,c}$ contains no terms of zero order in u_a. Now

$$g^{bc}u_{a,b,c} - \frac{\partial}{\partial x^a}(g^{bc}u_{b,c}) = g^{bc}(u_{a,b,c} - u_{b,c,a})$$
$$= g^{bc}(u_{a,b,c} - u_{b,a,c} + u_{b,a,c} - u_{b,c,a}),$$

and the last two terms cancel since the order of differentiation is indifferent in Euclidean space. Hence

$$g^{bc}u_{a,b,c} = \frac{\partial}{\partial x^a}\{g^{bc}u_{b,c}\} + \text{terms of 1st and 2nd order}$$
$$= \frac{\partial}{\partial x^a}\left\{\frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}g^{bc}\sqrt{g}\,u_b\right\} + \text{terms of 1st and 2nd order}$$
$$= u_b\frac{\partial}{\partial x^a}\left\{\frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}(g^{bc}\sqrt{g})\right\} + \text{terms of 1st and 2nd order.}$$

The first and second order terms have already been evaluated and we have finally

$$g^{bc}u_{a,b,c} = \frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}\left(g^{bc}\sqrt{g}\frac{\partial u_a}{\partial x^b}\right) - 2g^{bc}\Gamma_{ac}{}^{r}\frac{\partial u_r}{\partial x^b} + u_b\frac{\partial}{\partial x^a}\cdot\frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}(g^{bc}\sqrt{g}),$$

or, writing the second term in full,

$$\frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}\left(g^{bc}\sqrt{g}\frac{\partial u_a}{\partial x^b}\right) - g^{bc}g^{rs}\frac{\partial u_r}{\partial x^b}\left(\frac{\partial g_{as}}{\partial x^c} + \frac{\partial g_{sc}}{\partial x^a} - \frac{\partial g_{ac}}{\partial x^s}\right) + u_b\frac{\partial}{\partial x^a}\frac{1}{\sqrt{g}}\frac{\partial}{\partial x^c}(g^{bc}\sqrt{g}) \quad \text{.....(E·3).}$$

In general orthogonal coordinates in the notation already used this reduces to the following form

$$\frac{1}{a} \times \text{first component of } (g^{bc}u_{i,b,c})$$
$$= \frac{1}{a^2bc}\left\{\frac{\partial}{\partial x}\frac{bc}{a}\cdot\frac{\partial aU}{\partial x} + \frac{\partial}{\partial y}\frac{ca}{b}\cdot\frac{\partial aU}{\partial y} + \frac{\partial}{\partial z}\frac{ab}{c}\cdot\frac{\partial aU}{\partial z}\right\}$$
$$- \frac{2}{a^3}\left\{\frac{1}{a^3}\frac{\partial a}{\partial x}\cdot\frac{\partial aU}{\partial x} + \frac{1}{b^3}\frac{\partial a}{\partial y}\cdot\frac{\partial aU}{\partial y} + \frac{1}{c^3}\frac{\partial a}{\partial z}\cdot\frac{\partial aU}{\partial z}\right\}$$
$$+ \frac{2}{a}\left\{\frac{1}{a^3}\frac{\partial a}{\partial x}\cdot\frac{\partial aU}{\partial x} + \frac{1}{ab^2}\frac{\partial a}{\partial y}\cdot\frac{\partial bV}{\partial x} + \frac{1}{ac^2}\frac{\partial a}{\partial z}\cdot\frac{\partial cW}{\partial x}\right\}$$
$$- \frac{2}{a}\left\{\frac{1}{a^3}\frac{\partial a}{\partial x}\cdot\frac{\partial aU}{\partial x} + \frac{1}{b^3}\frac{\partial b}{\partial x}\cdot\frac{\partial bV}{\partial y} + \frac{1}{c^3}\frac{\partial c}{\partial x}\cdot\frac{\partial cW}{\partial z}\right\}$$
$$+ \frac{1}{a}\left\{aU\frac{\partial}{\partial x}\frac{1}{abc}\frac{\partial}{\partial x}\left(\frac{bc}{a}\right) + bV\frac{\partial}{\partial x}\frac{1}{abc}\frac{\partial}{\partial y}\left(\frac{ca}{b}\right) + cW\frac{\partial}{\partial x}\frac{1}{abc}\frac{\partial}{\partial z}\left(\frac{ab}{c}\right)\right\} \quad \text{.....(E·4),}$$

each line of this equation corresponding to a term of equation (E·3). The sum of the first and second lines may also be written

$$\frac{1}{abc}\left\{\frac{\partial}{\partial x}\frac{bc}{a}\frac{\partial U}{\partial x}+\frac{\partial}{\partial y}\frac{ca}{b}\frac{\partial U}{\partial y}+\frac{\partial}{\partial z}\frac{ab}{c}\frac{\partial U}{\partial z}\right\}$$

$$aU\left\{\frac{1}{a^2}\frac{\partial}{\partial x}\left(\frac{1}{a^2}\frac{\partial a}{\partial x}\right)+\frac{1}{b^2}\frac{\partial}{\partial y}\left(\frac{1}{a^2}\frac{\partial a}{\partial y}\right)+\frac{1}{c^2}\frac{\partial}{\partial z}\left(\frac{1}{a^2}\frac{\partial a}{\partial z}\right)\right\}$$

$$\frac{U}{a^2bc}\left\{\frac{\partial a}{\partial x}\frac{\partial}{\partial x}\left(\frac{bc}{a}\right)+\frac{\partial a}{\partial y}\frac{\partial}{\partial y}\left(\frac{ca}{b}\right)+\frac{\partial a}{\partial z}\frac{\partial}{\partial z}\left(\frac{ab}{c}\right)\right\}.$$

It may be verified by direct substitution that these forms are equivalent to that included in equation (10·8).

Appendix F. *Method of reducing the term* $A^i \equiv \{\text{curl}\,(u^j{}_{,b}u^b)\}^i \equiv E^{iab}\,(u_{b,c}u^c)_{,a}$.

We have

$$\begin{aligned}0 &= \tfrac{1}{2}\{\text{curl grad.}\,(u^b u_b)\}^i\\ &= \tfrac{1}{2}E^{ica}\,(u^b u_b)_{,a,c}\\ &= E^{ica}\,(u^b u_{b,a})_{,c}\\ &= E^{ica}\,\{u^b\,(u_{a,b}+E_{abk}\xi^k)\}_{,c},\end{aligned}$$

since $u_{b,a}-u_{a,b}=E_{abk}\xi^k$ by equations (5·6), 6·8), (7·12),

or
$$0 = A^i + (\delta^i_b\delta^c_k-\delta^i_k\delta^c_b)\,(u^b\xi^k)_{,c},$$

using equation (5·3); or

$$A^i = (\xi^i u^c)_{,c}-(u^i\xi^c)_{,c}.$$

Now
$$\xi^i{}_{,c}u^c-u^i{}_{,c}\xi^c = u^c\frac{\partial\xi^i}{\partial x_c}-\xi^c\frac{\partial u^i}{\partial x_c},$$

since the terms containing Christoffel symbols cancel. Also $\xi^b{}_{,b}$ vanishes identically. Hence we have finally

$$A^i \equiv \{\text{curl}\,(u^j{}_{,b}u^b)\}^i = u^a\frac{\partial\xi^i}{\partial x_a}-\xi^a\frac{\partial u^i}{\partial x_a}+u^a{}_{,a}\xi^i.$$

DISCUSSION

Prof. W. Wilson. Mr Lock has succeeded in showing that the utility of the tensor calculus is not restricted to relativistic investigations and that it can be applied with advantage to hydrodynamical and similar three-dimensional problems in Euclidean space. This is due to the fact that the equations in these problems are, for the most part, statements of the equality of different expressions for the same component of some vector or tensor, and these equations retain their validity when the coordinate system is changed. It is easy to see why the tensor calculus developed in association with the theory of relativity. The most important principle of that theory is that the equations describing physical phenomena have the same form whatever may be the system of reference. I should like to congratulate Mr Lock on his very interesting paper.

Mr T. Smith commended tensor methods, and all algebraic methods in which a single symbol denotes a number of terms, to the attention of physicists, and said that they not only simplify the formulation of problems but also lend themselves to very rapid numerical calculation. The distinction between covariants and contravariants is important; physicists interested in a particular case, as distinct from a more general case, are sometimes misled by the similarity between the coefficients in covariant and contravariant expressions and consequently reach a wrong result. He thought the paper would be improved if the author would explain some points in the notation, such as the use of a comma to denote differentiation.

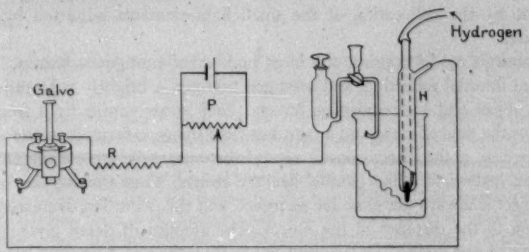
Capt. C. W. Hume. Is it essential to use commas to denote differentiations? Formulae printed in this way are rather confusing, especially when an equation is followed by a punctuational comma.

Author's reply. A good example of the necessity for distinguishing between covariant and contravariant components is the case of the symbol ∇^2 applied to a vector and to a scalar, where, as remarked in § 6, the actual formulae, while identical in Cartesians, are different in curvilinear coordinates. A third different expression is that denoted by $D(\psi)$ in equation (12·5) for the case of motion symmetrical about an axis. Modifications have been made to the paper in proof to explain more clearly the use of the comma notation. In answer to Capt. Hume, the comma, as denoting covariant differentiation, appears the most convenient of the notations one has seen, and, if it is suggested that the comma might escape notice, it may be remarked that Eddington, in his original tracton relativity, used no special notation at all, writing A_{ab} indifferently for a tensor of the second order or for the covariant derivative of the vector A_a. The ordinary differential notation cannot be used, since the covariant derivative is not identical with the ordinary partial derivative.

DEMONSTRATIONS

A *p*H Apparatus. *Demonstration given on January* 24, 1930, *by* W. E. DORAN, M.A., The Cambridge Instrument Co., Ltd.

A solution is either acid, alkaline or neutral, and the degree of effective acidity or alkalinity is entirely dependent upon the concentration of undissociated hydrogen-ions in the solution. This hydrogen-ion concentration, which may be represented in *p*H units, can be accurately determined by immersing a hydrogen electrode in the solution and measuring the electric potential set up. The outfit demonstrated is designed for this purpose, and gives direct readings in *p*H units on the scale. It consists of a hydrogen electrode, calomel electrode, portable potentiometer, reflecting galvanometer and scale, together with a supply of hydrogen. The potentiometer is employed both for standardising the galvanometer deflection and also for balancing out the calomel e.m.f., the deflection of the galvanometer then being due to the e.m.f., of the hydrogen electrode only.



C = Calomel electrode; H = Hydrogen; P = Potentiometer; R = High resistance.

At the beginning of the demonstration, the hydrogen electrode was immersed in a solution which was neither acid nor alkaline, and the scale reading was 7, which is the *p*H value of a neutral solution. By the addition of a drop of nitric acid the *p*H reading was reduced, indicating an increase in acidity, and with a little sodium hydroxide the *p*H value was increased until at 7 it was again neutral. A further addition of sodium hydroxide caused the liquid to assume an alkaline state. In this way it was demonstrated that the acidity of the solution can easily be varied and controlled to almost any desired value by means of the apparatus.

The electrode method of measuring hydrogen-ion concentration is now used in nearly every industrial process. As an example of its value, mention was made of the importance of measuring the acidity of beer during manufacture. If the beer is too acid the effects may be detrimental to the consumer and at least unpleasant to the palate; on the other hand, if the acidity is too low it is favourable to the growth of bacteria. The actual *p*H value varies in different types of beer. Among

other applications may be mentioned the tanning industry, sugar manufacture, bread making, preserved food manufacture, textile dyeing—in fact, there are few industrial processes in which hydrogen-ion concentration does not play an important part.

Model illustrating the Mosaic Theory of the Compound Eye, *lent by* The Department of Zoology, University of London, King's College. *Demonstration given on February* 14, 1930, *by* Capt. C. W. HUME.

The eye of an insect consists of a large number of tubes (ommatidia) formed with small lenses at their outer ends and associated with a retina at their inner ends, the number of ommatidia varying from 10,000 upwards. It has been suggested that each lens forms an independent image, the effects of all the images being combined as in the case of binocular vision. According to Alternburg*, however, the lenses serve merely as condensers. Light from any point of the object can pass through that one of the ommatidia which is pointing directly at it, but is absorbed by the walls of the other ommatidia. An erect image is consequently formed on the retina by the collocation of the small light-elements admitted by different ommatidia.

Altenburg's model consists of a large number of contiguous straws, blackened to prevent internal reflection, and arranged between a brightly coloured and illuminated object and a ground-glass screen. Each straw admits light from a small element of the field of view, and a faint but clear image is formed on the screen.

In the case of the insect eye the axes of the ommatidia are convergent towards the retina, instead of being parallel like the straws. Thus the angular magnitude of the object is always the same for an insect, and the definition decreases in direct proportion to the distance of the object. The amount of detail given by an eye containing 10,000 ommatidia is equivalent to that given in television by a Nipkow disc with 100 apertures, whereas the number of apertures usually employed is about 30.

The illumination of the insect retina is weak, as only a very narrow pencil is admitted from each point on the object. The retina is probably more sensitive than the retina of a vertebrate, but the weakness of illumination may account for the inactivity of insects on cloudy days.

* *British Journal of Experimental Biology*, **4**, 38 (1926).

REVIEWS OF BOOKS

Cambridge Five-figure Tables, by F. G. HALL, M.A. and E. K. RIDEAL, M.A. Pp. viii + 76. (London: Cambridge University Press.) 3*s*. 6*d*.

To the making of books of tables there is no end, but there is still room for a compact book of five-figure tables, published at a reasonable price. The book before us, containing logarithms, natural sines, circular measure, values of e^x and e^{-x}, useful constants, atomic weights, and a list of "useful differentials and integrals," is full but compact, and can be commended. The type is not all that could be desired, but the page is saved from overcrowding by the compilers' practice of breaking it up into short columns and, after the first five-figure entry, printing, in any one column, only the last three figures. No difference columns are necessary. The reviewer confesses to a hankering after the type used in Barlow's tables.

The table of constants is useful and might with advantage be extended, and the list of differential coefficients and integrals will bring joy to the heart of many a toiling chemist.

A. F.

Magnetism, by E. C. STONER, Ph.D. Pp. vii + 114. (London: Methuen & Co., Ltd.) 2*s*. 6*d*.

Dr Stoner has produced a small but very useful work on the magnetic properties of materials. He deals briefly but capably with the magnetic properties of atoms, diamagnetism, paramagnetism, ferromagnetism, and the magnetic properties of the elements. His book is confessedly one for those to whom the subject is not new, and perhaps plunges *in medias res* a little suddenly for those who like to be dosed "with learning put lightly, like powder in jam," but, as the author hopes, it is admirably fitted for those whose desire it is to reach a stage at which they may carry out research on the subject. It can be recommended strongly.

A. F.

Magnetic, Meteorological and Seismographic Observations made at Bombay and Alibag, 1924, under the direction of S. K. Banerji, D.Sc. Pp. iii + 133. (Calcutta: Government of India Publication Branch.) 11*s*.

The delay in the appearance of this volume is of little moment in view of the great advance that it marks, an advance on which India is warmly to be congratulated. The equipment and programme of the Bombay Observatory was largely modelled on that of Kew, but those whose work required hourly values of any of the elements would look in vain for them in the first forty-two annual volumes. The breaking down of tradition caused by the war led to the publication of two-hourly values of all the important elements in 1921, though the cost of printing hourly values was then regarded as prohibitive: but thanks to support from the Royal Society the hourly scheme has now been adopted for the data of pressure, wind, temperature, relative humidity, cloud, the magnetic elements and microseisms. No very abnormal winds are recorded, the greatest velocity being only 27 miles an hour.

G. T. W.

Greenwich Magnetic and Meteorological Observations. Pp. D 62 + E 46. (London: H.M. Stationery Office, 1927.) 7*s.* 6*d.*

The electrification of railways forced the magnetic observatory to be moved from Greenwich to near Abinger on the northern slope of Leith Hill, and the present equipment includes a Schuster-Smith coil magnetometer for absolute measurements of horizontal force as well as an earth inductor for absolute inclination. The meteorological instruments still include a black-bulb radiation thermometer, in spite of the difficulties elsewhere found with changes in the sensitiveness of this instrument.

G. T. W.

The Observatories' Year Book, 1927. Pp. 430. (London: H.M. Stationery Office.) £3. 3*s.*

This volume contains not only data of continuously recorded terrestrial magnetism from Lerwick and Eskdalemuir, of atmospheric electricity from Lerwick, Eskdalemuir and Kew, and of meteorology from Aberdeen, Eskdalemuir, Cahirciveen (Valentia) and Kew; but also of auroras from Lerwick, and of underground water-level, atmospheric pollution and seismology from Kew; and there is an aerological section with upper-air data derived from registering-balloons.

With regard to the microseisms, which are presumably due to waves at sea and so have about seven times the amplitude in winter that they have in summer, it would be interesting to examine whether very long waves of small amplitude could be used as a warning of the existence of distant storms.

India is now playing her part in the publication of hourly values of her data: we hope that other portions of the Empire may see their way to make their geophysical materials more readily available for general use.

G. T. W.

THE PROCEEDINGS OF THE PHYSICAL SOCIETY

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THE GENERATION OF SOUND BY THE SIREN PRINCIPLE

BY E. SIMEON

Communicated by Dr J. S. G. Thomas, March 18, 1930.
Read and discussed May 9, 1930.

ABSTRACT. The paper discusses various undesirable features of the simple siren, considered as a sound-source for technical work, and describes a siren with a reasonably pure note, whose intensity can be kept constant throughout a range of pitch from about 70 to about 7500 cycles.

THE production of a steady note at good strength is quite a simple matter, but to range over six or seven octaves with any certainty as to relative intensity is a good deal more difficult. The air siren (assumed to have a drive independent of the pressure air) seemed to promise well, on the assumption that the intensity (energy) of the sound will be some definite fraction of the input air energy, which is easily measured. In the simple type of siren, by reason of the resonant cavities and the light construction generally and also of a pump-action due to the revolving disc, this result can hardly be counted on, but by suitable design it is possible to remove the sources of error more or less completely, at the same time improving the wave-form and lengthening the useful frequency band at both ends.

Inside the usual air chest, waves from front to back set up by the escaping puffs must always be present to some extent, and may have a choking effect at some frequencies. By employment of a shallow chamber the effect might be avoided within the pitch range under consideration; the absence of an adequate reservoir would, however, both spoil the wave-form and also set up pulsations in the supply pipe to the detriment of some or all of the lower notes.

The best plan appeared to be to construct a push-pull or double siren and mount it on a large baffle board, the sound being radiated half to the front and half to the rear in opposite phases. The total hole area open at any instant would thus, with suitable choice of hole shapes as described later, be a constant. The need for a reservoir would disappear: the air would flow smoothly up to the point where it

divided to the front and back sets of holes, the two sets being in close proximity. The rear radiation, together with any driving noises, would, of course, be kept away from any instrument under test.

The mechanical difficulties in the twin-rotor arrangement are greater than would be expected, and finally the principle shown in Figs. 1 and 2 was adopted. It employs the usual single rotor. Half or more of the output is lost, the remainder, however, being quite adequate for the purpose. In the trial siren of Fig. 1 the stationary front holes are drilled round in sets of three, adjacent sets being in opposite phase and on opposite sides of the separating fin; the latter serves to turn the unwanted sound to the rear as before. In Fig. 2 the stationary holes are in twos, an arrangement less liable to damp high frequencies, since it brings the opposite

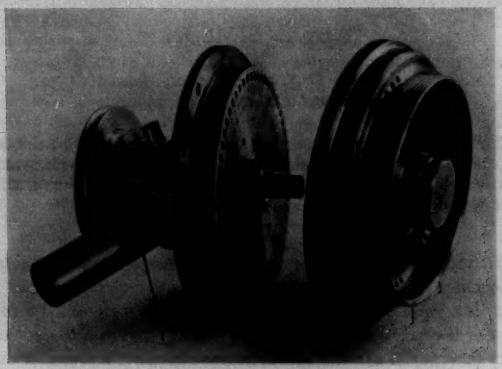

Fig. 1. Trial form of siren.

sets closer together. In passing, it is suggested that the push-pull construction might render more practicable the under-water, hydraulically blown siren. The simple type of siren appears actually to have been tried for submarine signalling, and to have failed to give satisfaction from some cause. Certainly the heavy and incompressible nature of water, as compared with air, calls for radical departure from the conventional design.

Something may be said on the question of wave-form. It has been pointed out* that for a pure note the flow of air should be controlled sinusoidally, e.g. by a disc with a continuous sine curve cut round the edge, the air under constant pressure issuing from a narrow radial slit. The unsatisfactory pointed wave-form with equal circular fixed and moving holes was also shown, and a special hole shape worked out. In the present case the hole dimensions cannot exceed about $\frac{1}{4}$ inch, as the

* E. A. Milne and R. H. Fowler, *Proc. R.S.* A 98, 414 (1921).

peripheral velocity is quite limited; the only practicable shapes are the circle, ellipse, rectangle, etc. The width of moving and fixed holes must evidently be equal, for if one were smaller, no change in the air-flow would occur as it passed within the larger, so that a flat part would occur in the wave-form. For a similar reason the inter-hole width must be equal to the hole.

Fig. 3 shows how the effective area changes during the travel with a pair of equal circular holes, and with one circular and one square hole the same width, the direction of travel being perpendicular to a side. The wave-form with the latter is quite a good imitation of sine form (shown dotted); it is also symmetrical about the

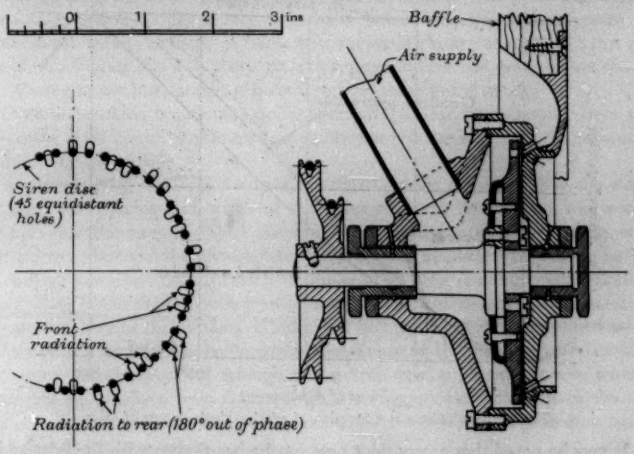

Fig. 2. Final form of siren. Left: diagrammatic view showing drilling of front. Right: longitudinal section.

half-open line, a necessary condition if the two phases are to dovetail and draw air smoothly from the supply. Owing to the inclined drilling introduced in the design, the shape of the stationary holes becomes elliptical, and the rotor holes are accordingly made rectangular. The wave-form is not affected. The actual air flow resulting is probably not quite proportional to these aperture areas, as the holes are so small; at the same time the wave-form should remain much the same at any frequency. In other words, the relative strength of unwanted harmonics will not vary, and they can generally be neglected. The ear readily detects some departure from tuning-fork purity with the siren of Fig. 1; its workmanship was, however, only indifferent.

When the rotor is running at speed it naturally sets up a swirl of air in the case, and this adds something to the pressure on the holes. If the air be free to enter and flow through, a weak note can be produced in this way on all but the lowest part of

the frequency range. The pressure actually existing at the inner end of the fixed holes may be measured if a short connection be made between two opposite phase holes in the front, and thence to the pressure gauge. This pressure, in ordinary testing, will be only an inch or two of water; the low part of the range naturally seems weaker to the ear, and may need more pressure, but a rushing of the air begins to be audible with about four inches. For the determination of the air energy the volume per second is also required. A holder of one to two cubic feet makes a good source of air, and if it be placed at the edge of the baffle there is no danger of sound pick-up.

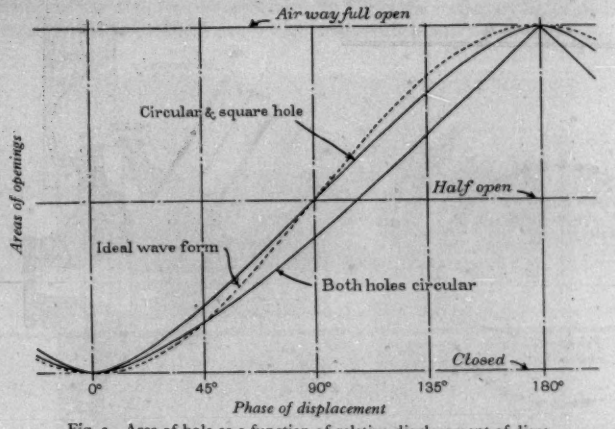

Fig. 3. Area of hole as a function of relative displacement of discs.

It may be noted that a very faint note can be heard even with the inlet and all joints effectively sealed. It is too weak to be of much consequence, so long as small working intensities be avoided at the higher frequencies. It seems to be due to a feeble air-flow from one side of the baffle to the other through both sets of holes, and must be put down to an accidental slope of one set towards the direction of motion, or the fact that one set slopes outwards and the other inwards.

A point of some uncertainty (which, however, applies to some other sources of sound) concerns the distribution of the different frequencies in front of the siren mounted on its baffle. The positions in which tests can be made are confined to the central axis, the appreciable dimensions of the siren relative to short wave-lengths leading otherwise to interference. If the low notes tend to spread out more rapidly than the higher, as seems possible, a source of error will be introduced in testing over a considerable pitch range. The effect, if present, could be detected by the taking of a few measurements with any good class microphone at low, medium and high frequencies, and repetition of the process farther out along the axis. If then found necessary, a correcting factor could no doubt be arrived at in some way.

A few notes on the construction may be added. When the trial siren of Fig. 1 was made it was thought that a speed of 7500 r.p.m. was as much as could be expected from a pulley drive. More recently, however, a different apparatus fitted with a pulley similar to the smaller one on the siren and a $\frac{1}{8}$ in. leather belt ran light at over 13,000 r.p.m. As compactness is very desirable a smaller rotor is shown in the design of Fig. 2 (pitch diameter 3·44 in. with 45 holes and spaces, each 0·12 in. across) to run up to 10,000 r.p.m., or 7500 cycles per second. To avoid distortion it is made from annealed mild steel. A damping disc with paper washer is screwed to it to discourage vibration, and for the same reason the case is substantially made.

The lower pitch limit is rather undefined. A musical tone is distinguishable down to 35 cycles, though a very large baffle would be required to preserve the fundamental at full strength. A baffle 6 ft. square is clumsy enough, but this size should be adequate down to about 70 cycles, provided the test point be not too far out. Owing to the low speeds, it is hard to hold these notes steady.

Open air working is generally preferable; but if a deadened chamber were employed the siren could be mounted in an aperture in the wall, and the unwanted sound dissipated outside.

As already suggested, the purity of the note depends a great deal on the workmanship. The placing and width of the holes ought to be accurate, well within 0·002 in. For the siren of Fig. 1 the rotor disc, and later the front casting, were attached to a plate and the dividing was carried out at five times the drilling radius. All drilling was done through hard bushings in an arm rotating pointer-fashion. The drilling of the sloping holes proved difficult, particularly as regards the setting in opposite phase of the two sets of holes, but this work would be simplified by the slightly altered form of the baffle attachment shown in the drawing. One cause of inaccuracy was the breaking through of the drill at an angle underneath, where a waste disc should have been fastened. A faint raggedness noticeable in the tone may be traced to the hand-filing of the rectangular rotor holes, a procedure hardly good enough for its purpose.

DISCUSSION

Dr A. B. Wood: Mr Simeon is to be congratulated on a very nice piece of work. Both the design of the siren and the actual construction are excellent. The point of particular importance is, of course, the introduction of the artificial load (the rearward holes) between the intervals of useful sound generation (by the forward holes). This should result in a greatly improved smoothness of running of the siren.

With regard to the wave form: presumably Fig. 3 illustrates the theoretical wave form. I should very much doubt, however, whether this will be found to agree with the observed wave-form. It seems to me that the amount of high-pressure air escaping through the overlapping orifices at any instant will depend not only on the area of the combined opening but also on the periphery and general shape of the opening. Eddies at the edges of the opening are bound to have an important influence on the wave-form of the sound generated by the siren. This criticism will apply also to the siren devised by Milne and Fowler.

As Mr Simeon remarks, the fundamental principle of his improved siren might also find an application to a water-siren. The general design would, however, require radical alteration for such use. A siren for signalling in the sea was designed during the war by Sir Charles Parsons. This was driven by high-pressure water and gave ranges of several miles. Difficulties of a practical nature proved to be rather serious and precluded its general use as a submarine signalling device.

Mr R. W. Hardisty: I should like to congratulate Mr Simeon on a most interesting paper on the subject of a device, which, if the claims made for it can be substantiated, will be of great service to workers in applied acoustics.

Telephone engineers have been paying much attention recently to the improvement of the frequency/response curves of carbon microphones as applied both to the ordinary subscriber's telephone instrument and to studio transmitters used in broadcasting stations. Carbon microphones must always be calibrated in an open sound field, and the need for a device with a constant sound-output in any particular direction is acute, owing to the non-linear distortion present in many microphones of this type.

The only available source at present is the loudspeaker which is generally of the moving-coil type; and, as is well known, the frequency-response curves of all loudspeakers are far from linear and are moreover liable to change with atmospheric conditions. These changes produce very considerable errors in the resultant frequency characteristics of microphones.

I should like to ask the author if he has made any measurements of the acoustic output of the device at a point distant about 4 or 5 ft. from it. The output from sirens is quite large as a rule and, as Dr Thomas pointed out when reading the paper, the wave-form could have been improved by utilising only half the width of the orifice. Such an improvement might be desirable for the purpose considered where a sound pressure of only about 5 dynes/cm.^{2} is required.

Author's reply: I must thank Dr Wood and Mr Hardisty for their kind remarks. As Dr Wood suggests, it is unlikely that the wave-form is actually as good as that indicated in Fig. 3, which only pretends to deal with orifice areas. The form would be further degraded by mechanical errors, such as certainly exist in the trial siren. Nevertheless, from experience with the latter I think one could obtain a note quite as pure as that of the flute, for example. Obviously further experiment is required, and this would incidentally show whether the wave-form does remain approximately the same at all frequencies. The question is an essential part of the whole problem, as one could not properly apply the push-pull principle with a definitely asymmetrical wave-form.

The original suggestion as regards under-water sirens has been slightly revised. I imagine that the difficulties ought not to be insuperable, but am totally ignorant on the subject.

No satisfactory measurements of the acoustic energy have yet been obtained, owing to the great difficulty of the process, particularly with limited resources.

With air continually streaming from the siren the Rayleigh disc cannot very well be used; the only method seems to be that employing a calibrated microphone in the open air. Once tested, the siren ought to hold its calibration well, especially if the rotor were made of some tough rustless alloy. The rectangular holes might be produced by milling in from the edge, and shrinking on an outer ring.

The paper is not put forward without apologies for its incompleteness; but some of the sound-sources that have been suggested seem so questionable that one gains confidence in proportion.

I cannot conclude without thanking Dr Thomas for his very kind help and interest on this and many other occasions.

SURFACE RESISTIVITY MEASUREMENTS ON SOLID DIELECTRICS

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The National Physical Laboratory

Received April 3, 1930. *Read and discussed May* 9, 1930.

ABSTRACT. The paper describes (1) a new form of electrode suitable for surface resistivity measurements on insulating materials in sheet form, and (2) a method for the determination of the "volume leakage" correction for any system of electrodes. Data are given on the volume leakage correction for the various types of electrode in general use, and on the "leakage resistivities" of materials commonly used in the construction of laboratory instruments.

§ 1. INTRODUCTION

Most electrical measuring-instruments are provided with terminal fittings mounted on a small panel of a solid dielectric, such as ebonite, bakelite, amber, ivory, or fused quartz. None of these materials is a perfect insulator, and it is found that in many cases the imperfection of the insulation is largely due to the passage of leakage currents over the surface of the material, rather than through its bulk. Accordingly, the measurement of the surface resistivity of materials of this kind is of some importance.

The usual method of measurement is to apply two conducting electrodes to the surface of the material, arranging them so that their nearest edges are parallel and a definite distance apart, 1 cm. say, and then to measure the total leakage current flowing when a known voltage is applied to them. Two difficulties arise: that of ensuring that the electrodes make good contact with the insulating material along their entire length, and that of estimating the leakage current which flows through the body of the material and is measured along with the surface current. A form of electrode that has been found to be very satisfactory for such measurements will now be described, and also a method for determining the correction to be made on account of volume conductance.

§ 2. THE ELECTRODES

An important series of measurements of surface resistivity has been made by Curtis*. His electrodes consisted of metal strips 1 cm. wide which were clamped to the sample (a piece 10 × 10 cm. cut from a flat sheet of the material) as shown in Fig. 1. The difficulty with this arrangement is that the strips bend, and so make good contact at the edges of the sample only. To get over this difficulty, Curtis wrapped the strips round with tinfoil, and pressed this against the insulator

* *Bulletin of Bureau of Standards*, **11**, 359 (1915).

along the inside edge of each strip. This process will give satisfactory results but it is difficult to be quite certain that the contact has been made good enough. The arrangement used by the writer is shown in Fig. 2, which shows one electrode in elevation. The sample, shown shaded, is clamped between two stiff brass bars *A* and *B*, the pressure being applied in the first place by the wing nuts at the ends. Between each of the clamping bars and the insulating material there is a flexible strip of brass about 3 mm. thick, and these strips are pressed into contact with the

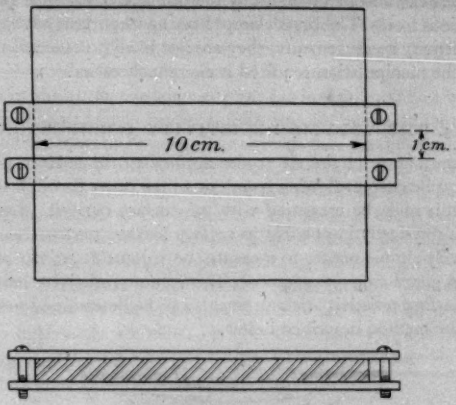

Fig. 1. Brass strip electrodes.

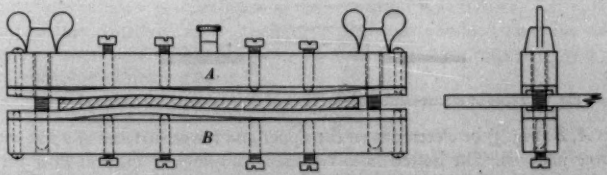

Fig. 2. The new form of electrode.

sample by the set screws shown. The set screws are tightened in turn, until it is seen that the flexible strips, which are the real electrodes, make good contact with the sample over its entire length. A recess is cut in the face of each electrode, as shown in the end view in Fig. 2, so that contact is made not over the whole face of each strip but along the edges only, an uncut rim 1 mm. wide being left for this purpose. This arrangement improves the contact along the edges of the strips, where it is most important, and at the same time reduces the total area of contact, thereby diminishing the correction due to volume leakage. These electrodes are

specially useful for dealing with samples which are neither flat nor of uniform thickness. Fig. 2 is greatly exaggerated in this respect in order to show the principle more clearly. Mere inspection of a sample in these electrodes is sufficient to show that the contact is good over the entire length. The degree of contact was also found to be reproducible; the same sample inserted in the clamps several times gave results differing by about 5 per cent., which was well within the probable experimental error due to temperature change and to the handling of the sample.

Good contact can also be obtained by the use of electrodes of mercury, and of graphite in various forms. The brass clamps have an important advantage over these for surface resistivity measurements; they are less likely to contaminate the surface to be tested. The manipulation required is also much easier.

§ 3. THE VOLUME RESISTIVITY CORRECTION

Whatever the nature of the electrodes applied to the surface of an insulator, there will always be current flowing from one to the other through the body of the material, and this must be measured with the surface current. Separation of the two currents is physically impossible in surface leakage measurement, although it is a comparatively simple matter to measure the volume resistivity alone by use of the well-known guard-ring arrangement. The volume resistivity being known, the correction on surface resistivity measurements can be determined for any system of electrodes by the method described below.

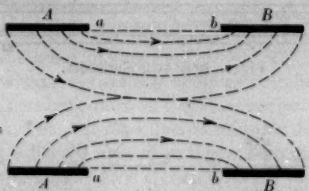

Fig. 3. Lines of electrostatic force or lines of flow of volume leakage current.

Let A, B (Fig. 3) be electrodes on the upper and lower surfaces of a flat sheet of insulating material. The dotted lines represent the lines of current-flow through the body of the material when a p.d. is applied between A and B, and the configuration of these lines determines the "volume resistance" of the arrangement. Now this configuration is the same as that of the lines of electrostatic force which will traverse the corresponding volume if the same electrodes be completely surrounded by a perfect dielectric medium. It follows that, if the volume resistivity be known, it is possible to determine the volume resistance by means of measurements of the capacity of similar electrodes, embedded in a medium of known dielectric constant. The case of the new electrodes described above will be considered as an example.

The standard sample is 10 × 10 cm., and thus the contact areas of the electrodes

(i.e. the effective electrodes) consist of strips 10 cm. long and 1 mm. wide. A model of these electrodes was made by the cementing of strips of tinfoil on to pieces of ebonite 10 × 10 cm. The strips representing the upper and lower parts of the electrodes were placed on separate pieces of ebonite, so that by the placing of a third ebonite piece of any desired thickness between them, a model of the electrode applied to a test piece of this particular thickness was obtained. All the pieces of ebonite measured 10 × 10 cm. and the tinfoil electrodes were always surrounded by ebonite at top and bottom. This is important since the arrangement must be symmetrical with respect to the plane of the electrodes if the correct field distribution is to be obtained. The symmetry ensures that the shortest line of force joining the two electrodes lies in the plane of the electrodes and thus corresponds to a line of flow of the surface leakage current (*ab* in Fig. 3). The electrode-model was now connected to a capacity bridge and its capacity measured for various thicknesses of sample from zero to 11 mm. It is obvious that the difference between the capacity for zero thickness and for, say, 1 cm. thickness, will give the capacity corresponding to the required field distribution in the sample 1 cm. thick. The leads connecting the model to the bridge are the same in every case, and all the external capacities are unaltered by the changing of the sample. Thus the capacity required is easily measured with a probable error of, say, 0·02 $\mu\mu$F.

The method* of measuring small capacities has already been described, but for these measurements no elaborate precaution is needed as all the usual errors cancel out when differences are taken. If C is the capacity in $\mu\mu$F of the required field in a medium of dielectric constant ϵ, then the corresponding volume resistance of a sample of volume resistivity ρ, is given by

$$R = \frac{\epsilon}{3{\cdot}6\pi C}\rho.$$

The most convenient way of expressing the correction is as follows. Let σ_a be the apparent surface resistivity of the sample, i.e. the value obtained when the volume resistivity correction is neglected. Let σ be the true surface resistivity, and ρ the volume resistivity. Then we have

$$\frac{1}{\sigma} = \frac{1}{\sigma_a} - \frac{\alpha}{\rho},$$

where α is the correction factor. This factor has been determined by the above method for the new electrode, for brass strips 1 cm. wide as used by Curtis†, and for the circular system of electrodes now widely used, in which the upper surface of the sample carries two electrodes, a circle surrounded by an annular ring concentric with it and separated from it by a gap across which the surface resistance is measured. The lower surface of the sample in the latter case carries a circular electrode of the same diameter as the upper ring electrode. For the surface resistivity measurement, the upper circle should be raised to a high potential. The ring should be connected to the current-measuring instrument, while the lower electrode should be earth-connected. In this way the bottom electrode acts as a partial shield

* *Proc. Phys. Soc.* **36**, 399 (1924). † *Loc. cit.*

for the ring electrode and reduces the volume-leakage correction. In the determination of the correction for this electrode-system the above conditions were satisfied, i.e. the capacity between the upper circle and the ring was measured, the capacities to the bottom electrode being eliminated by connection of this to earth*. In order to obtain a capacity measurement corresponding to zero sample-thickness in this case, a single sheet of tissue paper (thickness 0·02 mm.) was inserted between the upper and lower electrodes. This was sufficient to insulate these electrodes without separating them by an amount detectable by capacity measurements of this kind.

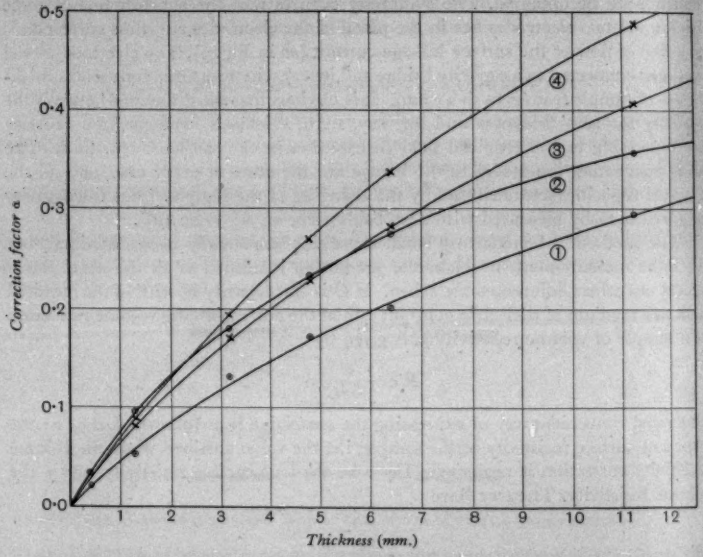

Fig. 4. The correction factor for volume conductance as a function of the thickness of the sample, (1) for the new electrodes 1 cm. apart; (2) for circular electrodes with a gap of 5 mm. and radii of 10, 10·5 and 15 cm.; (3) for plain brass strips, 1 cm. wide and 1 cm. apart; (4) for circular electrodes with a gap of 1 cm. and radii of 10, 11 and 15 cm.

All the results are shown in Fig. 4. It will be observed that the correction is least for the new brass clamps. From a consideration of the calculated capacity between strips infinitely wide on a sample 1 cm. thick, and strips 1 cm. wide on a sample infinitely thick, Curtis concluded that the correction for his brass strips was roughly one-third. The value now obtained is $0{\cdot}36_5$, which confirms Curtis' estimate.

* *Proc. Phys. Soc.* **36**, 399 (1924).

It is evident from the general uniformity of the results in Fig. 4, that the correction for any system of electrodes can be read from this chart with all the accuracy required for such work.

§ 4. RESULTS

An interesting series of results obtained by the use of the new electrodes described above is shown in Fig. 5. They are the outcome of an investigation undertaken by Mr R. S. J. Spilsbury, of the Electrotechnics Department of the National Physical Laboratory, and the writer, in order to obtain data for the design of the terminal fittings of standard instruments. Two samples, 10 × 10 cm., were cut from ¼ in. sheets of each of the following insulating materials: high-grade ebonite (unloaded), bakelite, and keramot. The surface of one sample of each was polished, and

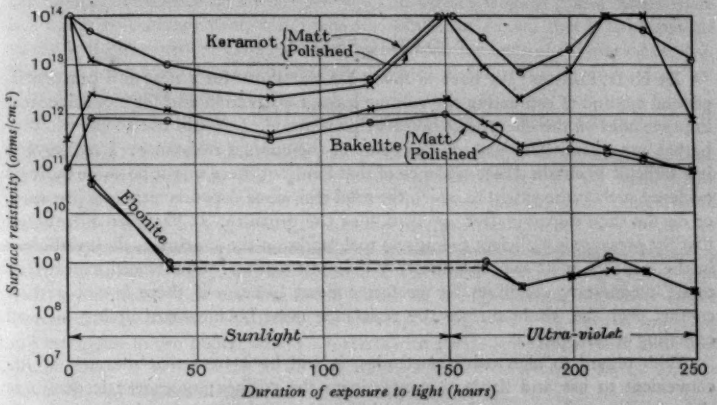

Fig. 5. Surface resistivity of insulating materials exposed to the action of light under ordinary atmospheric conditions.

the other was given the matt finish commonly found on instrument panels. Measurements of leakage resistivity* were made on each sample. They were then left in the ordinary atmosphere of a laboratory for several weeks and exposed to the action of light, at first sunlight (sometimes real, and sometimes artificial) and afterwards ultra-violet light from a mercury arc in quartz. Comparative measurements of leakage resistivity* were made at intervals. The applied p.d. was 200 volts and the current was measured with an ordinary moving coil galvanometer, so that resistivities greater than 10^{14} could not be measured. Initially, the values for ebonite and keramot were higher than this, but the ebonite deteriorated very rapidly. A comparatively slight deterioration of the keramot occurred at first, but it is evident that

* A term, first used by Curtis, for the apparent surface resistivity uncorrected for volume leakage.

from the point of view of instrument design, the leakage resistivities of bakelite and keramot are unaffected by exposure to light, although the colour of the keramot fades. The changes of resistivity shown in Fig. 5 for keramot and bakelite are undoubtedly due to changes in atmospheric humidity. It is interesting to note that there is no very pronounced difference between the polished and matt surfaces of any of the materials. For bakelite the polished surface, i.e. the finish left by the manufacturers, is rather the better, but for keramot the reverse holds good. In the case of ebonite the results could not be distinguished.

When it is remembered that the samples were inserted into the electrodes afresh for each measurement, the general consistency of the results for keramot and bakelite, in spite of fluctuating conditions, gives a good indication of the uniformity of the contact obtained with this type of electrode.

DISCUSSION

Dr H. A. Daynes: We have to thank Dr Hartshorn for a neat and practicable general method of estimating the volume leakage-correction and for a considerable improvement on the old type of brass-strip electrode. I should like to ask whether he has any quantitative data on the value of the contact resistance. I realise that it is difficult to obtain direct evidence of that kind but there might be some indirect evidence such as the extent to which the total resistance depends on screw pressure, or on the time elapsing after application of the pressure. It has been mentioned that the pressure is sufficient to impress tool marks on the specimen. Is it sufficient in the case of a matt sample to destroy the matt surface? One would expect that to be a necessary condition for perfect contact because if there is not perfect contact over the whole surface the resistance must be increased owing to local crowding of flow-lines.

With regard to alternative electrodes, it will be agreed that mercury is inconvenient to use and liable to contaminate the surface; moreover it does not always make good contact. I do not think the same can be said of graphite in all forms. Work has been carried out by the Rubber Research Association for several years on surface-deterioration of ebonite and a few years ago we introduced the use of "aquadag" as a means of applying a conducting electrode. Details will shortly be published in the *Transactions of the Institution of the Rubber Industry*. The colloidal water-suspension of graphite is painted or stencilled on and allowed to dry off. The adverse effect of the water disappears in about 2 hours. In our experience such an electrode is free from objection.

It may be mentioned that serious deterioration under the influence of sunlight does not take place with all qualities of ebonite. It is most marked with the purest qualities such as are required for very high dielectric strength and low dielectric loss at high frequencies. Where exposure to sunlight is inevitable, a better all-round performance can often be obtained by the use of an ebonite containing mineral and other ingredients, the trade product mentioned in the paper being one example of such compounded ebonite.

Dr L. SIMONS: In view of the fact that most rolled or cast materials have an effective skin in which the physical properties vary progressively with the depth, is the method adopted of correcting for the volume resistivity completely independent of such conditions? For example the capacity of the electrodes would be different when they are surrounded by a perfect insulator from what it is when the samples are sandwiched between dielectrics with surfaces exactly similar to those of the samples.

Dr J. H. VINCENT: I congratulate the author on this important piece of work. Do the graphs in Fig. 5 indicate that keramot deteriorates at first and then recovers its high resistance?

Mr D. K. MCCLEERY: I want to draw attention to the strong influence of atmospheric humidity on surface leakage. In certain tropical places where there is a damp and steamy heat, conditions are sometimes so bad that it is impossible to test without guard wires (I am referring to insulation tests of submarine cables). When guard wires cannot be used, a coating of paraffin wax on the surface has been found to give good results.

I should like to suggest the term "creepance" for the surface conductance.

Mr D. A. OLIVER: I should like to express my admiration of the elegant method devised for determining the volume resistivity correction. It has enabled the measurement of the true surface resistivity to be placed entirely on an experimental basis. The results in Fig. 5 are of considerable practical interest, and bring out clearly that keramot—known already to possess good machining properties, a relatively small power factor and a coefficient of linear expansion not greater than ordinary ebonite—has the additional virtue of a high surface resistivity under ordinary atmospheric conditions. As corresponding values of volume resistivity are not given in the paper, would Dr Hartshorn give, for a typical case, some idea of the order of error likely to be introduced into a measurement of true surface resistivity by neglect of the volume resistivity correction?

Prof. C. L. FORTESCUE: Both physicists and electrical engineers are very much indebted to Dr Hartshorn for this work, because though the remarkable deterioration of ebonite and some other materials with exposure to light has been known for fifty or more years, no very exact measurements have been available. Now that these methods have been developed it is to be hoped that careful tests will be made of all the other materials available in place of ebonite, and that the results will be published.

AUTHOR'S reply: I have no quantitative data on the actual contact resistance between the metal and dielectric surfaces, but the indirect evidence available is to my mind sufficient to show that such contact resistance is negligible in measurements made in the manner described in the paper. The applied mechanical pressure could of course be varied considerably by adjustment of the set screws, but once the proper contact was made, a further increase of pressure had no appreciable effect on the leakage current. Further, it will be observed that matt and polished

surfaces of the same materials gave practically the same results in most cases. Thus even with matt surfaces the intimacy of contact was adequate. The results shown in Fig. 4 are evidence of the fact that perfect intimacy of contact, such as would allow for microscopic surface irregularities, is not necessary. For curve (3) the area of electrode contact was five times that for curve (1), whereas the volume leakage current is only greater by about 30 per cent. Thus with the degree of contact obtainable with the new electrodes, one could hardly hope to detect any increase of resistance due to local crowding of the lines of current flow at the contact surfaces.

The mechanical pressure was sufficient to impress tool marks on a polished ebonite surface, but not to destroy a matt surface. A polished ebonite surface takes an impression very easily, and was therefore used to test the uniformity of the contact along the length of the electrode.

I have used the graphite electrode described by Dr Daynes and believe it to be quite satisfactory for ebonite and similar materials. It does however demand careful manipulation. For testing new and unknown materials I prefer the brass electrode, because although the effect of applying water to an ebonite surface may disappear in two hours it does not follow that this would happen for all materials.

The volume leakage correction cannot be applied with accuracy to non-homogeneous materials of the kind described by Dr Simons. In fact such materials cannot be said to have a definite 'volume resistivity,' and any separation of the volume and surface leakage currents is purely arbitrary in such cases.

The apparent improvement noted by Dr Vincent in Fig. 5, is simply due to a diminution in atmospheric humidity.

The volume leakage correction is usually of importance only when measurements are made with a relative humidity less than 20 per cent. Curtis* has given data illustrating this point, and the following values of surface resistivity at low humidities are selected from his paper.

Bakelite,	uncorrected	10×10^{12},	corrected	30×10^{12}
Stabalite,	,,	5×10^{13},	,,	9×10^{13}
Amberite	,,	2×10^{15},	,,	2×10^{15}

* *Bulletin of Bureau of Standards*, **11**, 415 (1915).

DIAMAGNETISM AND MOLECULAR STRUCTURE

Lecture delivered on November 8, 1929, by*
SIR C. V. RAMAN, F.R.S.

PHYSICISTS at the present day do not need to be reminded of the great importance of the subject of magnetism considered from the standpoint of chemistry, i.e. in relation to molecular structure. The subject of diamagnetism in relation to molecular structure, however, has not been so much to the fore as that of paramagnetism. At Calcutta we have approached the subject from a rather unusual standpoint in connexion with our investigations on the scattering of light. We were led to discuss the question how far it was possible to obtain a quantitative expression for the magnetic double refraction observed in liquids. I will remind you of a discovery in about 1912 by two French physicists—Cotton and Mouton†—who found that if they placed an organic liquid in a strong magnetic field H, Fig. 1, and observed the effect of the liquid on a beam of light L, passing through it transversely to the lines of magnetic force, certain liquids such as nitro-benzene showed a feeble double refraction, so feeble that it was at first only in very special classes of organic compounds, viz. benzene and its derivatives, that it was possible to observe it at all. Later on, improved methods of observation have extended very considerably the list of organic liquids in which this magnetic double refraction can be observed. These two French physicists also succeeded in detecting it in a very small number of aliphatic compounds, such as chloroform, bromoform, etc. and in a great many common liquids such as water and paraffins and the ordinary aliphatic hydrocarbons, ethers, alcohols, acids, etc. With some of the acids they were not able to observe the effect, and thus the great importance of the subject did not receive adequate attention.

Magnetic double refraction is exhibited by certain substances owing to their molecules possessing a magnetic susceptibility. That, of course, is obvious, but what is not quite so obvious is how exactly the magnetic double refraction arises.

About the time of the discovery by Cotton and Mouton there was already available certain work by Voigt‡, who showed that, as a consequence of the dispersion theory, if a beam of light passes through a substance placed in a transverse magnetic field, there must be a feeble double refraction produced; and this refraction was shown to be related to the Zeeman effect. You will find some reference to Voigt's work in his well-known book on magneto-optics published by Methuen.

* Completed and revised, in the absence of the author, by J. S. G. Thomas, D.Sc.
† A. Cotton and H. Mouton, *Comptes Rendus*, **141**, 317–319 and 349–351 (1905).
‡ W. Voigt, *Gesell. Wiss. Göttingen Nachr. Math.-Phys. Klasse*, **2**, 215–220 (1913).

When Cotton and Mouton made their observations on magnetic double refraction in liquids they found that the actual effect observed by them was nearly 1000 times larger than what would be roughly estimated from Voigt's theory of the phenomenon. The consequence was that they realised that an entirely different explanation must be sought. Prof. Langevin was the first to suggest what is now fairly generally accepted as the real explanation of the effect*. I would like to make the thing clear by a little diagram, Fig. 1. If the incident light is plane-polarised and, in the absence of the magnetic field, is completely cut off by an analyser, suitably arranged, then we find that as soon as a transverse magnetic field is established there is a feeble restoration of light. This restoration is not a matter

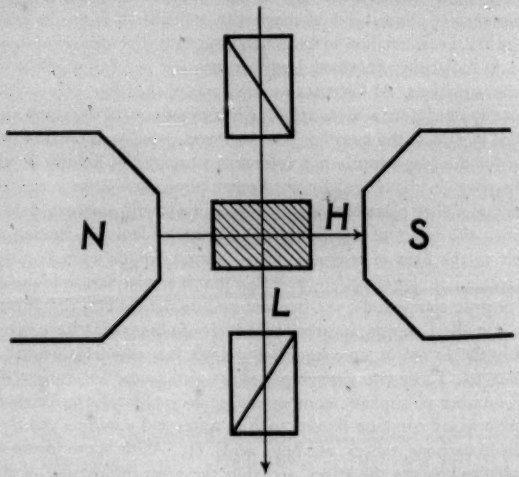

Fig. 1

of the ordinary Faraday effect at all, as here we are dealing with light that is passed through a *transverse* magnetic field. The light has become elliptically polarised, and the ellipticity can be measured by suitable experimental methods. The idea, however, with which Langevin started was that the molecules of the liquid possess magnetic anisotropy. Langevin's theory has been developed by Born, who assumes that the molecules possess permanent magnetic moments and that in the condition of statistical equilibrium of the molecules in the magnetic field a small percentage of the molecules becomes preferentially oriented. The magnetic field only acts on the molecule if the latter has a permanent magnetic moment whose axis is fixed so that the field exerts a couple on it. If the molecule is magnetically non-isotropic, i.e. if it is not equivalent to a magnetic sphere in its behaviour, the magnetic moment

* See also C. V. Raman and K. S. Krishnan, *Proc. R. S.* A, 117, 1 (1927).

has in general a direction inclined to the lines of force and, in consequence, a couple acts on the molecule. In every case the tendency of this couple is to orient the molecule so that the magnetic susceptibility of the system tends to a minimum value. This orientation, however, is unable to exert its full effect.

When dealing with a liquid in which the molecules are in a state of continual thermal agitation we have a kind of statistical equilibrium and the magnetic field tends to orient the molecules. We do not have the molecules oriented at random in all directions but there is a certain preferential orientation of the molecules in that direction in which their potential energy in the field of force is a minimum. This statistical equilibrium can be evaluated, by a well-known method, and if we postulate further that the molecules are not optically isotropic, i.e. that the electrical moments induced in the molecules by the electric field in the light wave are not always parallel to the field, we have as a consequence which, I think, is fairly obvious, that the liquid exhibits a feeble double refraction and acquires some of the characteristics of a crystal. This effect is, however, extraordinarily feeble; in fact, I doubt very much whether, except perhaps in one or two laboratories in the world, the phenomenon has been observed in the course of the ordinary work of the physicist.

I myself have observed it with a small electromagnet and when on one occasion I got the effect in a few minutes I was very jubilant, but in half an hour I discovered that I was observing a small residual Faraday effect, and not magnetic double refraction at all. The position is that the observation is extremely difficult.

Now, obviously, while Langevin's theory gives us a general account of the phenomenon, its real significance is that if we knew the magnetic characteristics and also the optical characteristics of the molecule we could calculate the double refraction and see if this agreed with observation. On the other hand, if we knew the actual magnitude of the double refraction and the optical characteristics of the molecule we could go back and infer from them the magnetic characteristics of the molecule, or at least the degree of magnetic anisotropy of the molecule. That is the importance of double refraction from the standpoint of the theory of magnetism generally.

Perhaps I can make the importance of this clearer by referring to the work of Pascal who some years ago paid a great deal of attention to the examination of the magnetic properties of a great variety of compounds, both organic and inorganic, and attempted to find some general relationship between them. Pascal's work led to the belief that there was nothing startling about diamagnetism and that you could, in general, determine the diamagnetic susceptibility of an organic compound simply by adding up certain contributions from the separate atoms of which the molecule is composed, that is to say, diamagnetic susceptibility is an additive molecular property. In a few cases there were certain residual differences, but later on Pascal made further observations, though I think they are relatively of minor importance from the quantitative point of view. Nevertheless, Pascal emphasised that they constituted an influence of diamagnetism that was not altogether negligible. Pascal's work was concerned largely with liquid or crystalline bodies and in con-

sequence what he was actually measuring was the average susceptibility of molecules oriented in all possible directions relative to the lines of magnetic force. The average susceptibility so measured does not really tell us very much about individual molecules. The results of X-ray work and the fundamental conceptions of chemistry suggest that the molecules are not even approximately isotropic. This being so, if we really wish to understand the architecture of the molecule and determine its magnetic properties we have to study the diamagnetic properties of single molecules, and unless we have such information we cannot really even begin to understand how the problems of structural chemistry are related to the fundamental properties of diamagnetism.

We have, therefore, to seek for some method of investigating the diamagnetic character of molecules. We have dealt hitherto with liquids but when we pass from liquids to crystals we have a complication introduced by the fact that the actual structure of the crystal is not known in many cases; in fact, in most cases it is not known at all. Therefore the work on liquids tends in the direction of simplicity, and it is of importance to try and interpret magnetic double refraction in a quantitative manner and then to deduce the ultimate qualities of the molecules themselves.

This has been the fundamental idea with which we set out. There was, however, a very serious limitation in that the work of Cotton and Mouton was practically confined to certain specified classes of organic compounds, namely the aliphatic compounds, whilst in regard to a great many inorganic compounds they do not, for some reason, appear to have been able to get a measurement or even to observe the effect.

One of the first things we did, therefore, was to try and improve the technique of the subject in such a manner as to bring all known substances within the scope of the research. We felt quite certain that the failure of Cotton and Mouton to observe magnetic double refraction in water or one of the common paraffins was due to the insufficient sensitiveness of their apparatus. Moreover we knew from optical work on the scattering of light that there is no reason to suppose, if a molecule is electrically isotropic, that it must be magnetically isotropic as well. On the other hand, we know that the electrical and magnetic behaviour are counterparts of each other and that there must be correlation between these aspects. Therefore we felt that if the technique of Cotton and Mouton were improved it should be possible to study double refraction in all substances in the liquid state. The problem is twofold. First there must be a sufficiently intense magnetic field, and secondly there must be sufficiently delicate methods of observation. It is obvious that in order to increase the sensitiveness of the method of observation the liquid must be placed in an *intense* magnetic field.

Not possessing the financial resources which seem to lie so close to hand in the case of many American physicists, and perhaps also to some physicists in this country, I wandered about the streets of Calcutta and looked into the old iron shops and discovered a huge dynamo of the Edison type, which had probably figured in some nobleman's house but had been thrown away in favour of some newer model. This dynamo happened to be in perfect order and we got it for the

price of the old copper it contained. The old Edison dynamos are enormous and the marked feature of the electromagnet was its great length of 35 cm., while the gap was very small, only about 1 cm.[2] We found that with a current of about 10 amp. we had a field of 25,000 gauss. Using the electromagnet we were able to observe magnetic double refraction in practically all the substances we examined. In order to obtain quantitative results we have had also to develop more sensitive methods of measurement and for this purpose we employed a very interesting device, due to the late Lord Rayleigh*, which proved to be of great service. The set up of the apparatus is given in Fig. 2, which shows diagrammatically the apparatus employed by one of my students, M. Ramanadham†.

S is a "Pointolite" source of light, L a converging lens illuminating the slit of the collimator C. N_1 and N_2 are respectively the polarising and analysing Nicols. P_1, P_2 are the poles of the electromagnet and T the observing telescope. The glass strips M and N form the compensator. A piece of flint glass, M, sup-

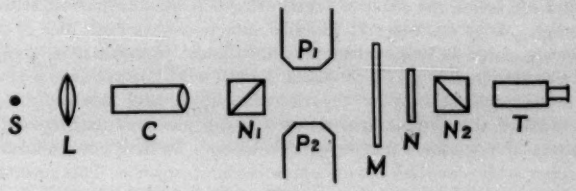

Fig. 2

ported at its ends, is bent in a vertical plane by a small adjustable force applied at its centre. The glass thus strained is interposed between the crossed Nicol prisms. The double refraction induced in the glass plate by the applied stress manifests itself by the restoration of light everywhere in the field of the telescope T except along a horizontal dark band corresponding to the neutral axis of the plate M. If any doubly refracting medium is interposed between the strained glass and the polariser N_1 a shift of the dark band occurs and can be measured with reference to the cross wire of the observing telescope. In order that both kinds of double refraction may be determined quantitatively this shift is compensated by the interposition of a second glass strip N which can be compressed either horizontally (for positive birefringence) or vertically (for negative birefringence). The plate N is connected with a pan in which weights can be placed so that the applied stress necessary to compensate the shift can be measured.

In this manner Mr Ramanadham has been able to measure the double refraction occurring in a great many common substances, including something like 35 compounds in which Cotton and Mouton failed to observe the effect, and in which it is exceedingly small. There is a large difference between the aliphatic compounds and the aromatic compounds and I have some figures here relating thereto. If we

* *Phil. Mag.* **4**, 678 (1902).

† M. Ramanadham, "Magnetic birefringence in liquids," *Indian Journal of Physics*, **4**, 15 (1929).

take the double refraction observed with nitrobenzene as 100, the figure for water is only 0·4, and that for ethyl alcohol is of the same order. There is a very remarkable difference referring not only to the magnitude but also to the signs of the double refraction and their relation to chemical constitution. A characteristic feature of the effect as observed by Cotton and Mouton is that all compounds of the benzene series—naphthalene and similar aromatic compounds—show a positive birefringence, whilst the aliphatic compounds show a negative birefringence. In other words, aromatic compounds behave like positive uniaxal crystals, aliphatic compounds like negative uniaxal crystals. The chemist has divided organic compounds into the aliphatic and aromatic classes and the distinction, which is fundamental in chemistry, is reflected magnetically in the fact that one class gives a positive and the other class a negative birefringence. Only, however, by very refined arrangements such as I have outlined is it possible to observe and measure this characteristic difference.

In aliphatic series, the effect of substitution is to produce considerable change in the magnitude of the magnetic double refraction observed. This is shown in the following series of relative experimental values: [nitrobenzene, 100]; formic acid, + 2·5; acetic acid, + 1·1; propionic acid, + 1·1; butyric acid, 0·7; capronic acid, + 0·7; heptylic acid, + 0·5; caprylic acid, 0. We have obtained these results and you will see that formic acid shows a strong positive birefringence. I have no doubt that if we proceed further with the series—we have not yet studied some of the higher acids—we shall obtain a negative birefringence. This reversal of the sign of the birefringence is analogous to the reversal of the long chains of fatty acids observed by Müller by X-ray methods. We have shown that a negative birefringence is characteristic of the aliphatic compounds, and the same sort of influence is shown in the ketones and as we proceed up the series we obtain a negative birefringence. On the other hand there is evidence that double compounds have a positive birefringence.

I have given you a few illustrations, but do not run away with the idea that the method is only applicable to organic compounds. It is just as possible to investigate inorganic compounds. In this case, of course, we have only to use solutions of such compounds. Results with some of the nitrates, such as sodium nitrate and nitrites, show that any kind of unsaturation increases the positive birefringence and decrease the negative birefringence. In all this I am merely indicating the field of work that has been opened up by the extending of the work of Cotton and Mouton and the making of their method of investigation applicable to a great variety of substances.

Now I come to the question of the interpretation and use we make of these results. It is not sufficient to say that there occurs positive or a negative birefringence. We must try to deduce the magnetic characteristics and here we are face to face with considerable difficulties. Langevin considered the case of a molecule with an axis of symmetry, and the mathematics becomes much simpler in such a case. We, however, in the general case, have to assume that the magnetic ellipsoid and the optical ellipsoid of the molecule have unequal axes and that these

axes may have no obvious relationship to each other. You will find an account of Langevin's theory given in Prof. Born's monograph, *Handbuch der Radiologie*. This theory of Langevin, complicated as it may seem, is inadequate because he has assumed that the polarisation field, i.e. the optical field acting on the molecule, is of an isotropic character.

When we have a beam of light passing through a liquid, the actual electromagnetic field acting on the light wave is due to two causes; one, the external field of the light wave itself, and the other, the optical polarisation of the molecules in the immediate neighbourhood. Langevin and Voigt used the well-known theory of refraction in this matter, but our investigations show that Langevin's theory is quite inadequate, even as an approximation, and when we come to deal with such recondite phenomena as double refraction it is wholely inadequate. Molecules are not spherical in form. They are highly anisotropic. Much depends on how the molecule is oriented in the light wave. Mr Krishnan and myself* have developed a general theory of optical and electrical properties of a liquid and we have been able to show that the influence of the shape of the molecule is, in consequence of its environment, effectually diminished in a manner that can be experimentally determined. The reason I have had to speak about optical anisotropy is that magnetic birefringence depends on *both* optical and magnetic anisotropy.

The value of C_m, the Cotton-Mouton constant†, which is a measure of the optical birefringence exhibited by a liquid in a strong magnetic field is given by the equation

$$\begin{aligned} C_m &= (n_p - n_s)/\lambda H^2 \\ &= -3(n_0^2 - 1)^2 [(A - B)(A' - B') + (B - C)(B' - C') \\ &\qquad + (C - A)(C' - A')]/8\pi n_0 \lambda \kappa T v (A + B + C)^2, \end{aligned}$$

where A, B and C are the moments induced by unit electric force in the incident light wave acting along the three mutually perpendicular axes of the optical ellipsoid of the molecule, and A', B', C' are the magnetic moments for unit magnetic fields acting in the same directions. The remaining symbols have their customary significance. It is clear that the sign of C_m depends on that of the factor

$$\Sigma (A' - B)(A' - B').$$

If the optical moments A, B, C are in descending order of magnitude and if A', B', C' are in the reverse order, then $\Sigma (A - B)(A' - B')$ is positive and the value of C_m is positive. But if A, B, C and A', B', C' are in the same order, either descending or ascending, then the value of C_m is negative. The quantities A, B, C are connected with the refractive index, n, by the relation

$$(A + B + C)/3 = 3(n_0^2 - 1)/4\pi v (n_0^2 + 2),$$

so that we have

$$C_m = -(n_0^2 - 1)(n_0^2 + 2)[\Sigma (A - B)(A' - B')]/60 n_0 \lambda \kappa T [\Sigma A].$$

* *Loc. cit.*

† S. Bhagavantam, *Indian Journal of Physics*, **4**, 1 (1929).

If $A = B$, and $A' = B'$, we have

$$C_m = -(n_0^2 - 1)(n_0^2 + 2)(A - C)[3C' - (A' + B' + C')]/60n_0\lambda\kappa T(2A + C)$$

also, if δ be the optical anisotropy, then

$$\delta = (A^2 + B^2 + C^2 - AB - BC - CA)/(A + B + C)^2.$$

It is because the molecule is both optically and magnetically anisotropic that we get double refraction. If we knew the quantities A, B, C and A', B', C' it would be possible to compute the Cotton and Mouton constant absolutely and see if it agrees with observation. I think the principal feature of the theory that Mr Krishnan and I have worked out is the determination of A, B, C and if we could get A', B' and C' we should get a value for the Cotton and Mouton constant. But we do not know A', B' and C'. What we want to do is to know A, B and C and to calculate A', B' and C'. If we assume the molecule to have an axis of symmetry the method becomes a perfectly practicable one. This assumption is definitely permissible in a number of cases. Take carbon bisulphide or benzene. In these cases the assumption of an axis of symmetry is justified and we can go ahead and compute the magnetic characteristics. This was done some time ago* and the remarkable fact was discovered that in the case of benzene the magnetic susceptibility is nearly twice as large in a direction normal to the plane of the ring as in a direction parallel to the ring. In the case of anthracene the magnetic susceptibility in a direction normal to the plane of the benzene ring was four and a half times that in a direction parallel to the ring. This is sufficiently remarkable but I should mention that these compounds show positive and negative birefringence and I come to the simple conclusion that in the case of certain compounds, as for example benzene, maximum diamagnetic susceptibility coincides in direction with minimum optical polarisation-susceptibility. We have here an effect deduced from observation which awaits interpretation, and there is no doubt that it is extremely significant from the point of view of molecular structure.

If I stopped here, as I ought from considerations of time, I should leave the whole subject in an extremely unsatisfactory state. You might very well say that I have discussed double refraction and spoken of light-scattering, but that you know nothing at all about magnetic anisotropy, and yet you are asked to accept the fact of this double refraction, and that this places rather a heavy strain on your credulity.

Fortunately, however, the most interesting things that have come out of our work are conclusions drawn from wholly unrelated fields of investigation. From our studies on light-scattering and other investigations of that kind we can compute the magnetic characteristics of the molecule. This matter is discussed in a paper to be published shortly. If we take, for example, hexamethyl benzene we find that it is a remarkable substance. Lonsdale has analysed its crystal structure, and as soon as we saw his results we came to the conclusion that it should be possible for us, without much difficulty, to determine the magnetic characteristics of this crystal

* See e.g. "Magnetic birefringence in liquids of the aliphatic series," by M. Ramanadham, *Indian Journal of Physics*, **4**, 15 (1929); and "The anisotropy of the polarisation field in liquids," *ibid.* p. 39.

from our investigation of light-scattering, together with our studies of its magnetic birefringence. We proceeded to fix the magnetic axes of the crystal and to find out what should be the magnetic susceptibility normal to the plane of the ring, and also in a direction parallel to it, and the observations showed themselves to be in complete agreement with the results referred to. Hexamethyl benzene, in a direction at right angles to the plane of the benzene ring, behaves as an aromatic compound, whilst in the plane of the ring its behaviour is typically aliphatic and attributable to the CH_3 groups.

We appear to have developed a new technique for studying the magnetic characteristics of crystals. This is shown diagrammatically in Fig. 3. The apparatus is based essentially upon the gravity balance of Threlfall and Pollock* and was used by S. Bhagavantam†. A fine quartz fibre is stretched across the poles of the electromagnet *NS* as shown and is kept taut by two screws. A glass fibre carrying

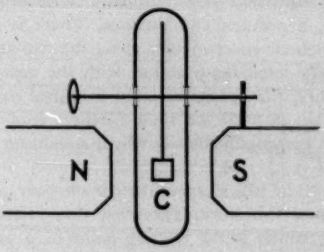

Fig. 3

the crystal, *C*, between the poles of the electromagnet is attached to the quartz fibre as shown, so that the crystal hangs in the non-homogeneous field of the electromagnet. As the quartz fibre is stretched across the gradient parallel to the field, it at once responds to forces acting on the crystal in the direction of this gradient and the resultant twist of the quartz fibre results in an angular displacement of the end of the glass fibre which is measured by a microscope.

I have taken up a large amount of time and I cannot describe to you in any detail all the various kinds of instruments we have developed for our investigations. This new technique, however, has been applied to a very large number of organic crystals, so that we now have a means for direct quantitative observation of the magnetic characteristics of organic crystals, especially naphthalene, anthracene, hexamethyl benzene and a number of others. I want you to realise that here we have the beginning of a practically limitless field of research. The organic chemist manufactures and the physicist designs for our benefit thousands and thousands of crystals, all of which await a detailed study of their magnetic construction. This field of work might perhaps at one time have been considered too unimportant because the effects exhibited were so small, but I hope you will realise from what

* *Phil. Trans.* A, 193, 215 (1899). † *Loc. cit.*

I have been telling you that the magnetic anisotropy of an organic crystal is a remarkable phenomenon, the diamagnetic susceptibility being four or five times greater in one direction than in another. Important results must consequently follow from an extended study of the diamagnetic behaviour of organic crystals. I would be failing in my duty if I did not emphasise to you that this new technique enables us to go much farther than has been possible by older methods in the matter of crystal structure. X-ray analysis is a most powerful method of research and I would be the last to decry it because I use it myself, but there are limitations to the X-ray method, and the magnetic study of crystals has revealed properties which are at present beyond the reach of X-ray examination. I will take as an illustration the case of hexamethyl benzene. One would assume that it has axes of symmetry and that the magnetic susceptibility is the same in all directions and that likewise the optical susceptibility is the same in all directions. In one respect, as stated already, this substance is an aromatic compound but in another it is aliphatic in character, a peculiar combination. There is a difference of about 10 per cent. in the magnetic susceptibility along the two axes of the plane of the ring and these two axes coincide absolutely with the optical axes in that plane. Here we have, therefore, the optical and the magnetic susceptibilities following each other closely. That is what we should expect from the crystal structure. I cannot understand at present chemically why it should be asymmetrical, but it *is* asymmetrical.

I will not stop here but will trespass further on your patience and indicate a still further possibility of this work. Take that very interesting and remarkable substance azobenzene, which is the starting-point of a great industry, viz. the manufacture of dyes. Azobenzene is composed of two benzene rings bridged together by an azo group. This substance has been examined and it has been found that the crystal does not show that high degree of magnetic anisotropy which we should have expected from the existence of two benzene rings presumably in the same plane; at least this is the structure the chemists provide us with. There must be something in the crystal structure which masks the magnetic anisotropy; in other words, there must be more than one molecule in the lattice and these molecules must be so oriented relatively to each other that in the crystal itself there occurs a masking of the anticipated magnetic anisotropy. One can predict that the two molecules in the crystal lattice cannot be oriented parallel to each other. Similarly, I fear that in the case of naphthalene and benzene, the orientation which Sir William Bragg, in his book published some years ago, assigned as probable, cannot now be accepted, because it does not take into account the optical and magnetic aspects and there is evidence that more recent X-ray results will lead to a revision of their structures so as to fit in with the optical and magnetic characteristics. Optical and magnetic characteristics never mislead—X-ray results may sometimes mislead.

You see now that starting from the molecule we have entered into the crystal region and provided we have enough data in these two fields of research, magnetic study can be regarded as a most powerful auxiliary to crystal analysis. That is the point I am trying to make.

I should like to mention another very remarkable new field of research in relation to diamagnetism which we have entered upon. For a long time it has been part of our scheme of research on magnetism in Calcutta to investigate how the physical state of a substance influences the magnetic characteristics. Suggestion have been made that the diamagnetism of certain compounds is different in the liquid state from what it is in the solid state. Mr V. I. Vaidyanathan* started a research some time ago in Calcutta to develop a complete technique by which the feeble diamagnetism of vapours and gases could be accurately measured. The method is an extremely simple one; it was first used by Faraday and was modified by Glaser and by Hammar.

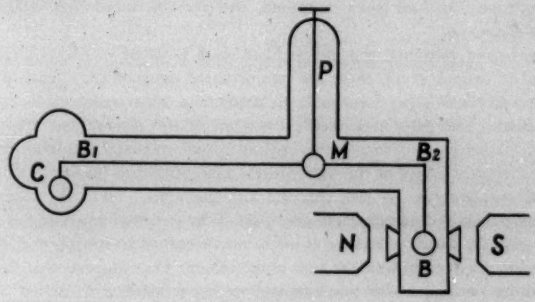

Fig. 4

A thin aluminium vane B_1B_2, Fig. 4, carrying a concave mirror, M, is suspended by a phosphor bronze strip within a vessel of the form shown, which can be filled with any gas or vapour. One end of the vane carries the evacuated glass bulb, B, whilst the other end carries a counterpoising bulb C. B is suspended in the magnetic field of the electromagnet NS.

The glass of the bulb B is specially chosen so as to have the smallest possible magnetic susceptibility. It is highly paramagnetic, so that magnetically this glass bulb is practically neutral. By suspending it inside the cylinder containing the gas or vapour—a magnetic analogue of the principle of Archimedes—we can determine the magnetic susceptibility of the gas or vapour. One of the firstfruits of the method which we devised was connected with results obtained by Glaser, who investigated CO_2 and one or two other inorganic gases and came to the conclusion that the susceptibility of a gas is not proportional to the pressure. Glaser laid it down that at low pressures, instead of the magnetic susceptibility decreasing proportionally to the pressure, the susceptibility pressure curve departed from this linear relationship. That result caused a great deal of sensation and got into the text-books, which ought not to have happened. As a matter of fact, I saw this paper of Glaser's on a Saturday afternoon and on the following Tuesday evening I gave an Address, proving to my own satisfaction, if not to the satisfaction of my audience, that Glaser's work must be wrong and his result attributable to faulty technique.

* *Indian Journal of Physics*, 1, 183 (1926).

Glaser explains the result as due to some orientation of the molecules in the magnetic field. If such an orientation exists, we know from our work on double refraction that the effect of orientation is exceedingly small. The Cotton and Mouton constant contains the factor 10^{-14}, so that you will realise at once what an exceedingly small effect it is. In view of that, Glaser's statement that the molecules are orientated is palpably wrong. The subsequent work of Glaser I believe to be fallacious, but that was the starting-point, and one object which led us to develop this new technique of placing the vacuum bulb in a heterogeneous magnetic field was the investigation of his conclusion*. We have found throughout strict proportionality between pressure and the value of susceptibility for carbon dioxide, argon, hydrogen, and all gases examined, and are convinced that Glaser's conclusion is erroneous.

I wish to say, however, that this field of work must be extended largely. We have tried to extend it and there are two or three cases of very great interest in which the physical state does seem to produce a most remarkable change in diamagnetism. The most remarkable cases are those of graphite, charcoal and diamond. Pascal worked with many organic compounds and also with the above forms of carbon. Values of the diamagnetic susceptibilities (in 10^{-6} units) found by Pascal are: graphite, $-4{\cdot}0$; charcoal and diamond, $-0{\cdot}5$. In other words, magnetically diamond and charcoal are alike. Why is it that graphite has a higher value? Graphite stands out alone of all forms of carbon as having a diamagnetic susceptibility about eight times as high as the others. That suggests that there must be something peculiar about graphite and we set to work and carried out some experiments. We have found that the susceptibility of massive graphite is still higher; the average value is something like $-7{\cdot}0 \times 10^{-6}$. Graphite is magnetically anisotropic, the diamagnetic susceptibility normal to the plane of its structural ring (in 10^{-6} units) being -18, whilst in the plane of the ring the value is -2. We find with graphite that the value can be brought down to the value for sugar charcoal by powdering of the graphite; in other words, particle size determines the magnetic susceptibility of graphite. But the powdering must be extremely fine to achieve the result. The same sort of thing is found in the case of bismuth and antimony, and we now know that particle size has an influence on the diamagnetic property and there is obviously a relationship between the fact that the susceptibility is greater in the direction normal to the plane of the ring†.

I have tried to give you some idea of the kind of work that is being attempted in Calcutta. We have only just started these investigations and they have to be pursued and developed. We have obtained some results but we are yet far from being able to picture them and explain them in terms of the newer theories. Nevertheless, I think that what I have said will indicate that there is still plenty of room for experimental work in modern physics.

* Glaser, *Ann. der Physik*, **74**, 459 (1924); Hammar, *Proc. N. A. S.* **12**, 594 (1926); Vaidyanathan, *Indian Journal of Physics*, **1**, 183 (1926), **2**, 135 (1928).

† See S. Paramasivan, "Anomalous diamagnetism of graphite," *Indian Journal of Physics*, **4**, 141 (1929).

SOME NOTES ON WIRELESS METHODS OF INVESTIGATING THE ELECTRICAL STRUCTURE OF THE UPPER ATMOSPHERE (II)

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ABSTRACT. The comparative discussion of the various direct wireless methods of measuring the "equivalent" height of the atmospheric ionized region, begun in the first part of this paper, is continued so as to include the recently described moving-transmitter method of Mirick and Hentschel.

The relations between the optical and equivalent paths of waves deviated by the upper atmosphere and between the rates at which these quantities may vary with time are investigated theoretically, and, from the results of experiments carried out to test these relations, deductions are made concerning (*a*) the existence of more than one ionized region in the upper atmosphere, (*b*) the possible influence of magnetic storms on atmospheric ionization, (*c*) the gradient of ionization in the upper atmosphere and its alteration under solar influence at sunrise, and (*d*) the actual height reached by waves deviated in the upper atmosphere.

§ 1. INTRODUCTION

In a previous paper* of similar title a comparison was made of the various direct wireless methods that are now available for measuring the "effective height" of the atmospheric ionized layer and it was there shown that, for a layer of horizontal stratification and for conditions under which the earth's magnetic field may be neglected, the same quantity, namely the equivalent path of the atmospheric waves, was measured by all methods. The effective height of the layer, which may be deduced from the equivalent atmospheric wave path thus found, was shown to be greater than the actual maximum height reached by the waves in the process of atmospheric deviation.

The present paper continues in three directions the discussion begun in part I. In the first place it brings up to date the general enquiry into the significance to be attached to the term "effective height" of the ionized layer, by the inclusion of a discussion of a new wireless method of height determination recently described by Mirick and Hentschel†. Secondly, it presents a discussion of the relations which exist between the equivalent and optical paths of the atmospheric waves and the theoretical significance of the variations of these quantities with time and with the frequency of the sending station. The discussion of experimental data which then follows consists in part of an enquiry as to how far the theoretical

* *Proc. Phys. Soc.* **41**, 43 (1928). For convenience this paper will throughout be referred to as "part I."

† *Proc. Inst. Rad. Eng.* **17**, 1034 (1929).

conclusions of this and the previous paper fit in with the experimental results, and in part of deductions which can be made from the data as to the actual electrical structure of the upper atmosphere.

§ 2. THE MOVING-TRANSMITTER METHOD OF HEIGHT DETERMINATION

In the course of observations made on the intensity of signals emitted by an aeroplane in flight, Mirick and Hentschel, of the United States Naval Research Laboratory, noticed periodic variations of signal strength at a rate which remained fairly constant over a considerable time interval; and, by assuming that such signal variations were due to interference between direct waves and indirect waves which had been reflected by the upper atmosphere, they were able to deduce from their observations the effective height of the Kennelly-Heaviside layer. For the purpose of their calculation Mirick and Hentschel examined the imaginary case of a fixed emitting source and a moving observer, the latter travelling towards or away from the sender so as to pass through the maxima and minima of the interference system formed by the direct and indirect trains of waves. They then assumed that the same results would apply if the relative velocity between the source and the observer were due to the motion of the source, the observer being at rest. From the rather involved formula resulting from such a calculation it is not easy to see exactly what significance should be attached to the term "effective height" in this case, and the following alternative way of regarding the problem is therefore offered.

Let us consider the aeroplane sending station as travelling towards the observer with a velocity v. If the frequency of the waves emitted by this station when at rest is f_0 the frequency of the waves travelling towards the observer will, by Doppler's principle, be f_1, where

$$f_1/f_0 = c/(c - v) \qquad \text{.....(1),}$$

c being the velocity of electric waves in free space. Let us now suppose that the atmospheric waves which ultimately reach the observer set out from the sender in a direction making an angle θ_0 with the vertical. The frequency of these waves will therefore be f_2, where

$$f_2/f_0 = c/(c - v \sin \theta_0) \qquad \text{.....(2).}$$

If now we have $c \gg v$, as is actually the case in practice, (1) and (2) may respectively be written

$$f_1/f_0 = (c + v)/c \qquad \text{.....(1a),}$$

and

$$f_2/f_0 = (c + v \sin \theta_0)/c \qquad \text{.....(2a),}$$

so that

$$(f_1 - f_2)/f_0 = v\,(1 - \sin \theta_0)/c \qquad \text{.....(3).}$$

Now the uniform succession of signal maxima and minima experienced at the receiver may be regarded as beats of frequency $(f_1 - f_2)$ produced by the direct

and indirect waves. As this quantity* is measured directly, $\sin\theta_0$ may be estimated if the velocity of the aeroplane is known.

It is therefore evident that in the moving-transmitter method, for an ionized layer of horizontal stratification, the angle of incidence at the layer is measured. The height deduced from this quantity is therefore the same as that obtained by means of the other methods previously discussed. This quantity, which has been termed the "equivalent height," is greater than the actual maximum height reached by the atmospheric waves.

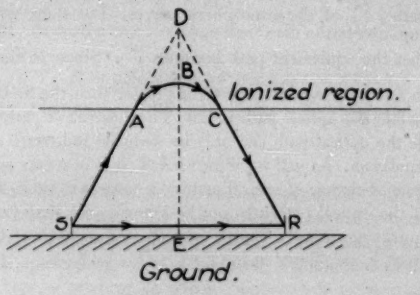


Fig. 1. Diagram of ray track.

Combining the above result with those arrived at in part I, we may summarise as follows:

(1) The frequency-change† method and the group-retardation method measure the same quantity, namely $c\int\frac{ds}{U}$ for the atmospheric waves, where ds is an element of the atmospheric wave track along which the group velocity is U.

(2) The angle-of-incidence method and the moving-transmitter method both enable us to find the angle of incidence of the atmospheric waves at the lower boundary of the layer.

(3) For cases in which the influence of the earth's magnetic field is negligible (e.g. for short waves) all four methods enable us to deduce the equivalent path

* The magnitude of $(f_1 - f_2)$, in the cases dealt with by Mirick and Hentschel, ranged from 0·1 to 0·4 cycles per second.

† In part I this was termed the wave-length-change method, but, in view of the present day tendency in wireless terminology, the name frequency-change method seems preferable. In the application of this method the interference phenomena which are observed are produced artificially by continuous variation of the frequency of an unmodulated continuous wave through a small range. A modification of this method may, however, be noted as specially suitable for ultra-short wave working, in which the modulation frequency of a telephony station and not the carrier frequency is varied continuously through a small range. Experiments have already been carried out with this method and it is hoped to give an account of the modifications it entails in a future paper.

$\int\frac{ds}{\mu}$ of the atmospheric waves (μ being the refractive index at any point along the path) which is equal to $SD + DR$ in Fig. 1; from a knowledge of this the equivalent height DE may be calculated.

§ 3. THE RELATION BETWEEN OPTICAL PATH, EQUIVALENT PATH AND FREQUENCY

In part I a distinction was drawn between the optical path $\int\mu ds$ and the equivalent path $c\int\frac{ds}{U}$ of the atmospheric waves. For short waves we may write $U = c\mu$ so that the equivalent path becomes $\int\frac{ds}{\mu}$. Since, in the ionized medium, μ is less than unity the equivalent path is greater than the lineal trajectory of the wave track, while the optical path is less. The equivalent path may be measured directly while the optical path can only be deduced indirectly and approximately after an interpolation. As will be shown below, it is however possible to measure directly the rate of change of optical path with respect to time, although the actual magnitude of the optical path may not be known. We shall call the optical path P and the equivalent path P'. Both P and P' are functions of the frequency and, as shown in part I, are related as in (4).

$$P' = P + f.dP/df \qquad \text{.....(4),}$$

where f is the mean frequency of the waves, so that

$$P = \frac{1}{f}\int_0^f P'df \qquad \text{.....(5).}$$

To estimate the value of P a curve exhibiting the relation between P' and f must be drawn. By extrapolation from this curve to zero frequency the value of the integral in (5) may be estimated for any particular frequency and the optical path thus found for that frequency.

When both the equivalent path and the optical path of the atmospheric waves are known for any particular frequency a rough estimate may be made of the geometrical length of the path. This is most easily seen in the case of vertical incidence, for in that case the actual height $s/2$ reached by the waves must lie between $P'/2$ and $P/2$.

The results immediately above are independent of the particular law of dispersion which is valid for the propagation of waves in the ionized medium, so long as the group velocity is less and the phase velocity greater than the velocity of light in free space. It is therefore evident that the relation between P' and f, which may be determined experimentally, is of the greatest importance.

If the law of dispersion for the layer is that usually taken as the basis of the theory of ionic refraction when the earth's magnetic influence is neglected, we have

$$\mu^2 = 1 - Ne^2/\pi mf^2 \qquad \text{.....(6),}$$

where N is the number of electrons, of charge e and mass m, per c.c. at any point. In such a case it is possible to proceed much further in the interpretation of the (f, P') curve, in that the actual height reached by any particular frequency and also the gradient of ionization in the layer may then be deduced.

Before proceeding to the general case of any type of ionization gradient it is instructive to consider first the two special types of gradient discussed in part I.

We take first the case in which the ionization N varies directly as the height y above a certain level h. We then have, from (6),

$$\mu^2 = 1 - ay/f^2 \qquad \text{.....(7)},$$

where a is a constant. In such a case the equivalent path of the atmospheric waves* is given by

$$P' = 2h/\cos\theta_0 + 4f^2\cos\theta_0/a \qquad \text{.....(8)},$$

where θ_0 is the angle of incidence of the waves at the layer. For vertical incidence this becomes

$$P' = 2h + 4f^2/a \qquad \text{.....(9)},$$

so that this type of gradient would be indicated by a parabolic (f, P') curve, from which h and a could be determined. For this particular case the geometrical length of path s of the atmospheric waves may be shown to be equal to $2h + 2f^2/a$, so that the actual height reached by waves of frequency f would be half this quantity.

For the second type of gradient, in which the ionization is taken as varying as the square of the height y above the lower boundary of the layer, supposed to be situated at a height h, we have†, corresponding to (7), (8) and (9), the following equations:

$$\mu^2 = 1 - by^2/f^2 \qquad \text{.....(10)},$$

where b is a constant,
$$P' = 2h/\cos\theta_0 + \pi f/b^{\frac{1}{2}} \qquad \text{.....(11)},$$

and, for vertical incidence,
$$P' = 2h + \pi f/b^{\frac{1}{2}} \qquad \text{.....(12)}.$$

In this case therefore the relation between P' and f is linear, and from it h and b may be deduced. The actual length of path s of the atmospheric waves may be shown to be $2h + 2f/b^{\frac{1}{2}}$, so that the height reached by any particular frequency is half this value.

In either of the two cases considered above, since the actual height reached by any particular frequency f may be found and since at that height we know that the value of the electronic density N is equal to $\pi mf^2/e^2$, μ in (6) being zero‡, it is evidently possible to deduce the relation between ionization and height.

I am greatly indebted to Dr W. de Groot of the Physical Laboratory of Philips Lampworks, Holland, who, in the discussion of a lecture at Eindhoven, pointed out to me that the general problem of the determination of the actual height reached

* Part I, p. 51 (28) and (30). † Part I, p. 51 (29) and (31).

‡ This statement requires further qualification. Strictly the value of μ never beomes zero, for such a condition requires that there shall be no absorption, and absorption is never absent. However, as μ tends to zero, the wave-length in the medium tends to infinity, so that reflection proper occurs.

by waves of any frequency from the relation between equivalent height and frequency is entirely analogous to a dynamical problem solved by Abel many years ago, the equation to be solved being, in fact, that known as Abel's integral equation*. The general analogy between these dynamical and wave-problems is being fully dealt with by Dr de Groot elsewhere and only the solution of the integral equation so far as it concerns the problems discussed immediately above need be given here. Let us write (6) as

$$\mu^2 = 1 - Ne^2/\pi m f^2 = 1 - N'/f^2 \qquad \text{.....(13).}$$

In the case, say, of vertical incidence, we have

$$P' = \int \frac{ds}{\mu} = 2h + 2\int_0^{y_0} \frac{dy}{(1 - N'/f^2)^{\frac{1}{2}}} \qquad \text{.....(14),}$$

where y_0 is the height at which $N' = f^2$. The value of $2h$ is that of P' when f is zero, and must obviously be found by extrapolation from the (f, P') curve. Eq. (14) may be written

$$\frac{P' - 2h}{2f} = \frac{\phi(f)}{2f} = \int_0^{y_0} \frac{dy}{(f^2 - N')^{\frac{1}{2}}} \qquad \text{.....(15).}$$

Our problem is therefore to find y in terms of N', $y = \psi(N')$, say, when $\phi(f)$ is known. The solution of (15) is†

$$\psi(N') - \psi(0) = \frac{1}{\pi}\int_0^{N'^{\frac{1}{2}}} \frac{\phi(f)}{(N' - f^2)^{\frac{1}{2}}}.df \qquad \text{.....(16),}$$

so that if y_0 is the actual height above h reached by waves of frequency f_0 we have

$$y_0 = \frac{1}{\pi}\int_0^{f_0} \frac{\phi(f)}{(f_0^2 - f^2)^{\frac{1}{2}}}\,df \qquad \text{.....(17).}$$

If therefore the heights to which different frequencies penetrate are known, the ionization as a function of the height may be deduced.

§ 4. THE RELATION BETWEEN THE TEMPORAL RATES OF CHANGE OF THE OPTICAL AND EQUIVALENT PATHS

As was mentioned in the last section, the optical path of the atmospheric waves cannot be measured directly, though its approximate value may be deduced from the relation between equivalent path and frequency. It has been shown recently‡, however, that it is possible to measure the rate at which the optical path is changing with time, even though the magnitude of the optical path itself is not known. To do this the numbers of interference fringes produced by (*a*) increase and (*b*) decrease of the transmitter frequency through a known range in a known time are compared.

* Abel, *Œuvres, Journ.* 1, 11. See also Tait, *Proc. R. S. E.* 1874–5; and Bateman, *Brit. Ass. Report* 1910, p. 352.

† See Tait and Steele, *Dynamics of a Particle* (London, 1878), p. 406, where the following problem is proposed and solved. "To find the curve in which the time of descent to the lowest point is a given function $\phi(a)$ of a, the vertical height fallen through."

‡ *Proc. R. S.* A, **126**, 548 (1930).

Let us suppose that the optical path difference is altering at a definite rate dP/dt, t being the time. Then we can regard the frequency of the atmospheric waves as being $\frac{f}{c}\cdot\frac{dP}{dt}$ cycles per second less than that of the ground wave, so that beats will be produced. Let us now suppose that at the same time the frequency of emission is steadily increased. So far as this effect alone is concerned the down-coming waves will be $\frac{P'-P_0'}{c}\cdot\frac{df}{dt}$ cycles per second lower in frequency than the ground waves, P_0' being the equivalent path of the ground waves. The number of beats per second dn_1/dt between ground and atmospheric waves due to these two effects combined will therefore be given by

$$\frac{dn_1}{dt}=\frac{P'-P_0'}{c}\frac{df}{dt}+\frac{f}{c}\frac{dP}{dt} \qquad \text{.....(18).}$$

If now the frequency of emission is steadily decreased, the rate of change of optical path remaining as before, the number dn_2/dt of beats per second produced by the two effects is now given by

$$-\frac{dn_2}{dt}=-\frac{P'-P_0'}{c}\frac{df}{dt}+\frac{f}{c}\frac{dP}{dt} \qquad \text{.....(19).}$$

In practice we actually find the numbers Δn_1 and Δn_2 of signal maxima produced by first an increase and then a decrease in the transmitter frequency continuously through a range Δf in a time Δt, so that from (18) and (19) we have

$$P'-P_0'=c\left(\frac{\Delta n_1+\Delta n_2}{2\Delta f}\right) \qquad \text{.....(20),}$$

and

$$\frac{dP}{dt}=\frac{c}{f}\left(\frac{\Delta n_1-\Delta n_2}{2\Delta t}\right)=\lambda\left(\frac{\Delta n_1-\Delta n_2}{2\Delta t}\right) \qquad \text{.....(21),}$$

where λ is the mean wave-length. The equivalent path P' and the rate of change of the optical path dP/dt may therefore be derived from these measurements since P_0' is known, and, if the observations are repeated at intervals, the rate of change of the equivalent path with time may also be found, so that dP/dt and dP'/dt may be compared.

The results of part I may be used to show the relations that may exist between dP/dt and dP'/dt in special cases. We take first again the case of ionization varying as the height above a certain height h. For vertical incidence we then have, as before,

$$P'=2h+4f^2/a \qquad \text{.....(9),}$$

and also*

$$P=2h+4f^2/3a \qquad \text{.....(22).}$$

Now the optical path may vary owing to an alteration of h or to an alteration of a. The first would be an alteration of the height of the layer as a whole, while the second would be an alteration due to some readjustment of the ionization N*.

* See part I, p. 51 (30).

Evidently in the first case we should have dP'/dt equal to dP/dt, while in the second case dP'/dt would be equal to $3.dP/dt$*.

For the second case previously considered, in which the ionization varies as the square of the height above the lower boundary of the layer at height h, we have, for vertical incidence,

$$P' = 2h + \pi f/b^{\frac{1}{2}} \qquad \text{.....(12),}$$

and also†

$$P = 2h + \pi f/2b^{\frac{1}{2}} \qquad \text{.....(23).}$$

In this case therefore an alteration of the height of the layer as a whole would again be shown by the equality of dP'/dt and dP/dt, while for an alteration of the ionization within the layer dP'/dt would be equal to $2.dP/dt$.

I am much indebted to my colleague Prof. A. E. Jolliffe for friendly assistance with the general case of ionization varying as the nth power of the height y above the lower boundary of the layer at height h. The value of the refractive index μ is now given by

$$\mu^2 = 1 - Ay^n \qquad \text{.....(24),}$$

where A is a constant. In this case, for vertical incidence we have

$$P' = \int \frac{ds}{\mu} = 2h + 2\int_0^{A^{-1/n}} \frac{dy}{(1 - Ay^n)^{\frac{1}{2}}} = 2h + \frac{2}{A^{1/n}} \cdot \frac{\Gamma\left(\frac{1}{n} + 1\right)\Gamma\left(\frac{1}{2}\right)}{\Gamma\left(\frac{1}{n} + \frac{1}{2}\right)}$$

.....(25),

and

$$P = \int \mu ds = 2h + 2\int_0^{A^{-1/n}} (1 - Ay^n)^{\frac{1}{2}}\, dy = 2h + \frac{2}{A^{1/n}} \cdot \frac{\Gamma\left(\frac{1}{n} + 1\right)\Gamma\left(\frac{3}{2}\right)}{\Gamma\left(\frac{1}{n} + \frac{3}{2}\right)}$$

.....(26).

An alteration in the height of the layer as a whole would mean an alteration in h and would be shown by equal alterations in P and P'. On the other hand a readjustment of ionization in the layer would mean an alteration in the value of A, for which case

$$\frac{dP'}{dP} = \frac{dP'}{dt} \Big/ \frac{dP}{dt} = \frac{n+2}{n} \qquad \text{.....(27),}$$

a general formula which includes the two examples discussed above as special cases.

* This significance of the relation between optical and equivalent paths has been recently discussed by G. Breit, *Proc. Inst. Rad. Eng.* **17**, 1815 (1929), who has also suggested means by which the group-retardation method can be modified so as to measure the phase of the downcoming waves.

† See part I, p. 51 (31).

§ 5. EXPERIMENTAL RESULTS AND DISCUSSION

Measurements of equivalent height by different methods. As has been shown above, we should expect the values of the equivalent height obtained by the frequency-change method and the group-retardation method to be the same if the observations are made under the same experimental conditions. So far no exact comparison has been made by use of the same wave-length, but a comparison on slightly different wave-lengths is possible. For example, Hafstead and Tuve* in America, using the group-retardation method and 70-metre waves, found heights of 252, 235, 225, 232, 225, 232, 220, 225, 226 and 232 km. (average 231 km.) on different days between October 19 and November 17, 1928. These results may be compared with those of a recent series of measurements made with the frequency-change method and 100-metre waves, in which equivalent heights of 223, 229, 224, 235, 245, 223, 250 and 223 km. (average 231 km.) were recorded. The observations in the latter series were made on consecutive days from December 10 to December 17, 1929, at the Radio Research Station, Slough, on transmissions from the National Physical Laboratory. The agreement between the results obtained by the two methods is therefore very good. It should be added that, in the English series of observations, there were also indications of partial reflection from a height of about 105 km. on four of the eight days in question, supporting the conclusion previously arrived at that there are two regions in the upper atmosphere which reflect wireless waves. Further reference is made to this matter below.

In the case of the comparison of results obtained by the angle-of-incidence method with those obtained by either the frequency-change or the group-retardation method, it is important to remember that unless very short waves are used, and unless the layer is horizontally stratified, we must not necessarily expect agreement. Unfortunately, no data for such a comparison of equivalent-height measurements with short waves are available, but a comparison can be made for medium wave-lengths. In Fig. 2 are shown the results of a series of observations made at Peterborough on transmissions from Birmingham on June 30, 1927, in which the equivalent height of the layer was measured simultaneously by both the angle of incidence method and the frequency-change method. These observations were made immediately after sunrise, when conditions are specially favourable for such a comparison, since during that period a single down-coming wave, free from secondary waves, is usually received.

It will be seen that, although the average heights by the two methods are about the same, the departure from the average is much greater in the case of the observations made by the angle of incidence method than in the case of those made by the frequency-change method. This difference can perhaps be explained by the assumption that "reflection" does not always take place at a point mid-way between the two stations, in which case we infer that the layer cannot always be regarded as horizontally stratified and that the stratification varies with time.

* *Proc. Inst. Rad. Eng.* **17**, 1521 (1929).

The existence of more than one ionized region in the upper atmosphere. The first set of observations on the diurnal variation of the equivalent height of the Kennelly-Heaviside layer were made with 400-metre waves, for which it was usually found that, after sunset, the equivalent height increased slowly, reached a maximum before sunrise, and subsequently fell rapidly to the daytime value. On certain occasions, however, during the few hours before dawn, penetration of the layer was found to take place, the waves then being reflected by an upper region which was richer in ionization. For convenience, the Kennelly-Heaviside layer or lower region has been termed *E* region, while the upper region has been termed *F* region.

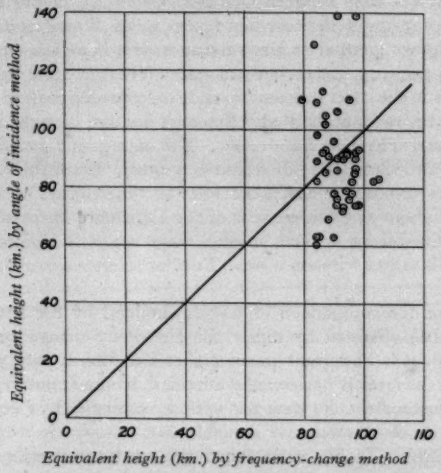


Fig. 2. Birmingham transmission received at Peterborough, June 30, 1927. Wave-length 491 metres.

With 100-metre waves it has been found that penetration of the lower region *E* takes place on all nights, reflection taking place from the upper region *F*. On the majority of days the same result is found during the daylight hours, but, on some days, the ionization in region *E* is sufficient to bring about the reflection of waves of this length. During the daytime, therefore, with 100-metre waves, one of two ranges of equivalent heights, corresponding to the two regions, may be recorded. On occasion it is found that reflection from both regions takes place simultaneously, and the heights of both regions may be measured. At other times it is found that, in a series of observations made at five or ten minute intervals, region *E* and region *F* (or *vice versa*) are recorded in successive observations indicating a rapid decrease (or increase) in the ionization of region *E*. Photographic records illustrating this

are given below. A typical series of observations in this connection is that made at King's College, London, on January 13, 1929, with 99·8-metre waves from the Teddington transmitter, when the following equivalent heights in kilometres were measured at 10 minute intervals between 10.0 a.m. and 2.0 p.m. G.M.T.: 229, 229, 236, 244, 217, 229, 229, 230, 204, 196, 229, 100, 99, 93, 98, 99, 96, 98, 232, 99 (and 220), 99, 229, 229, 99. It will be seen that these heights fall into two definite series of mean values, 226 km. and 98 km.

Another phenomenon, which illustrates the physical significance to be attached to the term "equivalent path," may also be noted. It is sometimes found that, even within such a short interval as a minute, quite definite variations of equivalent height are observed, which, if interpreted as variations in the position of the layer as a whole, would indicate extremely rapid movements. There is, however, another possible explanation in terms of the variations of the ionization density of the region through which the waves have to pass before reaching the region where they are reflected. This is suggested by the fact that during the daytime, when conditions are otherwise normally very stable, a sudden increase in the height of region *F* is accompanied by partial reflection from region *E*, while, on the other hand, a sudden fall in the height of region *F* is accompanied by the disappearance of reflected waves from region *E*. As examples of these effects the following results may be quoted; they all refer to transmissions on 100-metre waves from Teddington, the receiving stations being at King's College, London, and at the Radio Research Station, Slough.

(*a*) On December 3, 1929, the average height of region *F* recorded at King's College was 230 km., while that recorded at Slough was 238 km. (The difference is considered as being outside the limits of experimental error.) At the same time there were noted signs of reflection from region *E* at Slough throughout the series of observations, but no such signs were observed at King's College. The occurrence of reflection by region *E* therefore seemed to be accompanied by a greater equivalent height of region *F*.

(*b*) On December 4, 1929, at Slough, reflection from both regions took place at 12.5 p.m. and 12.10 p.m., heights of 243 and 112 km. being measured. At 12.15 p.m., however, all signs of reflection from region *E* had disappeared, and a height of 227 km. was recorded. At the same time there was noted a large increase in the amplitude of the downcoming waves from region *F*.

(*c*) At 12.25 p.m. on December 19, 1929, at Slough, rapid variations of the equivalent height of region *F* took place within one minute. At the same time the amplitude of the downcoming waves from region *F* was found to vary, an increase of equivalent height being accompanied by a reduction in amplitude and also by the development of signs of reflection from the lower region. Within this minute the equivalent height was found to increase from 230 km. to 240 km., the amplitude at the same time being reduced to about 40 per cent. of its original value.

The above results leave little doubt that the rapid variations in the equivalent height of region *F* measured during the day are not due to motions of region *F* at all, but to the influence of the ionization in the lower region *E*. An increase in

ionization brings about a reduction in the refractive index μ and so increases the equivalent path $\int \frac{ds}{\mu}$ for waves which pass through this region. At the same time the absorption of waves passing through it is increased.

In observations made during a magnetic storm, Hafstead and Tuve* recorded that the height of the ionized layer was abnormally great, and Maris and Hulbert† have interpreted this as an increase in the actual height of the reflecting region. It is clear from what has been said above that though this may have been the case it was not necessarily so, for an increase in the ionization in the lower regions of the atmosphere due to the magnetic storm would reduce the group velocity of the waves in those regions and thus give a fictitious increase in the height of the region that actually reflected the waves. In this connection it is of interest to note that the reflection of 70-metre waves from the abnormally great equivalent heights of region *F*, observed by Hafstead and Tuve during the magnetic storm of October 18, 1928, was accompanied by reflection from region *E* at a height of 100 km., although reflection from this lower region was not observed on the days either preceding or succeeding the magnetic storm.

The temporal rates of change of optical and equivalent paths. As has been shown above, it is possible to measure the rate of change of the optical path of the atmospheric waves by comparing the numbers of signal interference maxima produced by increasing and decreasing frequency-changes made at the sending station; and further, from a comparison of the rates of change of the equivalent and optical paths, it is possible to derive information relating to the type of variation in ionization which is responsible for these changes. Experiments designed to measure dP'/dt and dP/dt for this purpose were begun on April 26, 1929, with 100-metre waves from the Teddington transmitter, which were received at King's College, London, and at the Radio Research Station, Peterborough. Previous observations had shown that the equivalent height of the *F* region fell rapidly in the early morning, and this period was therefore considered a suitable one for measuring also the rate of change of optical path.

A typical series of the results obtained is shown in Fig. 3, where the values of the equivalent path of the atmospheric waves and the rate of change of optical path at different times on April 26, 1929, are shown. The two curves exhibit an interesting correlation. When the equivalent path is increasing, the optical path is seen to be increasing, while when the equivalent path is decreasing, the optical path is seen to be decreasing also. At about 4.44 a.m. the rate of decrease of the equivalent path is a maximum, while about this time the rate of change of optical path is also a maximum. During this period of rapid change the rate of change of equivalent path is definitely greater than the rate of change of optical path, the ratio being about 1·3. To get a more accurate comparison, a 20 minute continuous series of observations was made between 4.50 a.m. and 5.10 a.m., both P' and dP/dt being measured every minute. These results provide a very accurate comparison, and

* *Terr. Mag. and Atmos. Elect.* **34**, March (1929). † *Proc. Inst. Rad. Eng.* **17**, 494 (1929).

show that the average value of dP'/dt was 2·9 km./min., while that of dP/dt during the same period was 2·3 km./min.

During the period from 6.00 a.m. to 6.30 a.m. it is noted that the rate of decrease of optical path was actually greater than the rate of decrease of equivalent path; this indicates a state of affairs quite different from that obtaining earlier.

Special interest is attached to the period when the equivalent height is falling rapidly owing to the renewal of solar influence at sunrise. In the example quoted above, the ratio of dP'/dt to dP/dt was 1·3. For the same morning period on January 17, January 31 and February 11, 1930, this ratio was also found to be

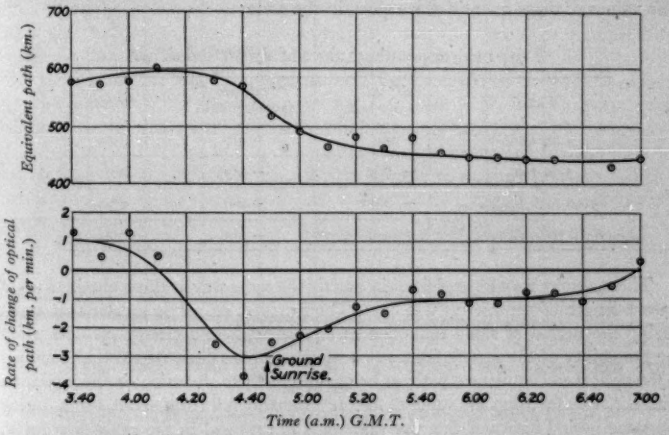


Fig. 3. Teddington transmission received at King's College, London, April 25, 1929. Wave-length 100 metres.

greater than unity, the respective values being 1·9, 1·9 and 2·2. We therefore conclude that the reduction in equivalent height which takes place in the early morning cannot be brought about solely by a reduction in the height of the layer as a whole but must be either wholly or partly brought about by an increase of ionization within the layer.

For the morning period succeeding the period of rapid fall, when the equivalent path seems practically constant while the optical path is steadily decreasing, another influence seems to be at work. During this period it is known that region E is forming in gradually increasing density. This formation would produce a gradually decreasing optical path and an increasing equivalent path for rays passing through this region. It appears possible, therefore, for a fall of the actual height at which waves are reflected in region F to be compensated, so far as equivalent-path

measurements are concerned, by an increase in ionization in region E, so that practically no variation in equivalent height results.

Similar results were found for the early morning period when a wave-length of 150 metres was used. In Fig. 4 is shown the variation of the equivalent path with time on March 21, 1930, for this wave-length. The transmission took place from the National Physical Laboratory and was received at King's College, London. It is seen that up till 6.30 a.m. the atmospheric waves were deviated by region F, but that after about this time region E formed in sufficient intensity to reflect. During the period of the rapid reduction in the equivalent height of region F measurements of dP/dt were made also, so that dP'/dt and dP/dt could be compared. The values of these quantities are given in the table:

Table: Corresponding values of dP'/dt and dP/dt

Time	dP'/dt (km. per min.)	dP/dt (km. per min.)	dP'/dP
5.20 a.m.	− 20	− 4·7	4·25
5·30 a.m.	− 7·5	− 3·35	2·2
5.40 to 6.0 a.m.	− 2·35	− 2·6	0·92
6.10 to 6.30 a.m.	+ 0·6	− 2·5	− 0·24

These results may be explained in exactly the same way as those obtained when a wave-length of 100 metres was used. The fact that dP'/dP is greater than unity during the period of rapid reduction of equivalent height clearly excludes again the possibility of the layer's falling as a whole, and we must regard the reduction in the optical and equivalent paths as being due chiefly to alterations in the ionization gradient. The fact that the value of dP'/dP gradually fell from about 4 to 2 during the same period indicates that the value of n in (27) and in the expression for μ, viz. $(1 - Ay^n)^{\frac{1}{2}}$, increased from about $\frac{3}{2}$ to 2, so that the effect of solar radiation was to sharpen the boundary between the non-refracting and refracting regions.

For the period immediately succeeding that of rapid fall the equivalent and optical paths were altering in opposite senses. This is what we might expect as a result of the formation of region E. The reduction in the refractive index of that region, due to gradually increasing ionization, would increase the equivalent path and reduce the optical path for waves which pass through it.

It may not be superfluous to add that when both P' and P are decreasing, the actual length of paths of the downcoming waves is being steadily reduced at a rate which lies between dP'/dt and dP/dt. This follows because ds/dt must lie between the limits of $\frac{d}{dt}\int\frac{ds}{\mu}$ and $\frac{d}{dt}\int\mu.ds$.

During the daytime, variations in the optical path of the downcoming waves are not usually marked. On one occasion, however, on December 3, 1929, between 12.15 and 12.30 p.m., a slow increase of optical path of downcoming rays from

region F (indicated by the number of "fringes" for increasing frequency changes being slightly larger than those for decreasing frequency changes) took place throughout the period of the test. At the same time the equivalent height was found to decrease slightly. These changes may be interpreted as indicating the steady reduction in the ionization of region E, which would increase the value of P but decrease the value of P' for the waves passing through it.

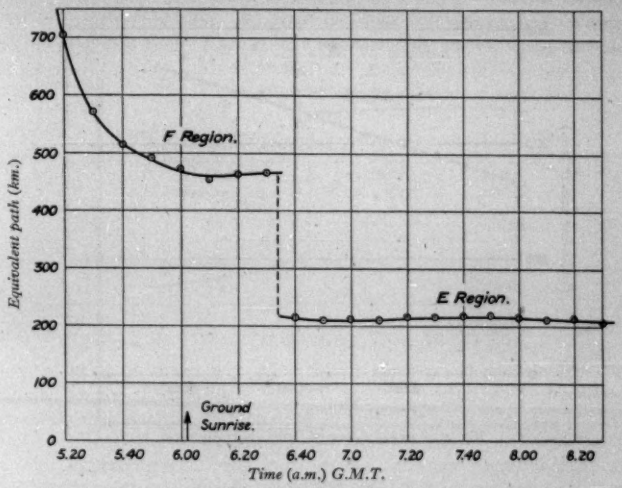


Fig. 4. Teddington transmission received at King's College, March 21, 1930.

The variation of equivalent path with frequency. It has been shown in § 3 that information relating to the actual height reached by the atmospheric waves may be derived from the curve exhibiting the relation between P' and f. It will be easily understood, however, that the experimental difficulties of getting the necessary simultaneous measurements of P' for different values of f, in order to construct such a curve, are very great and, as yet, no such complete series of observations is available. We can, however, get a rough idea of the shape of this curve if we take the average of the results obtained on different days. In that case it is, of course, important to include only observations made at the same time of the day and only those obtained when reflection for all the frequencies used is taking place from the same region (i.e. say from region E). The time chosen in this connection is the period consisting of the two hours after ground sunrise when the equivalent heights measured do not as a rule vary rapidly with time.

The average values of P' for different frequencies corresponding to wave-lengths of 400, 212, 150, 130 and 100 metres for observations at short distances (i.e. at

King's College, London, and at Ditton Park on transmissions from Teddington) are exhibited in Fig. 5. The observations are perhaps not accurate enough in this connection to enable us to be quite certain whether the curve is linear or parabolic for the higher frequencies and thus enable us to decide between ionization gradients varying with the square of the height or with the height above the lower boundary of the layer, and more data are being sought in this connection.

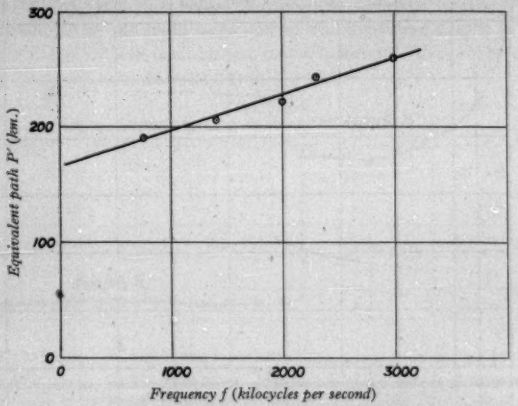


Fig. 5. Average values of P' for short distances.

The curve, however, does enable us to determine without very grave error the optical path P for the highest frequency used (i.e. 3000 kc./sec.), for the exact nature of the extrapolation to zero frequency cannot affect the value of the integral required to any large extent. The value of P for 100-metre waves is in this way found to be 212 km., and since the case in question is one of approximately vertical incidence we know that the actual height reached by the atmospheric waves lies between 106 km. and 129 km. The results of § 4 and equations (28) and (29) in Part I show that the actual height lies nearer $P/2$ than $P'/2$ and it may be shown that this quantity would be 112 km. for an ionization gradient varying linearly with the height, and also about the same value for an ionization varying as the square of the height above the lower boundary of the layer. The (f, P') curve also shows that ionization begins to be appreciable at this time of the day at a height of about 80 km. since this is the value of $P'/2$ when f is zero.

Although a complete set of observations for any one morning is not available, the results of many early morning measurements leave little doubt that there is a discontinuity in the experimental (f, P') curve, so that its form for a wide range of values of f must be roughly somewhat as indicated in Fig. 6.

The critical value of f for which the penetration of region E just takes place depends on the time of the day at which observations are made, the value just before dawn sometimes corresponding to a wave-length of 400 metres, while the value during the daytime corresponds to a wave-length of about 100 metres. The question whether, from this diagram, we can derive information relating to the actual height reached by waves reflected by region F by the method adopted for region E therefore arises. In this connection let us first consider the variation of optical path with frequency. If, for a particular frequency f_1, the waves just penetrate the lower region there will be a discontinuity in the relation between P

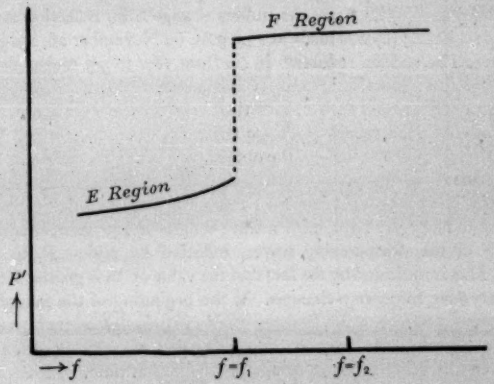


Fig. 6. General form of P'/f curve.

and f at this frequency so that the equivalent path P', which is equal to $P + f\frac{dP}{df}$, will be infinite for the same frequency. In trying to find P for a frequency such as f_2 in Fig. 6, we must therefore recognise the fact that, in integrating the area between the (f, P') curve and the f axis, the value of P' is infinite* at $f = f_1$. This result seems to indicate that we cannot rigorously obtain the integral required, so that the actual height reached by waves which are deviated by the upper region cannot be obtained by the method in question.

Some typical photographic records. On plate I, (*a*) to (*d*), are reproduced some illustrative records obtained by the frequency-change method. In all cases they represent the variation of signal intensity with time. During the periods when the frequency is changing (which are usually from two to three seconds in duration) interference maxima and minima are artificially produced. The signal variations

* The fact that for a critical frequency the equivalent path is theoretically infinite and therefore the group velocity zero may perhaps be of importance in connection with the explanation of echoes of long retardation.

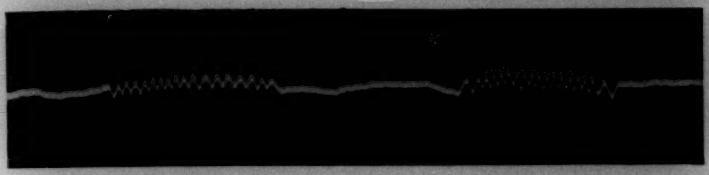
during the intervals when the frequency of the sending station is maintained constant are due to natural variations in the intensity and phase of the downcoming waves. In all cases the number of signal maxima (Δn) divided by the frequency change (Δf) which produced them gives the difference in the times required for a group of waves to travel from sender to receiver via the atmosphere and via the ground, so that $c.\Delta n/\Delta f$ is the equivalent path difference. When the optical path is altering it is important to note that the mean of the two values of Δn for increasing and decreasing frequency changes must be used in this connection (see § 4). In all cases, except where the contrary is stated, the emitting station was at Teddington and the receiving station at Ditton Park.

Records (*a*) and (*b*) illustrate the sudden change from reflection at region *F* to reflection at region *E*; (*a*) was made at 12.5 p.m. on November 26, 1929 and (*b*) ten minutes later. The sudden reduction in Δn from 18·0 to 9·5 means that the equivalent path of the atmospheric waves was almost halved. The frequency change for both records was 10·4 kc./sec. and the mean frequency 3000 kc./sec. (wavelength 100 metres). In record (*b*), since the value of Δn is greater for decreasing than increasing frequency-changes, the optical path of the downcoming waves must have been decreasing slowly.

Record (*c*) illustrates what has been termed "phase-fading." It was made at 7.40 a.m. on January 31, 1930, with a wave-length of 150 metres, and shows the optical path of the downcoming waves, reflected by region *F*, to be steadily decreasing. This is indicated by the fact that the value of Δn is greater for decreasing than for increasing frequency-changes. At the beginning of the record, when the emitted frequency was constant, the steady reduction in optical path produced slow regular beats of period about 3·7 sec., indicating that the optical path of the downcoming waves was being reduced by 150 metres in that time. This corresponds to a value of $-$ 2·4 km./min. for dP/dt. The same quantity can be found from the difference in Δn for increasing and decreasing frequency changes. For example, for substitution in (21), we have $\Delta n_1 = 22{\cdot}5$, $\Delta n_2 = 23{\cdot}8$, $\Delta t = 2{\cdot}5$ sec., so that dP/dt is $-$ 2·4 km./min.

Record (*d*) was made at King's College at 7.11 a.m. on February 14, 1930, the mean wave-length being 150 metres. It illustrates the gradual growth of subsidiary "fringes" during the short period of 2½ sec. in which the frequency change took place. Since the subsidiary maxima are just more than twice as frequent as the primary maxima, equivalent paths in the ratio of just over 2 to 1 are indicated, the subsidiary maxima being most probably due to waves which had been twice reflected by the ionized region.

(*a*)



(*b*)

(*c*)

(*d*)

[*To face page* 338

§ 6. ACKNOWLEDGMENTS

In conclusion I wish to express my indebtedness to Mr E. L. Hatcher and Mr A. C. Haxton who have carried out the transmissions from the National Physical Laboratory on which the majority of the observations recorded above were made, and also to Mr W. C. Brown and Mr R. D. Gander for valuable technical assistance at the Ditton Park and King's College, London, receiving stations respectively. I also wish to thank Mr A. L. Green, M.Sc., for assistance in the reduction of some of the data. Finally I wish to acknowledge the kind collaboration of Dr D. W. Dye, F.R.S., who has made accurate measurements of the frequencies and frequency-changes used throughout the series of experiments described above.

The problems discussed arose in connection with work carried out for the Radio Research Board of the Department of Scientific and Industrial Research.

INTERFERENCE MEASUREMENTS WITH SINGLE MOLECULES

Lecture delivered on April* 11, 1930, *by*

PROF. P. DEBYE, the Physical Institute, University of Leipzig.

IN the elementary classical theory of the diffraction of X-rays by crystals every atom is supposed to be the centre of a secondary radiation. The observed effects are accounted for by the phase-differences of the secondary rays set up by the spacial arrangement of the atoms in regular lattices. Now the regularity of the atomic arrangement and the large number of atoms involved, although responsible for the special character of the crystal-scattering, are by no means essential for the creation of an interference effect itself. Any atomic structure will give rise to interferences; and even if only a very small number of atoms be contained in such a structure an effect should still be observable, provided only that the wave-length of the radiation is comparable with the atomic distances. The difference between this type of interference effect and the effect of a crystal grating should not be larger than the difference, say, between the scattering of light by an ordinary grating and the scattering of it by a few parallel slits. We cannot but believe that the chemical formulae in common use are a geometrical representation of the real spacial arrangement of the atom in the molecule. For this reason we have to expect that even for a single molecule, containing perhaps only a few atoms, the angular intensity-distribution of, say, scattered X-radiation will show the effect of the path-differences arising from the peculiar spacial distances between the atoms in the molecule. The observation of such an effect must therefore lead to the absolute measurement of atomic distances in free molecules.

In practice, however, it is not possible to observe the scattering from one single molecule. The best one can do with X-rays, for instance, is to observe the energy scattered by something like 10^{16} molecules at a time, corresponding to the amount of matter contained in 1 mm.^{3} of a gas at atmospheric pressure. Here, then, a serious difficulty seems to arise. Though all these molecules have the same constitution, each of them will give a different angular distribution of scattered intensity, for this distribution will be essentially dependent on the orientation of the molecule with respect to the primary ray. So it is not impossible that if one molecule gives a maximum of intensity in a certain direction, another molecule will give a minimum of scattered intensity in the same direction, merely because it has another orientation. In these circumstances it might be suspected that the summation of all the primary molecular effects, as it is performed in an actual experiment, would destroy everything observable. Fortunately, however, this is not the

* Mr L. Woodward, of Oxford, who is working in the Leipzig Laboratory, has had the kindness to correct this account.

case. A simple calculation shows that the interference effect cannot be entirely obliterated, even by the most irregular distribution in orientation*. A well-known illustration of an effect of the same kind is given by the powder-method in X-ray crystal-analysis†.

To express this result in a formula let us consider a molecule containing atoms 1 ... i ... n, each of these atoms being characterized by a scattering function ψ_i. Then taking the average over all possible orientations, the scattered intensity J will be proportional to the expression

$$J = \frac{1 + \cos^2\theta}{2} \sum_1^n \sum_1^n \psi_i \psi_j \frac{\sin x_{ij}}{x_{ij}} \qquad \ldots\ldots(1).$$

In this formula the summation is to be taken twice over all the atoms and

$$x_{ij} = ksl_{ij} \qquad \ldots\ldots(1'),$$

where l_{ij} is the distance of any atom i from any other atom j, $k = 2\pi/\lambda$ (λ denoting the wave-length of the primary radiation), and $s = 2 \sin \theta/2$ (θ denoting the angle between the primary and the secondary ray). The factor $(1 + \cos^2\theta)/2$ has been introduced to allow for the effect of the polarization accompanying the scattering. The function $\sin x/x$ shows maxima and minima (decreasing in magnitude with increasing order) and so also the whole sum expressing J will in general not give a function which decreases steadily with increasing angle θ. As one of the simplest applications we may consider the case of the CCl_4 molecule, neglecting for the moment the scattering of the central carbon atom, because of the relatively small number of electrons it contains. According to van't Hoff and Lebel the chlorine atoms are arranged at the corners of a regular tetrahedron. The distance of a Cl atom from any other Cl atom therefore is the same, say l, and we only have one value of x, namely

$$x = ksl.$$

Also there exists only one function ψ, the scattering function of the Cl atom. The intensity function is thus

$$J = \frac{1 + \cos^2\theta}{2} 4\psi^2 \left[1 + 3\frac{\sin x}{x}\right] \qquad \ldots\ldots(2).$$

Let us for the moment make the assumption (which we shall justify later) that the influence of the scattering function ψ is only of secondary importance. Let us further consider the intensity curve corrected for the polarization-effect, that is to say not J itself but $2J/(1 + \cos^2\theta)$. Then we have to expect that the scattering of CCl_4 vapour will show maxima and minima in its angular distribution corresponding to the maxima and minima of the function $\sin x/x$. The first maximum should occur (with very good approximation) for $x = 5\pi/2$ and as $x = ksl = 4\pi\,(l/\lambda) \sin(\theta/2)$, we have to expect the first maximum for an angle θ defined by the equation

$$\sin\frac{\theta}{2} = \frac{5}{8}\frac{\lambda}{l} \qquad \ldots\ldots(3).$$

* P. Debye, *Ann. der Phys.* **46**, 809 (1915); P. Ehrenfest, *Amsterdammer Akademie*, **23**, 1132 (1915).

† P. Debye and P. Scherrer, *Göttinger Nachr.*, Dez. 1915; *Phys. Zeitschr.* **17**, 277 (1916); A. W. Hull, *Phys. Rev.* **10**, 661 (1917).

With a given wave-length of the primary radiation, it will only be necessary to observe this maximum and the angle θ for which it occurs to be able to calculate the distance l of the Cl atoms in the single free molecule.

It might appear feasible to observe the scattered radiation from liquids, instead of using gases as is supposed in the foregoing considerations. In liquids the molecules are also oriented at random and the scattered intensity is much larger. Moreover, a great number of liquids have already been investigated and at least one interference maximum has been observed in every case. A closer examination of the condition in liquids, however, shows that here we are not dealing with a pure case of intermolecular interference. There is no reason why this effect should not exist in liquids, but it is certain that at the same time another intermolecular effect is superposed due to the interference of the secondary rays emitted by different molecules. It is this last effect which is responsible for the main part of the interference picture as it is ordinarily observed. No theory exists which is perfect enough to eliminate from the observed intensity-curve the part due to intermolecular interference. So we are left with the necessity of eliminating this effect experimentally and this is done by the use of matter in the gaseous state. Here on the average the molecules are far enough apart to diminish the intermolecular interference effect to such a small fraction of the total effect, that it can be completely disregarded. By this remark I do not mean to say that the observation of the X-ray scattering by liquids is not in itself interesting enough. But by such experiments we do not learn so much about the intra-atomic structure of the molecules; it is the peculiar average regularity in the distribution of the whole molecules with respect to each other which gives the clue for the explanation of the greater part of the interference pattern. Even so it must be emphasized that a satisfactory discussion of the interference phenomena for liquids can only be based on a knowledge of the scattering function of the single molecule. Therefore in this field also the experiment with the gas should come first.

The first experiment was carried out with CCl_4 vapour*. A narrow primary X-ray beam of circular cross-section was passed through a layer of the vapour a few mm. thick at a temperature of approximately 20° above the boiling point and at atmospheric pressure. The primary rays were emitted by a copper-target and consisted for the larger part of the K radiation of this metal (wave-length 1,539 A.U.). The bottom of the box containing the vapour consisted partly of a sheet of aluminium 0·01 mm. thick. A few cm. from this box a photographic plate was mounted to register the secondary radiation emitted by the vapour, whereas the primary ray was absorbed before it could strike the plate. As the result of a twenty hours' exposure a ring was photographed encircling the centre corresponding to the primary ray. So it was proved that in the scattered radiation of the CCl_4 molecule a maximum of intensity occurs at least in one direction making an angle of approximately 36° with the primary ray.

This apparatus, however, was not very suitable for the observation of the scattered intensity over a larger range of angles. Moreover, it seemed possible to diminish

* P. Debye, L. Bewilogua and F. Ehrhardt, *Phys. Zeit.* **30**, 84 (1929).

the time of exposure appreciably by another design, especially of the cross-section of the primary bundle. So the following experiments* were carried out with another apparatus, the principle of which is made clear by Fig. 1. The primary ray now has a rectangular cross-section of 1×4 mm.^{2}; it passes again through a metal box containing the vapour, which is heated electrically. Ordinarily it is absorbed at the end of its path through the vapour. By removal of a slide at the end for a short time it can be allowed to pass through in order to print a mark on the film characterizing the direction of the primary ray ($\theta = 0$). Just beneath the primary beam and as near as possible to it is a window 6 mm. by 3 mm., covered with aluminium foil of thickness 0·01 mm. Through this window the scattered radiation of the vapour leaves the box and is photographed on a film enclosed in a circular film-holder which is centred on the window. The front part of this film-holder consists

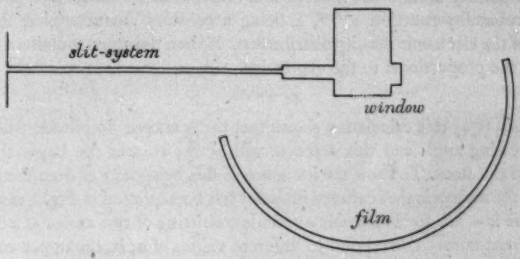


Fig. 1.

of black paper and the back part has double walls, between which water is kept running to cool the film, which might otherwise become too hot owing to the heat coming from the gas-box. The details of the inner construction of the box and of the part defining the primary beam were carefully designed to prevent secondary or tertiary radiation of the walls from passing through the window. This last condition in particular made it necessary to use different apparatus for different parts of the angular range. Three modifications of the apparatus are in use; with the one appropriate for small angles the smallest angle obtainable is $\theta = 8°$.

With the new equipment it was possible to observe in the case of CCl_4 vapour and with copper K_α-radiation the first three interference-maxima. They occur approximately at the angles $\theta = 36°$, $\theta = 65°$, $\theta = 110°$. The time of exposure was reduced to about 4 hours. By application of equation (3) the distance l between the Cl atoms can easily be calculated from the angle of the first maximum. The result is $l = 3{\cdot}1$ A.U.

In deriving this equation I had to point out that it can only hold to a first approximation, namely so far as the influence of the atomic scattering function can be neglected. In fact in the discussion leading to equation (3) I treated the atomic

* P. Debye, L. Bewilogua and F. Ehrhardt, *Sächs. Akad. Ber.* **81**, 29 (1929).

scattering function ψ merely as a constant. Now from the experiments on the scattering of X-rays by crystals we already know that ψ is by no means a constant, but that the intensity scattered by an atom decreases markedly with increasing angle θ. From the point of view of the theory we can explain this behaviour again by the interference principle. In fact the dimensions of the electronic atmosphere constituting the atom are comparable with the wave-length of the X-radiation. Therefore at not too small an angle θ the scattered rays coming from different parts of the atom will have path-differences of appreciable magnitude and can destroy each other by interference. If we had a picture of the electronic density in the atom as a function, say, of the distance from the nucleus, we should at once be able to calculate how ψ will vary with the angle θ.

The whole calculation* and its result become very simple if we assume that the electronic density around the nucleus is a function of the distance r proportional to the probability-function e^{-r^2/a^2}, a being a constant characterizing the "mean radius" of the electronic density distribution. If then the atom contains z electrons, ψ will be the proportional to the expression

$$ze^{-k^2a^2s^2/4}.$$

As $s = 2 \sin(\theta/2)$ this calculation shows that the scattered amplitude will decrease with increasing angle and this decrease will be the steeper the larger the "mean radius" of the atom. To show the influence of this behaviour of ψ on the scattering curve for the molecule the scattered intensity has been plotted in Fig. 2 as a function of x, where $x = ksl$, for a diatomic molecule consisting of two atoms at a distance l. The different curves correspond to different values of a; in the upper curve a has been taken as equal o. To illustrate the effect of the assumption about the molecule in the different curves, every curve is accompanied by a picture of the molecule in which circles are drawn at a distance $r = 2a$ from the nuclei. The distance l of the atomic centres is always the same. From the picture of Fig. 2 we see that the finite dimensions of the atoms will have a double influence.

Firstly with increasing atomic dimension the decrease of the total intensity becomes more and more pronounced. As soon as the atoms become large enough to be approximately equal to half the distance between them the maxima and minima generated by the interference between the radiation of the two atoms will disappear. The experimental evidence supports this conclusion. From scattering experiments with O_2 and N_2 of the same kind as those described for CCl_4 no maximum can be detected; if, however, two Cl atoms are connected with one C atom, as in CH_2Cl_2, the intermolecular interference effect can readily be detected. Another example is furnished by the molecules CO_2 and CS_2. Whereas in the former case the interference maxima are only faintly indicated, they are very pronounced in the latter.

Secondly, there is another influence of the atomic size which is of importance even if the effect of the atomic scattering function is not large enough to destroy the intermolecular interference maxima altogether. The upper curves in Fig. 2

* P. Debye, *Phys. Zeitschr.* **31**, 419 (1930).

show how with increasing atomic size the first minimum is shifted to the right, whereas the first maximum is shifted to the left. In the application of equation (3) to the calculation of the atomic distances no allowance has been made for this shift and therefore the value of l calculated in this way needs correction.

Although the foregoing discussion is good enough to show the result of the atomic size in a general way, it is not sufficient as a basis for a calculation of the correction. This is due to the fact that the assumed distribution-function for the electronic density in the atom represents the real distribution only very incom-

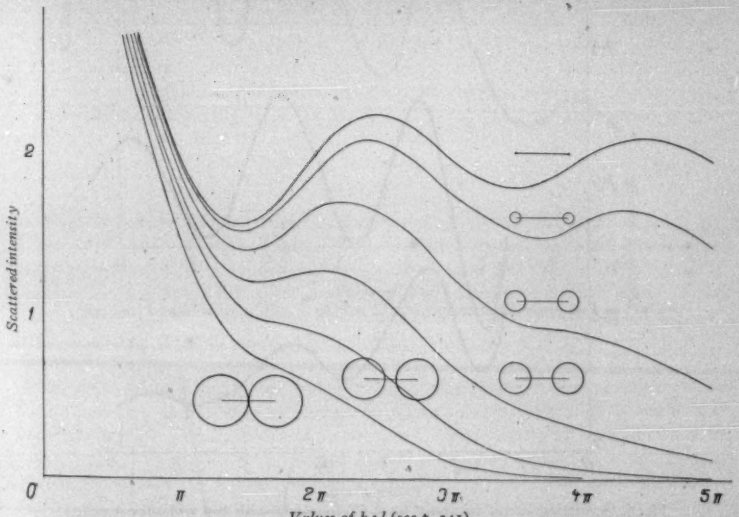


Fig. 2. Scattering curves for various values of a (see p. 344).

pletely. Now Thomas* and Fermi† have shown independently how to arrive at a fairly good approximation of the electron distribution in the atom. Applying essentially the same principle of calculation which had already proved of value for the discussion of the distribution of ions in a liquid and thereby for the properties of strong electrolytes, they arrive at a density function for the atom. The application of their result to our case is especially simple because of the fact that in the approximation attained by the Thomas-Fermi method the density-distribution curve for any atom can be simply read off from one single universal curve by mere adjustment of the scale for the distance r according to the number of electrons contained in the atom considered. From this property of the distribution curve it follows that the

* L. H. Thomas, *Proc. Cambr. Phil. Soc.* **23**, 542 (1927).
† E. Fermi, *Zeit. für Phys.* **48**, 73 (1928).

scattering function ψ of any atom can also be read off from one universal curve, in this case by adjustment of the scale of the variable s, or $2 \sin(\theta/2)$ according to the particular atom considered and the wave-length used. The universal curve itself cannot be expressed by a simple function and had to be calculated numerically*.

Taking account of this correction the intensity scattered by the CCl_4 molecule, corrected for the polarization, has been calculated and is plotted in the lower curve of Fig. 3. The upper curve represents the intensity when the atomic size is neglected. The abscissae are x/π. On the other hand, the films obtained in the CCl_4 experiments

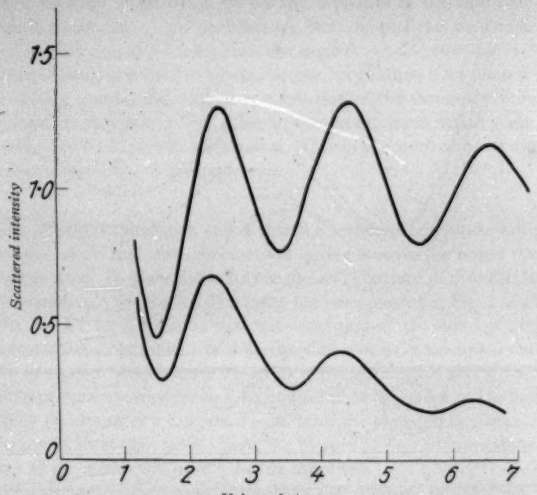


Fig. 3. Scattering curves calculated for C Cl_4 molecule, with and without correction for atomic size.

were recorded with a Zeiss recording photometer. The ordinates of the recorded curves were reduced to intensity values by the help of intensity marks obtained by the photographing of the primary radiation through a rotating sector. In this way it was possible to draw an experimental intensity curve for the scattering of the CCl_4 molecule. This curve is shown in Fig. 4 *b*, the abscissa being s, where $s = 2 \sin(\theta/2)$. The angle θ itself has also been indicated to show that the observation of the scattering for large angles near 180° is not important. The experimental curve is to be compared with the theoretical curve, already plotted as the lower curve in Fig. 3. To make the comparison easier this curve has been plotted a second time in Fig. 4 *a* with enlarged ordinates of the approximate size of the ordinates of the experimental curve, Fig. 4 *b*.

* The calculation in *Phys. Zeit.* **31**, 419 (1930) checks with the formerly published values of W. L. Bragg and S. West, *Zeit. für Krist.* **69**, 118 (1929).

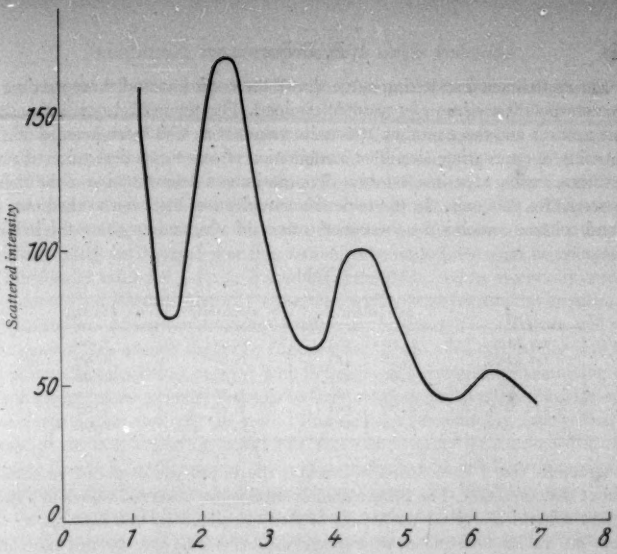


Fig. 4 *a*. Lower curve of Fig. 3, plotted on approximately the same scale as Fig. 4 *b*.

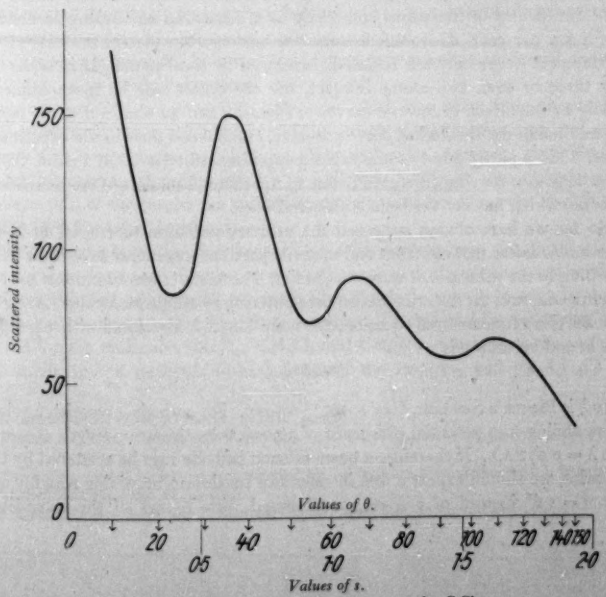


Fig. 4 *b*. Experimental scattering curve for $C Cl_4$.

The experimental scattering curve shows three minima and three maxima and the corresponding values of s were determined. The theoretical curve also shows three maxima and three minima at definite values of x, which can be read off from a drawing of appropriate size. The combination of any theoretical value of x with the corresponding experimental value of s constitutes a determination of the distance l between two Cl atoms. In the table this combination has been worked out. The second column contains the theoretical values of x/π, and to show the influence

Table

	x/π (theor.)	s (exper.)	l (A.U.)
1st Min.	1, 60 (1, 50)	0, 411	3, 00
1st Max.	2, 38 (2, 48)	0, 618	2, 97
2nd Min.	3, 60 (3, 42)	0, 93	2, 98
2nd Max.	4, 30 (4, 44)	1, 10	3, 01
3rd Min.	5, 70 (5, 50)	1, 46	3, 00
3rd Max.	6, 30 (6, 56)	1, 64	2, 96

of the atomic size I have added in brackets the values which would be obtained without the correction. The third column contains the observed values of s and in the last column are given the values of l as calculated by the formula $x = ksl$. These six values differ from an average value $l = 2{\cdot}99$ A.U. by not more than $\pm$ 1 per cent. Remembering that, without the correction for the atomic size, l was found as $3{\cdot}1$ A.U. from the angle of the first maximum, it is seen that after all the finite dimensions of the atoms enter only as a correction amounting to not more than a few per cent. However, it must not be forgotten that in the case of CCl_4 interferences occur between radiations scattered by four atoms. If in other cases only three or even two atoms interact, the correction will be more important. Finally a comparison of the two curves in Figs. 4 *a* and 4 *b* shows that the maxima and minima in the theoretical curve are more pronounced than in the experimental curve. This is partly due to some experimental imperfections, but I think that the essential reason for this difference is due to the thermal motion of the atoms in the molecule which has not yet been considered.

So far we have always supposed the primary radiation to consist of X-rays. But we now know that electrons and material particles in general have to be treated according to the principle of wave-mechanics. Therefore it was of interest to repeat with material rays the experiments on the scattering by single molecules. According to de Broglie's fundamental equation, the wave-length λ associated with particles of mass m and velocity v is

$$\lambda = h/mv,$$

where h is Planck's constant, $6{\cdot}55 \times 10^{-27}$ erg-sec. For cathode-rays with a velocity corresponding to a potential difference of 30,000 volts the wave-length comes out to be $\lambda = 0{\cdot}071$ A.U. If therefore a beam of such cathode rays be scattered by CCl_4 molecules, we should expect a first interference maximum occurring roughly at an angle $\theta = 1{\cdot}6°$ instead of $\theta = 36°$, as observed with X-rays of the wave-length

1·54 A.U. Inspired by the results with X-rays, Mark and Wierl* carried out experiments of this type. They passed a beam of cathode rays perpendicularly through a stream of molecules and in the same case of CCl_4, for instance, the first two interference maxima were observed at angles corresponding to de Broglie's wave-length. The time of exposure was small, of the order of 1 minute. Mark and Wierl have calculated from their experiments that $l = 3{\cdot}14$ A.U. for CCl_4. They apply formula (2), treating the atomic scattering function as a constant, just as we did in the beginning for X-rays. For this reason their value for l must be compared with our former value $l = 3{\cdot}1$ A.U. Formula (2) should indeed be essentially correct except for the polarization factor $(1 + \cos^2\theta)/2$ which, however, is of no importance for small angles. Moreover, the atomic scattering function ψ for electrons will not be the same as the atomic scattering function for X-rays. We can readily perform an approximate calculation, starting with Schrödinger's equation and assuming the electron energy of the primary beam to be large compared with the potential energy of the electron after entering the atom. From such a calculation it follows that in the case of electron scattering ψ will also have the property (in common with the scattering function for X-rays) of decreasing markedly with the increasing angle†. The correction will therefore be similar to the correction in the X-ray case.

A type of experiment which has not yet been performed and which seems worth trying is the scattering of atoms by single molecules in order to detect interferences of the scattered atoms‡. As the de Broglie wave-length involved for a beam of Hg atoms, for instance, moving with a velocity corresponding to their average thermal motion at ordinary temperature, is roughly 0·1 A.U., a possible first maximum for the scattering by CCl_4 molecules could be expected at an angle θ of about 2°. This is merely a suggestion, however, and is open to criticism in its details, though not, I think, in its general character.

The case of CCl_4 is so far the only case which has been more thoroughly discussed with respect to the influence of the atomic scattering function and even this only for X-ray scattering. Several other molecules, however, have already given interference maxima and minima as well with X-rays as with cathode-rays. The discussion of the results has yielded results of some interest, even though the variation of ψ has been neglected.

The photograph of the interferences for CCl_4 having been obtained, it was thought interesting to see if really, as was to be expected, the maxima and minima would become less pronounced for molecules in which only 3 or even 2 atoms could give a strong enough radiation to interfere. To this end the scattering was observed for the molecules $CHCl_3$, CH_2Cl_2 and CH_3Cl§. Decreasing the number of Cl atoms in the molecule indeed flattened the maxima, and for CH_3Cl no

* H. Mark and R. Wierl, *Naturwiss.* **18**, 205 (1930).

† Experiments of H. Mark and R. Wierl, *Zeit. für Phys.* **60**, 741 (1930) support this theoretical result.

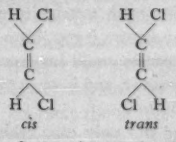
‡ The interference of molecular rays reflected by a crystal has been obtained by J. Estermann and O. Stern, *Zeit. für Phys.* **61**, 95 (1930).

§ P. Debye, L. Bewilogua and F. Ehrhardt, *Sächs. Akad. Ber.* **81**, 29 (1929).

interference maximum could be observed. At the same time it was found that the first maximum shifted very appreciably to smaller angles with decreasing number of Cl atoms. This means that the distance between the Cl atoms increases if a Cl atom is replaced by an H atom. It is by no means improbable *a priori* that van't Hoff's tetrahedron becomes irregular if the atoms connected with the central C atom are different, and the experiments show that this is indeed the case. Moreover, the same conclusion is supported by the comparison of the dipole moments for the chlorine-substituted methanes*. The advantage here is that the magnitude of the change in distance can be measured absolutely.

The molecule CH_2Cl_2 was the first instance in which it was shown that two atoms are enough to give an interference maximum which can be detected experimentally. From this and from the similar case of CS_2 I concluded that one was justified in saying that any distance in a molecule could be measured interferometrically, provided only that two atoms containing enough electrons to give a relatively large scattering effect are situated or can be substituted one at each end of the distance to be measured. From recent experiments of Mark and Wierl it seems to follow that in the case of cathode-rays the conditions for this kind of measurement are still more favourable than for the X-ray case.

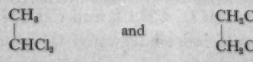
The above considerations were first applied to detect a difference in scattering between the two isomeric dichlormethylenes†, which in organic chemistry are represented by the two formulae



The interference maxima of the first and second order were photographed. As was to be expected from the chemical formula, the cis-compound (boiling at 60° C.) was the one for which the first interference maximum occurred at the larger angle, indicating the smaller distance between the two Cl atoms. A calculation similar to that discussed for CCl_4 yielded for the two distances in the two isomeric molecules

$$l_{cis} = 3{\cdot}6 \text{ A.U.} \quad \text{and} \quad l_{trans} = 4{\cdot}1 \text{ A.U.}$$

Another application of the principle was made for the two isomers ethylidenechloride and ethylenechloride:



Here also interference maxima of the first and second order were found. The first 1, 1-compound shows its first interference maximum at a larger angle than the second 1, 2-compound. The calculated distances between the Cl atoms are

$$l_{1,1} = 3{\cdot}4 \text{ A.U.}, \quad l_{1,2} = 4{\cdot}4 \text{ A.U.}$$

* R. Sänger, *Phys. Zeit.* **27**, 55 (1926). † P. Debye, *Phys. Zeit.* **31**, 142 (1930).

They agree with what is to be expected from the chemical formulae; for if the two Cl atoms are connected with the same C atom, we should expect the smaller distance. However, the 1, 2-compound calls for a more thorough discussion. In this case from the fact that no isomers of the cis-trans kind have been detected in organic chemistry, the conclusion has been drawn that the two parts of the molecule have the possibility of free rotation around the bond C–C. From the scattering experiment it seems that a really free rotation does not exist; for otherwise the gas would have contained a mixture of molecules with very different distances between the Cl atoms and the interference maxima could not have been so pronounced as they were. I have been inclined to draw the conclusion from the scattering experiments that an equilibrium position exists, which is similar to the structure of the trans-isomer in the case of the double bond. As to the finite dipole moment which has been observed for the 1, 2-compound* it can be explained by the thermal oscillations of the two parts of the molecule about their equilibrium position and around the C–C bond. The temperature variation of the moment which follows from this supposition seems indeed to exist†. Wierl has recently observed the scattering of cathode rays by the same molecules‡ and has also found the interference maxima mentioned above. In particular, from the fact that the maximum of the second order observed for the 1, 2-compound occurs at a smaller angle compared with the first than in a straightforward case like CCl_4, this author is led to believe that not one but two equilibrium positions exist, comparable in their structure with the two cis-trans-isomers. He explains the effect of the shift of the second maximum (which we also observed with X-rays) by a 50–50 per cent. superposition of two theoretical scattering curves for the two kinds of molecules. As in this calculation no account has so far been taken of the influence of the atomic scattering curve, neither of the two suppositions can be said to be proved. I should like, therefore, to postpone a definite conclusion. However, it is certain that very soon the discussion of the experiments on scattering will give a definite answer to the problem of free rotation.

The number of other questions being treated by the scattering method, with X-rays as well as cathode-rays, is by no means small. Although my account has been far from complete, I hope that I have been able to convince you that the new method is promising enough to deserve your attention.

* W. J. Williams, *Zeit. für phys. Chem.* A, **138**, 75 (1928).

† According to a remark of A. Eucken, about experiments carried out in his laboratory by L. Meyer, in *Molekulare Dipolmomente und ihre Bedeutung für die chemische Forschung, Fortschritte der Chemie, Phys. und phys. Chemie*, Bd. 20, Heft 5, S. 65.

‡ R. Wierl, *Phys. Zeit.* **31**, 366 (1930).

REVIEWS OF BOOKS

Differential Geometry of Three Dimensions, by C. E. WEATHERBURN. Vol. II. Pp. xii + 239. (Cambridge University Press.) 12*s.* 6*d.* net.

This volume is a continuation of the useful text-book, on the same subject, published by the author three years ago, the treatment being based on the systematic use of vector methods. Whatever views may be entertained as to the present-day effectiveness of these methods as an instrument of research, their conciseness and elegance frequently make strong appeal to students, and there is much to be said for their introduction at an early stage in the teaching of differential geometry. The new volume resumes and considerably expands the discussion of differential invariants begun in the first volume, and there are chapters on families of curves on a surface, families of surfaces, ruled and Weingarten surfaces, and curvilinear congruences; and two chapters deal with the deformation and application of surfaces. There is a short account of Levi-Civita parallelism. A large part of the book is derived from the author's own published papers, many sections of which are reproduced with little alteration other than a certain amount of rearrangement. This origin of so substantial a proportion of the matter of the book inevitably gives it a rather specialized character. It is written with great clearness and is provided with useful sets of examples, and the young geometer, as well as many other readers, will find it a handy means of getting rapidly into contact with an interesting range of recently published work.

G. S. L.

Einführung in die Wellenmechanik, von LOUIS DE BROGLIE. Pp. iv + 221. (Leipzig: Akademische Verlagsgesellschaft m.b.H.) Br. R.M. 11: Geb. R.M. 13.80.

This translation into German of a French manuscript of Prof. L. de Broglie presents the subject of wave mechanics as a development of classical and relativity theories, gives an account of some of the experimental work which the new ideas have inspired, and discusses the new views which arise out of the modern departures from the older theories.

The opening chapters are concerned with the classical mechanical and wave theories, especially with the principles of Hamilton and Fermat. These principles and their relation to one another are essential in the new theory and the reader will find the subject very concisely and clearly treated.

The modern atomic theory requires the physicist to be well grounded in the theory of wave motion. In the older quantum theory he was called upon to study the details of orbital motion, now he must be familiar with wave trains and wave groups. These subjects are treated in the early chapters and the reader will find here an introduction to them or a reminder of many points that it is easy to forget.

The experimental side is briefly treated in chapter VII, where the work of G. P. Thomson is examined in some detail and reference is made to the pioneer work of Davisson and Germer, the purpose being rather to bring forward experimental evidence for the value of the new principles than to treat the experimental side of the subject. There are several concrete examples of the application of these principles which are very illustrative and add to the ease with which the argument can be followed.

The introduction to the book contains a discussion of the various attempts made by the author and others to explain the significance of the wave function.

There are difficulties associated with each view put forward, but the reader will probably conclude that these are least if the interpretation put forward by Bohr and

Heisenberg be accepted. This view is that the function is a probability function. As a consequence of this point of view we are led to the uncertainty relations which are regarded as the expression of a physical principle of the first importance. Bohr regards it as essential to the whole structure of physics and as one which will enable us ultimately to rid ourselves of illogical conceptions which have grown up in the framework of the theory. We have not of necessity to banish altogether our physical models, though we may need a new design, and there are new workers who are so modest as to believe that their theories may merely remove the rust of the old ones and start them working again. But, whatever the outcome, the reader will find in this book a very attractive presentation of this important principle.

The difficulty of presenting in one volume the treatment by means of matrices as well as the other treatment is liable sometimes to lead to the view that there are two new theories. The introduction of matrices very briefly in the last chapter should help to remove this misconception.

The appearance of a book by the author of an important theory is bound to be an event of importance in the literature of the subject, and in this case the book is particularly welcome on account of the lucidity of the exposition.

X-Rays, by B. L. WORSNOP. Pp. ix + 102. (London: Methuen & Co.) 2*s*. 6*d*.

This series of handy volumes, which Dr Worsnop very ably edits, does, in the language of the prefaces, "fill a long-felt want." It is a pleasure to find writers who can, clearly and concisely, produce an account of some corner of modern physical science which shall be thorough, interesting, not too condensed, nor yet overloaded with detail. Dr Worsnop's volume on X-rays fulfils most of these conditions. In the ninety-five pages at his disposal he discourses admirably on early experiments, X-ray spectroscopy, scattering, emission and absorption, photo-electrons and direct reflection, refraction and diffraction. His book is to be commended, and our only criticism is that Dr Worsnop has perhaps not fully considered the difficulty of getting a quart into a pint pot.

A. F.

The Microscopical Examination of Coal, by CLARENCE A. SEYLER, B.Sc., F.I.C., assisted by W. J. EDWARDS, Department of Scientific and Industrial Research. Pp. vi + 67, with 17 plates. (London: H.M. Stationery Office.) 2*s*. 6*d*.

This is an account of the methods employed and the results obtained in a microscopical examination of coal, the aim of which is to determine the botanical nature of the original material and the nature of the physical and chemical changes which accompanied its transformation into coal, and to correlate this information with the coking and other properties of the surviving material.

First is given a method of preparing thin sections of coal for examination by transmitted light; then, the author has improved a method, based on metallographic practice, of polishing and etching flat surfaces of coal so as to bring out plant structures previously unrecognized; and there is a full description of a simple photomicrographic apparatus, particularly in its application to photography by vertical illumination. The excellence of the photographs testifies to the manipulative skill with which this work has been carried out.

The report is quite a good introduction to the study of the micropetrology of coal. It is informative and readable, and amazingly cheap.

M. T.

Recent Advances in Plant Physiology, by E. C. BARTON-WRIGHT, M.Sc. Pp. xii + 352. (London: J. & A. Churchill.) 12*s*. 6*d*.

Messrs J. & A. Churchill are publishing a series dealing with recent advances in various branches of modern science. There is a real need for such a series, the members of which shall treat their subjects adequately and in detail, with critical knowledge of and full references to original literature, but shall, at the same time, be produced at prices which do not put too great a strain upon the pocket of a student. So far, the series has been concerned with subjects mainly of medical interest, but Mr Barton-Wright's monograph makes an appeal to an audience whose interest is more purely scientific than that of the medical man.

Mr Barton-Wright, as was necessary if the volume was to be kept within moderate limits, makes but a selection from a large range of topics, and it so happens that his chapters include much that is of interest to the physicist. Soil and water relations, transpiration and allied subjects, carbon assimilation, nitrogen metabolism, respiration and the physiology of growth—these are Mr Barton-Wright's chapter headings, and serve to show the scope of his book. His treatment of these topics illustrates very clearly the change which is coming over those aspects of plant physiology which immediately concern the physicist. It is but a few years since the late Sir Francis Darwin remarked that the botanist concerned with a problem having physical bearings was wont to lose himself in a maze of suctions, pulls, osmotic pressure, capillarity, *vis a tergo*, tensile strengths and the like. This is still evident, but it is gradually disappearing, and we hear less and less of the very unphysical arguments which were once only too common.

Mr Barton-Wright gives a thoroughly interesting account of the transpiration problem, but it would have been better had he reminded his readers that some of the difficulties attendant on the use of the coefficient of relative transpiration—the ratio of the transpiration rate per unit area of the plant to the evaporation rate per unit area of some form of evaporimeter—might disappear if a certain purely physical fact were borne in mind, namely that the evaporation from a water surface of moderate dimensions is proportional not to the area A, but to $A^{3.4}$. Failure to recognize this fact has led to much confusion in the reduction of transpiration observations.

Mr Barton-Wright's book will prove of real service to students of botany, and physicists interested in the natural sciences will find many matters of interest in the chapters dealing with soil and water relations, transpiration and the physiology of growth—in particular there is room for much interesting work in the analysis of growth-curves (pp. 329 *et seqq.*), the study of which is exciting great interest at the present time.

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DISCUSSION ON MAGNETISM

FERROMAGNETISM AND HYSTERESIS

BY SIR ALFRED EWING, K.C.B., F.R.S.

ABSTRACT. The author reviews briefly his early work on hysteresis in the ferromagnetics, discusses his symbolic models of magnetic structure, and suggests that ferromagnetism affords the most promising source of clues to the outstanding problems of magnetic theory.

As a hopelessly old-fashioned magnetician, I find it an embarrassing compliment to be asked to open this discussion. You are taking down a dusty piece of apparatus from the upper shelf on which it was long ago put, and inviting it to function. It can do so only in pretty much the old way. I have nothing new to offer. Perhaps the best service I can render is to recall some aspects of the subject which were problems when I began to study magnetism fifty years ago, and are problems still.

My work dealt exclusively with the curious special group of substances we call ferromagnetics—iron, nickel, cobalt, and their alloys. To these we add a potential fourth member, manganese, because its alloys show ferromagnetic properties. Briefly, the distinguishing characteristics are (1) an immense readiness to be magnetized, (2) saturation, and (3) hysteresis.

Nowadays you are concerned with magnetism as a function of all kinds of matter. But I wish to emphasize the gulf that divides ferromagnetics from other substances. It is a difference in kind, not simply in degree. Of the distinguishing features none is more significant than hysteresis. In my own early experiments it forced itself on my attention at every turn. I became soaked in hysteresis, and was led to invent that name*, feeling the need of a word that should be sufficiently wide to include not only the phenomena of magnetic retentiveness, but other manifestations of what seemed to be essentially the same thing, though some of them were not associated with any visible magnetic change. I found, for example,

* *Proc. R.S.* **33**, 22 (1881); **34**, 39 (1882); **36**, 123 (1883); *Phil. Trans.* **176**, 524 (1885); **177**, 365 (1886).

that when a piece of iron or other ferromagnetic metal was subjected to a cycle of stress, as by hanging loads on a wire, and removing and reapplying them repeatedly, the quality of the metal went through a cycle of consequential changes, but irreversibly, being very different at corresponding points in the loading and unloading processes. This is true not only of magnetic quality but of other qualities as well; and it occurs with stresses that are far too small to produce permanent strain. The cyclic changes of stress appear to cause some kind of internal changes of structure, which are cyclic also but lag statically behind the changes of stress, exhibiting a sort of physical conservatism on the part of the metal—a tendency towards persistence of previous state.

That is only one example of the kind of phenomenon which the word hysteresis was intended to cover.

What is the physical cause of this curious lagging? Here is a problem which you have still to solve. From the fact that it occurs only in ferromagnetic solids one conjectures that it is closely associated with the effects of retentiveness which are observed in the process of magnetization.

The fact of saturation has long ago taught us to think of that process as an orienting of the elementary magnets first imagined by Weber. Each atom has its fixed place in the space-lattice, and we do not now think of the atom itself as turning, for in that case we should expect the cohesion of the substance to be affected, and we know it is not. I have tried the experiment of strongly magnetizing a piece of iron in a testing machine, while it carried a load which very nearly made it yield, and have found that repeated reversals of the magnetism did nothing to reduce the elastic resistance. We must rather think of something within each atom that can turn in response to an applied magnetic force, something which permanently possesses a magnetic moment sufficient to provide the saturation value when in every atom it is brought into parallelism with the applied force.

What controls its turning? When we begin to apply a magnetic force and gradually increase it we find three stages, always fairly distinct, and capable, under special conditions, of being very sharply separated from one another.

Interpreted as a turning of the Weber elementary magnets, the stages are, first, a small amount of deflection of a reversible kind in which the response to the applied field is slight and is proportional to the field, and there is no hysteresis; next, a big break away with irreversible tumbling into new positions of stable equilibrium, a process which involves much dissipation of energy. There may be more than one successive tumbling from one stable position to another on the way towards saturation. The last stage is again reversible; it is the final deflection from a stable position into complete parallelism with the applied force. In all this operation it is the tumbling from one position of stability to another that is the essence of hysteresis. It accounts for residual magnetism and for the expenditure of energy in a cycle of magnetization.

Forty years ago* I showed that all the characteristic features of the process could be very closely reproduced in a model made up of pivoted compass needles,

* *Proc. R.S.* **48**, 342 (1890).

regularly spaced like the atoms in a crystal lattice, and controlling one another simply by their mutual magnetic forces. Models have gone out of fashion now. My magnetic model was never more than a crude way of showing how hysteresis might be ascribed to the mutual action of parts of the interatomic magnetic system. More recently* I brought it a little nearer actuality, perhaps (though no atomic model can claim to be more than a piece of symbolism), by treating the Weber element as a part of each atom, capable of turning within the atom and controlled by magnetic interaction between itself and the other parts, which were regarded as fixed with respect to the neighbours in the lattice. Here again the control is imagined to depend upon magnetic forces, and we picture each Weber element as being able to respond to an externally applied field, first by a small reversible displacement, then by swinging violently into a new configuration in which it is again stable, but from which it can be displaced by a further increase in the field.

This notion, crude and vague though it is, seems to me still to point the way towards a clearing of our ideas about ferromagnetism. Shortly after it was first suggested it received an unexpected confirmation, of which I venture to remind you because it has escaped notice in most of the modern books. An acute critic, Mr James Swinburne†, took exception to my idea of magnetic control because it should imply the absence of loss of energy through hysteresis when a piece of iron has its magnetism reversed by the revolution of a very strong field, a field strong enough to secure saturation. For in that case the Weber elements would be held steadily pointing in the direction of the field and would have no opportunity of tumbling from one position of stability to another. It seemed most improbable that there should be so big a difference, in respect of dissipation of energy, between the reversal of magnetism which is brought about by the revolution of a constant strong field and that which is brought about by the reduction of the field to zero and its reapplication in the opposite sense. But soon afterwards it was discovered, through the experiments of Prof. Baily‡, that this difference does in fact exist. He measured the hysteresis loss caused by a revolving field, and found that for fields of moderate strength there was much loss, but that when the field was made stronger the loss passed through a maximum and rapidly fell almost to zero as the condition of saturation was approached. The whole action turned out to be just such as magnetic control of the Weber element would demand.

These reminders of the past will serve their purpose if they induce some of you, who are naturally engrossed by newer aspects of magnetic theory, to revert to a study of the ferromagnetic group. After all, it is there that we have the magnetism of the atom revealing itself on a relatively grand scale, there that we find an attractive tangle of conspicuous magnetic phenomena, and it is there, I think, that we may most profitably search for clues to what is now obscure.

* *Proc. R.S.E.* **42**, 97 (1922). † J. Swinburne, *Industries*, Sept. 19, 1890.
‡ F. G. Baily, *Phil. Trans.* A, **187**, 715 (1896).

MAGNETISM IN THE TWENTIETH CENTURY

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ABSTRACT. The state of magnetism at the beginning of the century is indicated and the work up to 1913, mainly that of Langevin and Weiss, is briefly described. The outlook during the quantum-principle period (1913–25) and after the reformulation of the quantum theory is sketched. Against this background the theoretical and experimental progress made in connection with dia-, para- and ferromagnetism is considered.

The diamagnetic susceptibility of atoms and ions is in good agreement with the charge-distribution indicated by wave mechanics; the susceptibilities of molecules give information as to their sizes, and of crystals as to the sizes and shapes of the molecules in the crystals.

Paramagnetic theory applies particularly to ions of the various transition series of elements. For the rare earths the observed magnetic moments agree with those corresponding to the ground states deduced by Hund's method. For the other series the susceptibilities support the view that the effective electrons (those in incomplete groups), being on the outside of the ions, take part in interaction with neighbouring ions.

Ferromagnetism is considered in the light of the molecular field hypothesis, and the following topics are discussed: spontaneous magnetization, the nature of the "carriers," specific heat, the Barkhausen effect, single crystals, Heisenberg's theory of the molecular field, ferromagnetic materials.

The importance of the work on strong magnetic fields is indicated, and the magnetic properties of the elements are briefly discussed.

§ 1. RANGE OF SUBJECT

In so far as magnetism can usefully be regarded as an autonomous branch of physics, it is mainly concerned with the magnetic properties of materials. The field aspect of magnetism is best treated as a part of electromagnetic theory, modified by relativity, or embodied in a more general relativity theory. The study of magnetic materials does, however, lead unavoidably to field theories, for the ultimate interpretation of magnetic properties is largely in terms of the magnetic properties of the electron, and the consideration of these necessarily demands some kind of relativistic quantum electrodynamics.

In its more limited aspect magnetism is concerned with the effect of magnetic fields on materials, as affecting primarily the intensity of magnetization, and secondarily optical (light emission and transmission), mechanical and electrical properties. From a magnetic point of view, substances may be divided into three classes. In paramagnetics the magnetic intensity is proportional to the field applied (except in high fields at low temperatures) and in the same direction; in diamagnetics it is in the opposite direction. The susceptibility—the ratio of the intensity to the applied field—of dia- and paramagnetics is usually very small compared with that of ferromagnetics, such as iron. Ferromagnetic substances differ from paramagnetics

in that under certain conditions they may be "spontaneously" magnetized in the absence of an applied field. The general character of the variation of the magnetization with the field is well known. Usually there are marked hysteresis effects. The three classes of materials differ further in the way in which the magnetic properties depend on the temperature. Above a certain critical temperature (the Curie temperature) ferromagnetics become paramagnetic.

Up to ten years or so ago, except for specialists, magnetism meant little more than the study of certain properties of iron. This was partly due to the difficulties in technique on the experimental side, and on the theoretical to the difficulties in interpretation of results: a susceptibility, as long as it is a mere susceptibility, can give little intellectual satisfaction. Yet for chemists, in the widest sense of this term, as well as for physicists, the magnetic properties of substances of all kinds are of the greatest interest and importance.

In recent years progress has been rapid, and the value of magnetic investigations has been much more fully realized. It is perhaps not too much to say that the study of the effects of magnetic fields on radiation from atoms in the Zeeman effect has proved the master key for the elucidation of the structure of atoms, and paved the way for the later developments of the quantum theory. The study of susceptibilities gives information about the sizes and shapes of molecules, and about the nature of ions in liquids and solids. Further work on the effects of magnetic fields, particularly the strong fields used by Kapitza, promises to shed light on some of the still baffling problems as to the nature of crystals, and on the characteristics of the types of interaction between the aggregating atoms.

In this paper a brief account will be given of the three main stages in the development of outlook during the present century, in its bearing on magnetism. The properties of dia-, para- and ferromagnetics will be considered, and some of the outstanding problems will be indicated. This introductory sketch is intended to serve as a background to the more special investigations which are described in other papers.

§ 2. THE THREE PERIODS

Progress under a cloud (1900–13). At the beginning of the century, owing to the work of Ewing and many others, much was known of the magnetic properties of the ferromagnetic elements, particularly iron. Curie's extensive investigations had led to the conclusion that in general the specific susceptibility of diamagnetics was approximately independent of temperature, while the susceptibility χ of paramagnetics decreased as the temperature T increased. For oxygen the susceptibility was found to be inversely proportional to the absolute temperature, and this was also approximately true for a number of paramagnetic solutions. The result is embodied in Curie's empirical law for paramagnetics

$$\chi = C/T.$$

The electron had been discovered, and attempts were being made to extend the electron theory, on the basis of classical conceptions, to cover a wider range of phenomena. The revolutionary theory of discontinuity of radiation had been put

forward by Planck, but in spite of the success of this quantum theory in certain directions, in connection for instance with temperature radiation and later with the photoelectric effect and specific heats, it was at first regarded rather as a desperate hypothesis, unlikely to have any ultimate validity. But it was realized that there were fundamental difficulties, for classical conceptions alone were unable to account for the properties, particularly radiational, of atoms built up of electrons, or even for their existence. Theoretical work on the structure and properties of atoms was therefore necessarily of a tentative and provisional character, carried out, so to speak, under a cloud of uncertainty.

In 1905 appeared Langevin's classical paper on dia- and paramagnetism. His investigations applied primarily to a gas, the molecules of which contained electrons rotating in orbits. The stability of such structures had to be assumed. An electron orbit, like a current circuit, behaves as a magnet. If the moments of the orbital magnets "balance out," the molecule as a whole will have no magnetic moment. The effect of a magnetic field is equivalent to the superposition of an angular velocity—the Larmor precession—and this gives rise to an intensity of magnetization oppositely directed to the field; the treatment is in essentials similar to that of Weber, who considered the hypothetical Ampèrian molecular current induced by an applied field. For the atomic susceptibility the expression obtained may be written

χ_{At}

$$\chi_{At} = -(e^2/4mc^2) \sum_n \overline{r_1^2},$$

r_1 where $\overline{r_1^2}$ is the time mean square of the radius of the resolved electronic orbit

n perpendicular to the field, the summation being taken over the n electrons in the atom.

μ When the molecule has a resultant magnetic moment μ there will be a second effect, usually much larger. The molecules will tend to orient themselves so that the magnetic axes lie in the field direction; if the conditions in the gas are such that a state of equilibrium can be attained (a question of some delicacy) statistical

$\bar{\mu}$ treatment shows that the mean resolved magnetic moment $\bar{\mu}$ will be given by

$$\bar{\mu}/\mu = \coth a - 1/a,$$

a where

$$a = \mu H/kT.$$

When $\mu H/kT$ is small—which means, in practice, in the case of paramagnetics, for any attainable fields except at low temperatures—the expression reduces to

$$\bar{\mu}/\mu = \mu H/3kT,$$

giving for the gram-molecular susceptibility

χ_M

$$\chi_M = \sigma_M^2/3RT,$$

σ_M where σ_M is the gram-molecular saturation moment, that is, the magnetic moment when all the molecular magnetic axes are parallel to each other.

Langevin's treatment thus accounts for the salient generalizations from Curie's work as to the variation with temperature of dia- and paramagnetic susceptibilities. It further shows how the results may be interpreted. Diamagnetic susceptibility

gives an indication of the size of the electronic orbits; from paramagnetic susceptibility the molecular magnetic moment may be calculated.

The next step was taken by Weiss. In ferromagnetics the magnetization in small applied fields is much larger than can be accounted for by the action of the external field alone. It is necessary to suppose that there is some mutual interaction between the molecules. Weiss suggested that there was a molecular field proportional to the intensity of magnetization, I, the total field H being given by I, H

$$H = H_e + NI, \qquad H_e, N$$

where H_e is the external field and N the Weiss molecular field constant. This is best regarded as a purely formal hypothesis, and as such its value in correlating a wide range of observed phenomena can hardly be overestimated. Although the molecular field is equivalent to a magnetic field in its effects, fields of the magnitudes required cannot be due to purely magnetic or to associated electrostatic interaction; and for this reason the hypothesis is perhaps better stated in the less committal form that there is internal energy ($-\frac{1}{2}NI^2$) associated with magnetization of intensity I.

In Langevin's theory the effect of any mutual molecular interaction due to orientation was neglected. With the assumption of Weiss the expression for the susceptibility of a paramagnetic becomes

$$\chi_M = \sigma_M{}^2/3R\,(T-\theta) = C_M/(T-\theta), \qquad C_M$$

where $\theta = N\rho\sigma_M{}^2/3MR$, ρ being the density and M the molecular weight. This θ, ρ, M
equation is an adequate expression of the variation of susceptibility with temperature observed for a large number of paramagnetics.

If N is positive there arises the possibility of spontaneous magnetization (in the absence of an external field) below a critical temperature θ (the Curie point), the value of which may be found by eliminating a between the equations

$$\sigma/\sigma_0 = \coth a - 1/a,$$

and

$$\sigma/\sigma_0 = a\,MRT/\sigma_M{}^2\rho N.$$

Normally ferromagnetics in bulk are not spontaneously magnetized. This fact is attributed by Weiss to the material being built up of domains (which may be perfect microcrystals) whose properties resemble those of elementary crystals of pyrrhotite. While the domains may each be spontaneously magnetized to the degree appropriate to the temperature, their directions of magnetization are distributed at random. The true spontaneous magnetization may be obtained by extrapolating to zero field the magnetization observed in large fields, where the variation is linear. The variation of the spontaneous magnetization with temperature found in this way is in good agreement with the equations; while the general character of the variation of magnetization with field in small fields can be qualitatively accounted for.

For paramagnetics the gram-molecular magnetic moment can be deduced from the measured susceptibility when its variation with temperature is known; for ferromagnetics the moment may be simply found from the spontaneous magnetization

at absolute zero (whose determination involves only slight extrapolation) when the magnetic "carriers" are parallel to each other. From the results of his own measurements, particularly on iron and nickel, and such others as were available, Weiss concluded that magnetic moments were all integral multiples of a definite unit, the Weiss magneton, the value of which has usually been taken as 1123·5 per gram-molecule*. For paramagnetics the atomic (or ionic or molecular) magnetic moment may be expressed as p Weiss magnetons, calculated from the formula

p

$$p = \frac{\sigma_M}{1123{\cdot}5} = \frac{1}{1123{\cdot}5}\sqrt{\{3R\chi_M(T-\theta)\}}$$
$$= 14{\cdot}07\, C_M^{\frac{1}{2}}.$$

σ_A From the saturation-moment σ_A per gram-atom of ferromagnetics the magneton value is

$$p = \sigma_A/1123{\cdot}5.$$

While there can be little doubt that this unit is not of fundamental significance it is a very convenient one, and for this reason will no doubt continue to be used; moreover, the idea that there was such a unit proved a most valuable incentive to further research.

In the period under review the names of Langevin and Weiss stand out. Langevin developed the theory for certain ideal cases. Weiss extended the theory and showed its wide application; he carried out pioneer work on ferromagnetic crystals; and he initiated lines of investigation which are still being actively pursued and continue to yield most valuable results†.

The quantum principle (1913–25). For magnetic theory, the quantization of angular momentum in Bohr's application of the quantum principle to the problem of atomic structure is of fundamental importance. If the angular momentum of an orbital electron can assume only values which are multiples of $h/2\pi$, a unit of magnetic moment is also provided, namely

μ_1

$$\mu_1 = eh/4\pi mc.$$

The value per gram-molecule (μ_1 multiplied by Avogadro's number) is 5593, just under 5 times the Weiss unit. Later, consideration of the Zeeman effect, in the light of the postulate that the frequency of radiation emitted is equal to the difference between the energies of the atom in its initial and final states divided by h, showed that an unexpected modification was necessary in Langevin's treatment of paramagnetism. Langevin had supposed that a magnetic carrier could assume any orientation in an applied field. Now the energy depends on the orientation, and the mere fact that in the Zeeman effect discrete lines are obtained shows that the magnetic moments of the atoms resolved in the direction of the field can only assume certain discrete values. There was much confusion in the earlier attempts at interpreting the Zeeman effect. It is unnecessary to retell that story in any

* The latest value is given as 1125·6.

† Some of these are described in his paper contributed to the present discussion.

detail. It must suffice to say that, as a result of experiment, generalizations and guesses, a scheme was eventually worked out which correlated the observations in a satisfactory or almost satisfactory manner. Chronologically the spatial quantization theory of Debye and Sommerfeld led to the remarkable experiments of Gerlach and Stern on the magnetic deviation of atomic rays. These provided direct evidence of "discrete orientation," and enabled the value of the Bohr magneton to be confirmed. From the Zeeman effect it was found that if the resolved angular momentum of the atom in Bohr units of $h/2\pi$ was m, the corresponding magnetic moment was not m, but mg (in Bohr units $eh/4\pi m_0 c$), where g is the Landé splitting factor—a function of the various quantum numbers defining the atomic state. An approach to a satisfactory interpretation of the g formula, and the clearing up of many other difficulties, was arrived at by the spinning-electron hypothesis of Goudsmit and Uhlenbeck—that the electron itself had an intrinsic spin $\frac{1}{2}h/2\pi$ ($\frac{1}{2}$ in Bohr units) and a magnetic moment $eh/4\pi mc$ (1 in Bohr units).

m

g

Schemes were evolved (Hund is largely responsible for the details) by which the quantum state of an atom could be worked out when the quantum numbers of the electrons in it were known. The resultant orbital moment l, the spin moment s, and the total resultant j can be found by combination in the appropriate manner of the quantum numbers of the constituent electrons. In the treatment of paramagnetism it is a complete knowledge of the quantum nature of the ground state of atoms (and ions and molecules) which is required. This will be considered in more detail later. Here only the character of the magnetic deviation experiments of Gerlach and Stern will be outlined. In these experiments a stream of atoms, delimited by slits, is passed between pole pieces which give a non-homogeneous magnetic field, and received on a plate, where a trace is formed. In the absence of a field the trace is a single line. When the field is applied traces are obtained which do, in fact, correspond to the possible mg values (that is the magnetic moments in the field direction) of the atom under investigation. Thus Cu, Ag, Au, Na, K, H, give a double trace corresponding to $mg = \pm 1$. In all cases where there are no disturbing effects the pattern observed is compatible with that anticipated from spectroscopic observations, and from the correlating scheme. This work gives perhaps the most vivid experimental demonstration of the necessity of a quantum interpretation of natural phenomena.

l

s, j

Reformulation (1925...). With the aid of the spinning-electron hypothesis the scheme built up was satisfactory, but only in a formal sense. Considering only the difficulties which bear on magnetic theory, we may say that atomic models, at the best, led to a g formula containing j^2, l^2, s^2 in place of $j(j+1)$, ... in the empirical formula. The orbital model of the atom was much more definite than the experimental facts warranted. Moreover there was fundamental inconsistency in that a quantum postulate and restriction were arbitrarily superposed on a system following, up to that point, classical dynamics. In Heisenberg's new quantum kinematics an attempt was made to formulate relations between quantities which were essentially observable. This was the starting point of the development in the

last few years of a self-consistent quantum dynamics approached from quite different points of view, but with equivalent results, in matrix and wave mechanics*. In some respects magnetic theory has undergone little modification. In so far as it was based on a formal scheme, much of the same treatment still applies, as in connection with quantum states of atoms, but the interpretation of the formal scheme is radically changed. In other directions, however, quantum dynamics has led to conclusions which have meant a fundamental advance in the understanding of certain magnetic phenomena.

A definite position and velocity cannot be assigned to an electron simultaneously. The electron orbits lose their definiteness. In an atom there is only a certain probability of an electron being in a given volume. Alternatively, but rather less rigorously, the electrons in an atom may be regarded as fused into a continuous distribution of space charge, which in its reaction to external fields can to a certain extent be treated *as if* it were a classical charge distribution.

The mathematical treatment of the fundamental wave equations leads naturally to the recognition of a new type of "interchange" interaction, which has been shown by Heitler and London to account for homopolar molecule formation; while Heisenberg has shown that it is capable of accounting for the effects correlated, but unexplained, by the molecular field hypothesis. In this connection a prominent part is played by the Pauli exclusion principle, which in its wave-mechanical no less than in its older formulation is one of the "stubborn facts" of nature.

In the hands of Dirac and others the spinning-electron hypothesis has lost much of its arbitrary nature, for it is shown to be formally necessitated by the combination of the wider principles of wave mechanics and relativity. But many difficulties remain, and an adequate quantum electrodynamics is only gradually being built up†.

This brief outline of the developments since 1913 will perhaps give a sufficient indication of the background against which more specific magnetic problems may be considered.

§3. DIAMAGNETISM. SIZES OF ATOMS AND MOLECULES

The diamagnetic susceptibility of a centrally symmetrical atom or ion is given by

$$\chi_{At} = -\frac{e^2}{6mc^2}\sum_n \overline{r^2}.$$

This expression will hold for the quantum orbital model. If the ion has a magnetic moment the paramagnetic effect will predominate, so the expression is of direct importance only for atoms or ions with no resultant moment (that is, in a $^1S_0$ quantum state). In dealing with experimental results on material in bulk, it is convenient to consider first those from which values for quasi-independent ions or atoms can be deduced with the minimum of uncertainty. The ideal substances are monatomic gases; but fairly definite conclusions can also be drawn from solutions of salts (containing definite ions) and from solid salts which are definitely ionic.

* See the paper contributed to this discussion by H. S. Allen.

† See the paper contributed by C. G. Darwin.

A number of accurate measurements of the susceptibilities of alkaline and alkaline earth halides are now available. (Ikenmeyer, Hocart, Reichender.) These give values for $\overline{r^2}$ compatible with other evidence. The susceptibility increases fairly regularly in series such as Na^+, K^+, Rb^+; F^-, Cl^-, Br^-. For ions with the same number of electrons the susceptibility decreases as the nuclear charge increases, as in Cl^-, K^+, Ca^{++}. Earlier measurements suggested that the inert gases were anomalous, but the measurements of Wills and Hector show that they fall into line, argon, for example, falling between Cl^- and K^+.

If the orbital model is replaced by the space charge model, it is possible to calculate very easily the equivalent value of $\overline{r^2}$ from the distributions for spherically symmetrical ions given by Hartree's self-consistent field method (Stoner). The agreement with experiment is good for the inert gases and positive ions, but for the negative ions there are considerable discrepancies which suggest that the charge in the outer parts of the ions is overestimated in Hartree's method.

It may be noted that the orbital model gave values which were much too large in cases where approximately complete calculations could be made, namely for He and H. It was shown by Van Vleck that the new mechanics led to satisfactory values. He has extended the treatment to the hydrogen molecule, and the agreement with experiment is very satisfactory.

For molecules generally, the complete calculation of the effect of a magnetic field obviously presents formidable difficulties. The diamagnetic susceptibility does, however, give an indication of the extent of the space charge distribution. The effect of double bonds and various kinds of linkages in organic molecules which is indicated by Pascal's extensive work, when interpreted in the light of modern views, seems capable of throwing valuable sidelights on problems of chemical constitution.

In crystals the problem is more complex. There is a regular arrangement of the positive centres of forces, and the distributed electronic charge linked with one or more of these may have a different effective area in different directions. The diamagnetic susceptibility will then have different values in different directions. Such directive effects have been known for some time; careful measurements have been made on a number of organic crystals (Raman, Bhagavantam) giving results of considerable interest.

The diamagnetism of a number of metallic elements is best considered separately, as in these the part played by free electrons has also to be taken into account.

Although the main lines of the theoretical treatment are clear, there are a number of points of detail to be cleared up. Experimentally there remains much that can usefully be done. To give simple examples, in addition to those indicated implicitly: there are very few accurate measurements available as to change of susceptibility with change of state or change of structure, or as to the difference in ionic susceptibility in solids and solution.

§4. PARAMAGNETISM. IONIC MAGNETIC MOMENTS

Atoms or ions containing incomplete groups of electrons will have a magnetic moment due to electron spin or orbital moments or both. In a free ion the spin and orbital moments give a resultant mechanical moment j; possible resolved moments (m) in the direction of an applied field are then $j, j-1, \ldots -j$. For an assembly of such ions, the theoretical value of the Weiss magneton number, p, calculated from the susceptibility as described above, is

$$p = 5g\sqrt{\{j(j+1)\}}.$$

This would apply ideally to the monatomic vapours of the alkali metals, but owing to the experimental difficulties it has so far only been confirmed as to order of magnitude for potassium (Gerlach). It has, however, been found to apply to the trivalent ions of the rare earths. In these it is the underlying N VI and VII groups ($n = 4$, $l = 3$) which are incomplete, and the moments deduced for the ions (from the susceptibilities of solid salts and solutions, measured by Cabrera, St Meyer and others) are, with one exception, in remarkable agreement with those corresponding to the spectroscopic ground states obtained by Hund's method. This is somewhat surprising, for it implies that the magnetic "carriers," even in the solid salts, are quasi-independent; and that their resolved moment in the field direction can change.

In accordance with the Bohr-Ladenburg view, paramagnetic properties appear in the salts of the various transition series of elements. Except for the rare earths, it is the $l = 2$ groups (which, when complete, have 10 electrons) which are involved. The first transition series has been studied most completely: this is the iron series, with from 18 to 28 electrons in the ions. There is considerable variability in the magneton values found for some of the ions, but for ions with the same numbers of electrons the values cluster closely round fairly well-defined means. These, however, do not correspond with those calculated with the j formula from the anticipated ground states of the ions; nor is the agreement greatly improved when account is taken of the effect of temperature in modifying the statistical distribution of the ions in different possible states (Laporte and Sommerfeld). It seems here as if the orbital moment l and spin moment s are best considered separately. A fair agreement between theory and experiment is obtained in a number of cases by supposing the spin moments only to be effective (Bose); practically all the cases are covered by supposing that the orbital moments also may be partly concerned (Stoner). The ions behave as though they were free with respect to the spin moment, but not with respect to the orbital moment. Qualitatively, many of the previous difficulties as to variability may thus be satisfactorily explained, but there remain many problems of detail. One of the most important of these is connected with the values found for θ in the expression $\chi = C/(T-\theta)$. The value of θ depends on the anion as well as the cation, being different in different salts; and although much valuable experimental work has been done, notably by Cabrera, the significance of such regularities and irregularities as have been observed is not clear. Heisenberg's

theory of the molecular field will undoubtedly prove the starting point for the solution of some of these problems.

In the ions of the first transition series, in contradistinction to those of the rare earths, the "magnetic" electrons are those in the outer group, and they presumably take part also in the interaction of a quasi-chemical character with neighbouring ions, whether in solids or solutions. It is for this reason that they do not behave as magnetically free. In the palladium and platinum families the ionic nature of the salts is much less pronounced; the magnetic moments found do not fit in with any of the usual theoretical expressions; and the variation with temperature is peculiar.

Molecules in general are diamagnetic; the outstanding exceptions are oxygen and nitric oxide. For these the moments correspond to the ground states of the molecules ($^3\Sigma$ for O_2, $^2\Pi$ for NO); the variation with temperature of the moment of NO predicted by Van Vleck has recently been confirmed (Bitter).

With respect to complex salts, many of which have recently been investigated by Welo, it must suffice to mention here that Sidgwick's conception of the "effective atomic number" of the central atom is a valuable aid in interpreting the magnetic results. To a first approximation the moments observed correspond to possible spin moments for ions with a number of electrons equal to the effective atomic number; but this is only a first step towards a satisfactory explanation of the magnetic properties of these salts.

§ 5. FERROMAGNETISM. PROBLEMS OF INTERACTION

Spontaneous magnetization. Ferromagnetism is a special case of paramagnetism. The value of the molecular field hypothesis lies in the fact that, in spite of its formal character, it serves as a trustworthy guide through the complexities of ferromagnetic behaviour. It is an essential consequence of this hypothesis that a substance containing magnetic carriers may be spontaneously magnetized below a certain critical temperature if the molecular field is positive; further, since an ordinary piece of a ferromagnetic is not necessarily spontaneously magnetized as a whole, the magnetization in one direction must be assumed to extend only throughout limited regions, or domains. An external field has the effect of making this spontaneous magnetization apparent, as the directions of magnetization in the domains tend to parallelism. By extrapolating to zero field the magnetization observed in high fields, the value of the spontaneous magnetization appropriate to the temperature (that is due to the molecular field alone) may be found. The variation of this magnetization with temperature has been investigated for a number of ferromagnetics. The most accurate results are those of Weiss and Forrer on nickel, and more recently on a number of ferromagnetics at low temperatures*. From the extrapolated magnetization at absolute zero, when the carriers will be parallel to each other, the magnetic moment per atom may be found; and from the character of the variation with temperature some interesting conclusions can be drawn.

* See the paper contributed to this discussion by P. Weiss.

The nature of the "carriers." In experiments on the gyromagnetic effect predicted by Richardson in 1908 the ratio of the magnetic to the mechanical moment of the magnetic carriers can be measured. For all the ferromagnetics which have been investigated, the ratio is found to be twice that for a classical "orbital" electron. From this the conclusion seems inevitable that it is the intrinsic spin moment of the electron that gives rise to the magnetic moment. This does not mean necessarily that the carriers are "free electrons." They may be free, or they may be electrons in atoms free to change their direction of spin; free, that is, to change their orientation in respect of the s, but not of any l moment.

The theoretical curve for the variation of magnetization with temperature when the carriers can only assume two orientations in a field* is quite distinct from that when the carriers can assume any orientation, as in the Langevin-Weiss theory. The experimental curves agree closely with the theoretical curves corresponding to electron spin. There are still, however, near absolute zero and near the Curie point slight discrepancies which remain to be explained.

Specific heat. The variation of specific heat and its sudden drop at the Curie point provide confirmation of the view that a ferromagnetic is spontaneously magnetized, and that the negative energy ($-\frac{1}{2}NI^2$) associated with the magnetization decreases more and more rapidly up to the Curie point and then disappears. If it is assumed that electron spin only is concerned, the number of effective electrons per atom can be estimated from the drop in the specific heat at the Curie point. Now, since the magnetic moment of the electron (1 Bohr magneton) is known, the number of effective electrons per atom can also be estimated from the low-temperature saturation-moment per atom. The numbers obtained in these two ways are in good agreement: 0·60 and 0·57 for nickel; 2·20 and 2·27 for iron.

The variation in the thermo-electric power of nickel against platinum with temperature near the Curie point has been measured by Dorfmann, Jaanus and Kikoin. They have interpreted their results as indicating that the magnetic electrons in nickel are also the conduction electrons. This raises a number of difficulties which, however, cannot be adequately discussed here†. At the Curie point there are also changes in volume and in elastic properties‡.

Above the Curie point. Above the Curie point, ferromagnetics become paramagnetic. The graph of $1/\chi$ against T gives a number of approximately straight lines. From these the values of p and θ are obtained in the usual way. The values of p, however, are such that the magnetic moment cannot be attributed to electron spin alone; in γ-iron, for example, about 10 electrons per atom would be required. At these temperatures the orbital moments must also play a part. This raises what seems to be a very baffling problem, which is part of the wider problem of the metallic state generally. While it is possible to account for the observed moments by assuming the presence of specific ions as "carriers," there is no independent

* Possible values of m when $s=\frac{1}{2}$ are $\pm\frac{1}{2}$.

† See the paper contributed to this discussion by W. Gerlach.

‡ See the papers contributed by F. C. Powell and L. F. Bates.

evidence for the existence of these in the metal. In some ways it might be preferable to treat the crystal as a whole; but this treatment does not at present show promise of accounting quantitatively for the values found for the susceptibilities.

The Barkhausen effect. Single crystals. In an ordinary magnetization curve, the steep part corresponds to the occurrence of irreversible changes of direction of magnetization throughout domains. The occurrence of discontinuities in the magnetization, when the applied field changes gradually and continuously, was first demonstrated by Barkhausen. In later investigations the number and magnitude of such changes have been observed. In some cases (as in a specially treated nickel wire investigated by Forrer) a single discontinuity may account for a great part of the whole hysteresis jump, but in general the discontinuities correspond to domains of small size, of the order of 0·1 mm.^{3}. The domains may be filament-shaped. It is not justifiable to identify the domains with grains or microcrystals.

Much progress has recently been made in the investigation of the magnetic properties of single crystals of ferromagnetic metals. These are not so simple as might have been anticipated, but this fact is partly due to the large effect produced by small quantities of impurities, and by even very slight mechanical distortion. In cubic iron crystals (Webster, Gerlach) there are slight directional effects, magnetization occurring most readily along the tetragonal axis; but the differences are small except in the region just below saturation. These directional properties correspond to internal fields of the order of a few hundred gauss, and can be accounted for, at least qualitatively, as arising from the classical magnetic interaction of elementary magnets placed at the lattice points (Peddie, Mahajani). The initial permeability is very high and the remanence low, though there is some uncertainty in the determination of the true remanence. Gerlach suggests that a single crystal may not consist of a single domain; but the state of things might possibly be due to the unavoidable imperfections in the crystals. The magnetostriction results on single crystals are simpler and more significant than those on ordinary material.

Heisenberg's theory. The classical and classical-quantum theories could give no satisfactory explanation of molecular field phenomena. Heisenberg has now shown how these may arise from a type of interaction characteristic of quantum mechanics. Consider an idealized ferromagnetic consisting of an aggregate of atoms each containing one electron free to change its orientation, the remainder of the atom having no magnetic effect. Each atom can interact with its neighbours. For two hydrogen atoms at a given distance apart there are two possible energy states, in one of which the electrons are parallel and in the other anti-parallel. The difference in energy between these two states far exceeds that due to the purely magnetic interaction. Similarly, in the idealized ferromagnetic, the energy will depend indirectly on the mutual orientation of the electrons in neighbouring atoms. The energy must be assumed to be less when the electrons are parallel. (For hydrogen, the anti-parallel state has least energy and corresponds to molecule formation.) A complicated statistical treatment is applied to the crystal as a whole, and expressions

J_0, z

are obtained for the Curie temperature and the variation of magnetization with temperature, in terms of the interchange interaction J_0 and the number z of neighbours of each atom. As a first approximation, the expressions obtained agree with those from the Weiss theory (modified for the 1-Bohr-magneton case) and so are in agreement with experiment; the detailed expressions, however, are unsatisfactory and indicate that some modification in the treatment is necessary. Heisenberg's idea represents a very great advance, for molecular field phenomena are now embraced within the scope of the general theory.

Ferromagnetic materials. The variation of the magnetization of ferromagnetics in relatively small fields has not been considered here. The general nature of the effects is well known, but the variety in detail in the behaviour of different materials is so wide as to make a brief summary practically impossible. The properties in question are of the highest technological importance, and mention can only be made here of recent progress in the production of materials having characteristics which are desirable for different purposes, such as iron-nickel alloys, particularly permalloy with very high initial permeability, ferrocobalt with a saturation magnetization greater than that of iron, and cobalt steels with high coercivity combined with high remanence. While it may be known how the presence of impurities and thermal or mechanical treatment may affect the magnetic properties of a particular material, so many factors are involved that it is in general by no means clear why such effects are produced. An explanation of the properties of a complex ferromagnetic material can hardly be given when there are still difficulties in interpreting the properties of, say, a relatively simple iron crystal. The general problem, which involves metallurgy and crystallography, is one of the most interesting in magnetism. From a theoretical point of view the study of the simpler binary alloys, with one or both constituents ferromagnetic, and in particular the determination of the variation of Curie point and saturation moment with composition, shows promise of yielding results of great value.

§ 6. MISCELLANEOUS

Strong fields. There remain a number of topics which should at least be briefly mentioned in this introduction. One of these, which would require fuller treatment were it not to be specially discussed, is the study of the effect of very strong magnetic fields by the method developed by Kapitza. The importance of these investigations lies in the fact that the fields used, approaching half a million gauss, are of the same order of magnitude as the pseudo-magnetic fields, arising possibly from some kind of interchange interaction, already existing in solids. The measurement of the change of electrical resistance in such fields has led to entirely unexpected results of great importance for the theory of conductivity. The method seems to be the most powerful one available for the experimental investigation of outstanding problems in connection with interatomic forces and crystal structure*.

* See the paper contributed to this discussion by P. Kapitza.

Susceptibilities of elements. In discussing dia- and paramagnetism a somewhat arbitrary selection of materials was considered, namely those which were "normal." Among the elements, however, there are many which are not normal, in that they may have a diamagnetic susceptibility varying with temperature, or a paramagnetic susceptibility which varies very little. Among the paramagnetics the susceptibility may be very small, so that its origin must be in some way different from that of normal susceptibility. If there are free electrons with a magnetic moment in metals these might be expected to give rise to strong paramagnetism. It was shown, however, by Pauli, by the application of the Fermi statistics, that the electrons will normally balance magnetically in pairs, and that when a field is applied the magnetization produced will be small; moreover, that the susceptibility will vary little with temperature. This treatment accounts qualitatively for the susceptibilities of the alkali metals, and removes one of the outstanding difficulties in connection with the magnetic properties of the elements. But many others remain. It should also be noted that the free-electron explanation cannot apply to other substances, such as potassium bichromate, which have a small constant paramagnetism. The recent progress in formulating more satisfactory theories of electrical conductivity, and the new facts brought to light by Kapitza's investigation of the effects of strong fields on a wide range of elements, will give an entirely new interest to the study of the susceptibilities of the metallic elements.

MAGNETISM AND THE QUANTUM THEORY

By H. S. ALLEN, M.A., D.Sc.,

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ABSTRACT. The results of the modern form of the quantum theory are reviewed in relation to the explanation which they afford of magnetic phenomena. Reference is made to the magnetic moment of the spinning electron which appears to play an important part in the new quantum mechanics, and to the explanation of the molecular field of Weiss as originating in the quantum interaction between electrons. The necessity for taking into account the principle of relativity is emphasized in connection with the possible structure of the electromagnetic field: the quantum theory may demand four-dimensional tubes of force in space-time. Heisenberg's principle of indeterminacy is briefly discussed.

§ 1. THE NEW QUANTUM MECHANICS

FRESH light has been thrown on the problems of atomic magnetism by the new quantum mechanics associated with the names of Heisenberg and Schrödinger. Although the subject was approached by entirely different routes the matrix mechanics and the wave mechanics form a self-consistent scheme, and from the mathematical standpoint may be regarded as equivalent. There is a remarkable, and even puzzling, difference between the starting point of the matrix mechanics and that of the wave mechanics. The former, like the earlier quantum theory, is based on discontinuities such as stationary states and quantum transitions. Schrödinger's work is based on the ideas of continuity and causality familiar in classical physics. While Schrödinger himself sought to interpret the field scalar, ψ, of his fundamental equation in terms of the density of the electrical charge, Born* interpreted it as a probability in the statistical sense. There seems to be little question that the statistical interpretation lies closer to the real truth, and this is the view emphasized by Condon and Morse in their recent book on *Quantum Mechanics*. The question of being able to establish agreement between the electromagnetic theory and the new wave mechanics has been discussed also by Bateman† and by Louis de Broglie‡. "It is possible that the classical values of the potentials and of the fields may be only the mean values of the real magnitudes."

It is true that the wave picture of the atom becomes less definite than that of Bohr, in which planetary electrons circulate about a massive nucleus in definite orbits, but where it has been possible to institute exact comparison the results are in every case favourable to the new model. The hydrogen atom may be regarded as a nucleus surrounded by a distribution of space charge determined by the quantum

* M. Born, *Zeit. für Phys.* **37**, 863 (1926); **38**, 803 (1926).

† H. Bateman, *Nature*, **118**, 839 (1926).

‡ Prince Louis de Broglie, *Comptes Rendus*, **184**, 81 (1927).

numbers, which emerge in a natural way from the equations. An alternative treatment, which has been favourably received by many theorists, regards the hydrogen atom as a centre of force surrounded by a field of probability, namely the probability of the electric charge which constitutes the negative electron being found within any assigned region. In this case the quantum numbers define the characteristics of the probability field. It is necessary to emphasize the fact that it is only in dealing with the one-electron problem that space of three dimensions can be employed in wave mechanics. In the general case of several electrified particles it is necessary to employ spaces of many dimensions, the number of dimensions corresponding to the number of degrees of freedom of the particles.

For each electron in an atom it is found that there is a total quantum number n and a subsidiary quantum number l which is less by unity than the azimuthal quantum number k of the orbital picture, so that $l = k - 1$. Also to each electron there must be assigned a quantum number s, defining the spin of the electron. For a single electron $s = \pm \frac{1}{2}$, so that the angular momentum of spin is $\frac{1}{2}(h/2\pi)$, where $h/2\pi$ is the quantum unit of angular momentum. The spinning electron was invoked to account for certain discrepancies between theory and experiment, the so-called "duplexity" phenomena, the observed number of stationary states for an electron in an atom being twice the number given by theory. Dirac* has shown that the earlier treatment employing the new quantum mechanics was incomplete in so far as it was not in agreement with relativity, or, alternatively, with the general transformation theory of quantum mechanics. His investigation, which satisfies the requirements of both relativity and the general transformation theory, leads to an explanation of all duplexity phenomena without further assumption. "All the same there is a great deal of truth in the spinning electron model, at least as a first approximation."

In a recent paper on the interaction of electric charges Eddington† has investigated the angular momentum of the electron, and finds that this is made up of two parts: "the second term is the angular momentum recognized in the older quantum theory; the first term is the 'spin of the electron' which has to be added to the orbital momentum. It will be seen that the so-called spin is deeply rooted in the matrix theory, being inherent in the transformation properties of the vectors and independent of the physical entities (mass, positive or negative electric charges) that may ultimately be located at the point (x). We obtain it purely from relativity considerations without introducing dynamical conceptions or even a wave-equation."

Associated with the mechanical moment there is in each case a magnetic moment. The Bohr magneton, $eh/(4\pi m_0 c)$, represents the magnetic moment due to the motion of an electron of mass m_0 in an orbit with angular momentum $h/2\pi$. For the intrinsic spin of the electron, the ratio of the magnetic to the mechanical moment is double that for the orbital moment, so that the magnetic moment of the spinning electron is one Bohr magneton.

* P. A. M. Dirac, *Proc. R.S.* A, **117**, 610 (1928).

† A. S. Eddington, *Proc. R.S.* A, **126**, 696 (1930).

The total angular momentum of an atom is to be found by taking the vector sum of the spin and orbital moments. This resultant is represented by the quantum number j.

j

In a magnetic field a single lj energy level separates into a number of component levels. They can be specified by a magnetic quantum number m, which may be regarded as the resolved value of j in the direction of the field. This magnetic quantum number can take the values $j(j-1)(j-2), \ldots -j$. The resolved magnetic moment of the atom expressed in Bohr units is given by mg, where g is the so-called "splitting factor" of Landé. The new quantum mechanics gives for g the formula

m

g

$$g = 1 + \frac{j(j+1) + s(s+1) - l(l+1)}{2j(j+1)},$$

in agreement with the results deduced from observations on the Zeeman effect.

Results of very great interest from both the theoretical and the practical standpoints have been reached by the attribution of angular momentum to the nucleus of the atom. The results are more especially striking in the case of hydrogen, for which an analysis of the band spectrum indicates an angular momentum of one half a quantum unit. The outcome of work on the specific heat of hydrogen has ultimately led to the experimental proof of the existence of two kinds of hydrogen molecules, in one of which the wave functions are symmetrical in the rotational wave function and in the other anti-symmetrical. The nuclear magnetic moment corresponding to nuclear spin must in general be very small. This conclusion is interesting in connection with an attempt made by the writer several years ago to investigate the effect on the spectrum of the magnetic field of the core of an atom.

The structure of the spectrum of helium for a long time eluded interpretation on the basis of the quantum theory, but Heisenberg was able to give an explanation based on the mutual interaction of the electrons in the atom. There is close correspondence between this problem and a classical resonance problem, and it may be said that Heisenberg's theory depends on a new type of quantum resonance. Another way of expressing the theory is to say that as there is nothing to distinguish between the two electrons, the effective result must be the same as if they were interchanged.

The problem of the formation of molecules on the basis of wave mechanics has been attacked by Heitler and London*. When two neutral atoms, say two atoms of hydrogen, combine to form a molecule, the homopolar compound produced is due to resonance phenomena which occur in the Schrödinger waves. The calculated value of the energy of combination of the hydrogen atoms was found to be in agreement with the empirical value.

Heisenberg† has advanced a theory of ferromagnetism which explains the molecular field of Weiss as originating in "the exchange degeneracy of the electrons in the different electronic systems of the lattice." His theory has been amplified by Fowler and Kapitza‡, who have shown how it accounts in a natural way for the large change of specific heat in passage through the Curie point, the relatively large change

* W. Heitler and F. London, *Zeit. für Phys.* **44**, 455 (1927).

† W. Heisenberg, *Zeit. für Phys.* **49**, 619 (1928).

‡ R. H. Fowler and P. Kapitza, *Proc. R.S.* A, **124**, 1 (1929).

of volume at the same point, and the phenomena of magneto-striction. These effects are far too large to be explained by magnetic forces, but the exchange effects of Heisenberg's theory are adequate to give at least a qualitative account of them. "The magnetization compels a new selection from the various possible energies of interaction, and this results in comparatively large changes in the forces of attraction and repulsion at given distances, and so in changes of size in the magnetized crystal."

A concise account of this quantum interaction theory of the molecular field may be found in E. C. Stoner's recent book*, which forms a valuable summary of modern work based on the quantum theory.

§ 2. LINES AND TUBES OF FORCE

Let us picture an imaginary experiment in which two observers are in uniform motion relatively to one another. The first is provided with a charged body delicately suspended; the second with a suspended magnetic pole (or, if objection be taken to the conception of an isolated pole, with a suspended magnet). The first observer on noticing a deflection of his charged particle concludes that it is acted on by an electric field, the second observer supposes that his magnetic pole is acted on by a magnetic field. Thus the field which one observer regards as electric, the other regards as magnetic. Which is right? According to the principles of the theory of relativity it is impossible to say that one is more right than the other. "How can the same body both give and not give a magnetic field?" asks Eddington†, and he supplies the answer: "On the classical theory we should have had to explain one of the results as an illusion. On the relativity theory both results are accepted. Magnetic fields are relative."

If our two observers adopt the view that a field implies the existence of lines of force, the first observer will postulate the existence of lines of electric force, the second will conclude that his magnetic pole is affected by lines of magnetic force.

It is well known that Faraday attached great importance to the conception of lines of force, and emphasized the physical nature of such lines. This remark applies both to magnetic and also to electric lines of force. Quotations might be multiplied to illustrate his insistence on the view that lines of force should be regarded as, in some sense, physical realities. Later investigators stressed the importance, sometimes of one set of lines, sometimes of the other, and it might almost be said that there was a conflict as to which was the more fundamental. As in so many other cases the conflict ended in the decision that both views were right, or that both sets of lines should be regarded as particular cases of a more general conception. In the four-dimensional world of Minkowski, as E. T. Whittaker has shown, we have to consider a covariant family of surfaces, which may be called tubes of force in the electromagnetic field or *calamoids*. The Faraday electric and magnetic tubes are not distinct and rival things, but two limiting cases of the same thing. In fact the words of Minkowski with regard to space and time may be adapted so as to refer to the

* E. C. Stoner, *Magnetism* (Methuen, 1930).

† A. S. Eddington, *The Nature of the Physical World*, p. 22 (Cambridge, 1929).

lines of force of classical theory, and we may say: "Henceforth electric lines in themselves and magnetic lines in themselves shall sink to mere shadows, and only a union of the two shall preserve an independent existence."

The present writer*, following S. B. McLaren, has drawn attention to the relation between lines of electric and magnetic force in three dimensions and the quantum constant. The angular momentum of McLaren's magneton or of the ring electron in its unitary condition is given by $(1/2\pi)\,N_e N_m$, where N_e is the corresponding number of tubes of electric induction, N_m the number of tubes of magnetic induction. Taking $h/2\pi$ as the quantum unit of angular momentum we find, on equating the two expressions,

$$N_e N_m = h.$$

§ 3. TUBES OF FORCE AND WAVE MECHANICS

It may be suggested that there is a connection between the hypothesis of quantum magnetic tubes and the wave mechanics of Schrödinger. Each quantum tube of magnetic induction, defined by the ratio of Planck's constant to the electron charge, is associated with an amount of energy $h\nu$, where ν is a frequency characteristic of the tube. The condition of the tube was formerly pictured as an internal spin or vorticity, but this representation may require some modification to suit the new wave model.

Schrödinger† points out that, on his view, in the unperturbed normal state of a conservative system either the distribution of electricity is constant in time or there is a stationary current distribution. "We may in a certain sense speak of a return to electrostatic and magnetostatic atomic models." Consider, for example, the rotator with fixed axis‡, in which a charged particle of mass m_0 moves in a circle of radius a. The allowed energy levels E_n are given by

$$E_n = \frac{h^2}{8\pi^2 m_0 a^2}\, n^2 \qquad n = 0, 1, 2 \ldots,$$

and the charge density (charge per unit angle) comes out to be simply $e/2\pi$, that is, the charge functions as though it were distributed uniformly over the circle. The magnitude of the current associated with the nth state is

$$\frac{eh}{2\pi m_0} \cdot \frac{n}{2\pi a^2}.$$

The external effect of this current distribution is a magnetic field equivalent to that due to a small magnet of magnetic moment $n\,\dfrac{eh}{4\pi m_0 c}$ in electromagnetic units, that is, n Bohr magnetons.

It is interesting to notice that this result is exactly analogous to that for the "ring electron" which formed the subject of a discussion at a meeting of the Physical

* H. S. Allen, *The Quantum and its Interpretation* (Methuen, 1928).
† E. Schrödinger, *Wave Mechanics*, p. 123 (Blackie, 1928).
‡ E. U. Condon and P. M. Morse, *Quantum Mechanics*, p. 96 (McGraw-Hill, 1929).

Society* and we may apply to this model the conception of quantum magnetic tubes which the author has employed in connection with the ring electron.

We thus find $$N_e N_m = nh,$$

where $N_e = e$ and is the number of tubes of electric induction, while N_m is the number of tubes of magnetic induction, associated with the model. This result points to the existence of discrete tubes of magnetic induction, the unit tube being defined as h/e. N_e, N_m

It would seem probable† that this result applies also to the general case of central-force motions as in the hydrogen atom. Fermi‡ has shown that by adopting Schrödinger's interpretation of the field scalar the correct expression for the magnetic moment of a hydrogen-like atom may be deduced. He employs the expression for ψ found by Fock and then integrates the wave equation. It appears that the component magnetic moment in the direction of the field is equal to the product of the Bohr magneton and the magnetic quantum number. In this problem the lines of current are circles situated in planes perpendicular to the axis, with their centres on the axis.

§4. THE PRINCIPLE OF INDETERMINACY

Heisenberg§ has formulated a relation which imposes a limit on the maximum accuracy with which simultaneous measurements of the position and momentum of a particle can be made. This principle has also been discussed by Dirac and by Bohr.

If p and q denote two conjugate quantities in the Hamiltonian sense, for example the momentum and corresponding coordinate of a particle, then there will always be in the simultaneous observation of these quantities an uncertainty given by the rule that $\Delta q . \Delta p$ is of the order of h (Planck's constant). Thus if we seek to determine with great accuracy the position of a particle, it becomes difficult to determine at the same time its velocity or momentum. Greater accuracy in the determination of position is compensated by a greater inaccuracy in the measurement of momentum. If, on the other hand, we strive to measure the momentum more accurately, we can only do so at the expense of the determination of position.

There seems to be something analogous to this in the relation $N_m N_e = h$, which has been considered in the preceding sections. We may associate the determination of the position of an electron with the determination of the electric lines of force which constitute its electric field, and we may associate the determination of its momentum with the determination of the magnetic lines of force which arise from its motion. The product of the two quantities N_m and N_e remains constant in a given state, and represents an integral multiple of the quantum of action.

It may be pressing a formal resemblance too far to suggest that Heisenberg's

* *Proc. Phys. Soc.* **31**, 49 (1919).

† This suggestion has now been verified. One of my research students, Mr G. B. B. M. Sutherland, has found that the result holds good for some special cases involving small quantum numbers, and Dr G. Temple has been successful in working out the general case. His paper on the subject will be found in the open part of this discussion. H. S. A. May 20, 1930.

‡ E. Fermi, *Nature*, **118**, 876 (1926).

§ W. Heisenberg, *Zeit. für Phys.* **43**, 172 (1927).

uncertainty relation, which is concerned with *errors* in the determination of p and q, may be associated with the difficulty, referred to in § 2, of distinguishing between the existence of an electric and that of a magnetic field. But if there is any physical significance in the resemblance, it points to the necessity of considering the uncertainty relation in the light of the theory of relativity.

Broadly speaking, there seems to be a necessity to interpret this relation in connection with a universe of four, or even of five dimensions, as is indicated at the end of Flint's *Wave Mechanics** in the discussion on the physical significance of the wave equation and the wave function.

* H. T. Flint, *Wave Mechanics*, p. 89 (Methuen, 1929).

THE POLARIZATION OF THE ELECTRON

By Prof. C. G. DARWIN, F.R.S.

ABSTRACT. A short account is given of the discovery of the spinning electron and of its analogies with and distinctions from polarized light. The question of the direct observation of the magnetism of the free electron is reviewed from the theoretical standpoint, and a comparison is made with the results of recent experiments.

§ 1. THE ORIGIN OF THE THEORY

The "spinning electron" was invented in 1925 and by now, as far as we may judge, its properties are fairly completely known. I shall therefore have nothing new to say on fundamentals, but shall chiefly discuss some minor points of interest. The starting point of the whole theory was of course the mysterious fact that every spectrum gave twice as many levels as it should. This doubling was explained in the old quantum theory by the idea of the spinning electron, a convenient term to describe the quality of possessing a definite angular momentum and magnetic moment as well as charge. The model had certain rather unnatural features, and needed some doctoring to make it work, but we need not criticize these as the interest in them has disappeared.

Turning to the wave mechanics we may make use of the picture of the atom drawn by de Broglie and Schrödinger, provided that we remember very emphatically that it answers only half the question, and is to be supplemented by rules of "interpretation" as to what will be observed. With this limitation we make a rough picture of the energy levels of the atom by imagining it to be a sphere of glass of variable consistency with refractive index increasing strongly towards the centre. A beam of light in the glass is bent strongly round the centre, and may be unable to escape. As it goes its distribution will of course alter and the natural analysis is into the normal modes of harmonic vibration, each of which corresponds to one of the energy levels. But now suppose that we look a little more closely and allow for the polarization of the light. Mathematicians have wisely refrained from the troublesome study of the passage of light in media of variable refractive index, but it is easy to show that even isotropic media will produce double refraction—the unequal reflections at an interface are really a special case of this. The double refraction splits each normal mode into two, in which the frequencies and regions of disturbance are slightly different. Now the doubling of the number of levels was the cardinal difficulty of the older theory, so that as soon as Schrödinger's theory was discovered, it suggested that it would be worth while to see whether this doubling could not be produced by something analogous to the polarization of light.

This was the idea that led me to attack the problem in 1926. It was not of course to be expected that the electron waves would be exactly like the electromagnetic, since for the electron the two polarized components are anti-parallel instead of perpendicular. The attempt to invent a suitable wave system by the use of the machinery of tensors met with no success, but finally it proved possible to form a pair of simultaneous differential equations which yielded the hydrogen levels approximately. The equations had one defect, in that the S-levels came in the wrong places; subsequent history showed that a very little more ingenuity would have put them right, and this in a way that would have been most suggestive for further progress. The equations were quite asymmetrical in appearance and it was even surprising that in fact they were independent of the choice of the axes x, y, z. Now relativity theory is concerned with systems independent of the choice of axes, and from this condition develops the conception of tensors apparently as a matter of necessity, and so it seemed clear that the equations ought to be put in tensor form. This I therefore did, but wrongly as subsequent events proved—we return to this later. My work had been empirical, but Pauli showed how the equations could be derived from the dynamical principles of the quantum theory by means of the spinning electron. He of course got the S-levels wrong too, and we can therefore say that the electron has dynamical qualities definitely not represented by the idea of spin. It is for this reason that I prefer to speak of the "polarized electron," for the word "polarization" is already used in several different senses, and one more will not add to the confusion.

§ 2. DIRAC'S EQUATIONS

The important step in the theory was made by Dirac by means of his principle that the equations must be of the first order in t, and so, to conform to relativity, in x, y, z also. The consequence is a system of four equations, or alternatively a single equation involving four-rowed matrices. The matrices can be chosen in many ways, so that the four equations can be put in a large variety of forms, but all are hopelessly asymmetrical, so that no inspection, but only detailed testing, can verify that they are independent of the choice of axes. Many attempts have been made to overcome this trouble by expressing them in tensor form, but on the whole I think it must be said that they are not of much use. To get the tensor form the four equations must be replaced by sixteen, relating together certain invariants, vectors and tensors of second rank. Moreover, the observable properties depend only on four quantities, so that we can never determine unique values for the sixteen. The fact is that six years ago Weyl showed that there were quantities, other than vectors, which possess the property of invariance for orthogonal changes of axes. They may roughly be described as half-order tensors; for when the axes are rotated through an angle, they change in a way that depends on half that angle. Dirac's variables are of this character. The property can only exist for orthogonal transformations of the coordinates, and if we demand the full gravitational theory of relativity we must use the sixteen equations, as has been done by several writers. If the present theory

were final, we should certainly be forced to do this; but I have a very strong impression that the whole thing will ultimately take a new form, and that for the present it is best to put up with the asymmetry of the four equations, in spite of the intolerable amount of algebra to which it leads in any but the most general theorems.

§ 3. THE MAGNETIC MOMENT

Though the four ψ's cannot conveniently be thrown into vector form, there are quadratic functions of them which can, and as the observed quantities all depend on these we must consider them. The chief of them is the current density $\psi^*\alpha_1\psi$ (where α_1 is one of Dirac's four-rowed matrices) with time component $\psi^*\psi$ for the density. There are also others which one can construct by writing down expressions such as $\psi^*\alpha_1\alpha_2\psi$ and $\psi^*\alpha_1\alpha_2\alpha_3\psi$, and finding how they transform for changes of axes. The complete set is two invariants, two four-vectors of which one is the current, and one six-vector. I overlooked the existence of the second four-vector in my paper on the subject. Its physical meaning is most easily seen if a wave-packet be constructed for an electron moving freely. The vector is closely related to the density of "null-moment," that is, of the magnetic moment as it would appear to an observer moving with the packet; its time component is the component of null-moment in the direction of the motion. The six-vector obeys the same rules of transformation as do electric and magnetic forces. If the electron is reduced to rest, three components vanish and the others give the magnetic moment. Now a moving magnet certainly gives rise to electric forces and so can be said to have an electric moment—this idea was developed long ago by Frenkel in connection with the old spinning electron—so it is the natural interpretation to regard the six-vector as the actual magnetic and electric moments of the moving electron. It appears that for a free electron the electric moment is always perpendicular to both the magnetic moment and to the direction of motion, and that the magnetic moment differs from the null-moment in that the component perpendicular to the direction of motion is increased in the ratio $1/(1 - v^2/c^2)^{\frac{1}{2}}$.

There are certain questions about this, however, which are not quite clear, for Dirac found no electric moment except an imaginary one, and it is not very evident what that means. I am inclined to think that the discrepancy is only a matter of definition. Dirac was discussing the action of a force on an electron, whereas here we have the force exerted by the electron on a set of distant electrometers. The two phenomena are of course related by dynamics in a perfectly precise way, but nevertheless the chosen interpretations may be different. If for example a non-uniform electric force cannot split a stream of electrons into two, we might say that they have no electric moment, whereas there might still be, in the distant electric field, inequalities which could be attributed to an electric moment. This suggestion is only tentative, and perhaps the matter ought to be cleared up. It would I think be mainly a question of classical electrodynamics, but would involve rather troublesome considerations of the electric fields of moving media.

§ 4. THE DETECTION OF THE MOMENT OF FREE ELECTRONS

It is interesting to consider what are the prospects of directly observing the magnetic moment of the electron. Both the general wave theory and the magnetic moment were first discovered as matters of indirect inference from the theory of spectra, but the first was soon verified for free electrons by experiments on their diffraction. The magnetic moment of the electron has of course been verified by the device of loading the electron with the neutral core of a silver atom and doing a Stern-Gerlach experiment, but this may be considered only indirect, and what we would like is to be able to exhibit the moment of a *free* electron.

A number of ingenious experiments of the Stern-Gerlach type have been suggested for the purpose, but on closer examination they have all revealed fallacies of some kind. Finally Bohr was led to the tentative suggestion that the magnetism of the free electron might be intrinsically unobservable. By one of those simple arguments which only he can produce, he showed that a Stern-Gerlach experiment is useless, because a non-uniform field must have a cross component which exerts an uncertain force on the electric current. Attacking the question from the other end, he imagined that a polarized electron had been somehow produced and considered how it would be observed by a magnetometer. He showed that again the uncertainty of position and speed would lead to a greater variation of the magnetic force than would the intrinsic magnetism. Other work has shown that Bohr's tentative principle was wrong, and this emboldens me to disagree with the last conclusion, by the suggestion that there is no reason why we should not have several magnetometers surrounding the track of the electron; then the uncertainties of position will have different effects from the magnetic moment so that by a *survey* of the magnetic field the moment could be determined. At all events it is certainly possible to construct a wave-packet for an electron polarized in any direction, and to show that the distant field depends on the polarization. The argument is perhaps not quite conclusive, because the essential feature of the uncertainty principle is that the observation disturbs the experiment, and no allowance is made in Bohr's argument for the reaction of the magnetometer or magnetometers on the electron. But as the matter has been settled anyhow in the negative, it is not worth further enquiry.

When the diffraction of electrons was first observed, several experimenters tried without success to find a polarization. I was myself led to examine what their prospects were—this was before Bohr had discussed the subject. The coherent scattering by a crystal offers greater intensities for the experimenter than the scattering by an irregular assembly of atoms and it also gives easier mathematics. I therefore discussed this case, but simplified the problem a little by using a grating instead of a lattice; the extension to a crystal lattice would be very easy. There was also another simplification, which, as it turns out, has an important effect, and this was the assumption that the electric field was everywhere small, whereas in fact it of course becomes infinite near the nuclei of the atoms. The regularly spaced electric and magnetic forces can be analysed into Fourier series, and each term then is responsible for one spectrum. The study of one such spectrum will suffice. If a beam

of electrons already polarized falls on the grating, the electrons in the lateral beam will have the direction of their magnetization altered. It is a true polarization effect, for the change of direction depends only on the quality and not on the magnitude of the forces in the grating. When the forces are only electric the effect is a pure rotation through an angle depending only on the speed, and a similar result is true for some types of magnetic force, though here the rotation is irrespective of the speed. In these cases, if the original beam is unpolarized, the diffracted beam will be so too, since all its components are simply rotated like a solid body. But there are cases involving both electric and magnetic forces where this would not be so. It appeared that they could hardly be realized in practice, and as I was not aware of Bohr's suggestion at the time, I merely recorded the way they must be treated and verified that they would really give the effect, but without any estimate of magnitude. Recently I have worked out one or two cases, so as to see what polarization might be expected. One of the most favourable is something like a sheet of atoms magnetized normally. The amount of polarization is roughly $(vH/cE).\sin(\frac{1}{2}\epsilon)$ measured in Bohr magnetons per electron, where v is the speed, ϵ the angle of deviation, and E and H are respectively the greatest electric and magnetic forces in the grating. This result does not perhaps get us much further, because in fact the electric force becomes infinite at the nuclei, and so it is not clear what value should be chosen for E in the formula.

Mott has made a much better study of the question by solving accurately the passage of an electron past a nucleus. He showed that the electric forces could produce a partial polarization without, as in my case, the necessity of a magnetic force as well; the discrepancy is presumably due to my assumption that the electric force was everywhere small. The polarization could be observed by the device of scattering the beam twice in succession. Suppose for example that both scatterings are through right angles, and that we compare the intensities of the beams going parallel and anti-parallel to the original beam; then Mott showed that with the most favourable velocity, 3/4 that of light, there would be found a difference in the proportional number of electrons of amount about $(Z/96)^2$, where Z is the atomic number of the scattering nuclei. In arriving at this number certain rather doubtful assumptions were made, but the conclusion is quite definite that the result ought to be observable by the use of heavy elements and high speeds, but not with light elements or low speeds.

Very recently Rupp has published an account of some experiments on the subject, in which he claims to have detected the effect. He used two reflections at successive mirrors, set at almost glancing incidence. There is of course no essential difference between reflection at a plate of atoms, and scattering at a single atom; the plate merely concentrates the intensity into one direction at the expense of others, while the quality of the rays depends only on the influence of the single atoms. By rotating the second mirror round the direction of the rays, Rupp found an asymmetrical effect of Mott's type. It was 10 per cent. for gold when the electrons had a high speed, but not when they were slow, and it could not be detected for beryllium even at high speeds. The work was evidently carried out with very great skill and

care, and the experimental results look most convincing. But they do not support the theoretical work on account of their magnitude; for Mott showed that the asymmetry should be proportional roughly to $\sin^3(\theta/2)$, where θ is the angle of deflection at each mirror, and for Rupp's angle of 20′ the value should be quite insensible. It is indeed common sense that when the electron is only slightly deflected, the forces are weak and so we should expect the effect on the polarization also to be small. To clear the matter up it would be desirable therefore to have experiments at broader angles, but the intensities would become very small, and no doubt the technical difficulties would be enormous. To conclude, I do not think we can reconcile the theory and the experiment. If the theory is right the experiment should have shown no effect; while if the result of the experiment is right, as it seems to be, and if it is really due to the magnetism of the electron, then we must suppose some of the effects disregarded in the theory are more important than we should have anticipated.

THE GYROMAGNETIC EFFECT AND PARAMAGNETISM

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ABSTRACT. The importance of determinations of the gyromagnetic effect is discussed in relation to paramagnetism, though the gyromagnetic amplitude is so minute as to render accurate determination difficult in some cases. (i) In the case of ferromagnetism the value of $g \fallingdotseq 2$ found experimentally is in accord with theory. (ii) In the case of paramagnetic substances only one accurate determination has yet been made, namely that for Dy^{+++}; it gives $g = 4/3$, in good agreement with both susceptibility measurements and theory. Preliminary data on Mn^{++} and Cr^{+++} suggest, in agreement with susceptibility results, that both these ions are in S-states. (iii) Experiments in progress on Eu^{+++} taken in conjunction with the Van Vleck theory should yield useful information.

§ 1. INTRODUCTION

According to the Langevin theory of paramagnetism, deduced for a free magnetic gas, the susceptibility χ varies with temperature according to the law $\chi T =$ constant, where T is the absolute temperature. For many paramagnetic solids and solutions, the variation of susceptibility is given by the modified Curie law

$$\chi_M (T + \Delta) = C_M,$$

where χ_M is the gram-molecular susceptibility and C_M the Curie constant per gram molecule.

The magnetic moment is usually expressed such that the number, p, of Weiss magnetons ($\mu_W = 1123$) is given by

$$p = 14{\cdot}07\, C_M^{\frac{1}{2}} \qquad \ldots\ldots(1).$$

This assumes that the magnetic carrier is as free as in the gaseous state. No satisfactory theory has been put forward to account for the varying values of Δ which are found for paramagnetic substances.

On the quantum theory the magnetic moment of an atom or ion will have only discrete values, and expressions for the magnetic susceptibility have been deduced on this assumption. Hund* showed that the number of Weiss magnetons could be determined from the spectroscopic state and that

$$p = 4{\cdot}97g\sqrt{\{j(j+1)\}} \qquad \ldots\ldots(2),$$

where g is the Landé splitting factor, and j the quantum number specifying the total angular momentum. He calculated the magneton numbers for the rare earth

* F. Hund, *Zeit. für Phys.* **33**, 855 (1925).

group and, with the exception of Sm^{+++} and Eu^{+++}, the results showed close agreement with the experimental data. The expression was later extended by Laporte and Sommerfeld*, who pointed out that there would be a distribution of the ions among the different possible j-states.

Recently Van Vleck† has developed a comprehensive theory of magnetism on the basis of the quantum mechanics. He obtains an expression for the susceptibility

$$\chi = \frac{N\Sigma_j \{[g^2 j(j+1)/3kT] + \alpha(j)\}(2j+1)\, e^{-W/kT}}{\Sigma_j (2j+1)\, e^{-W/kT}} \qquad \ldots\ldots(3),$$

W where $W = hc\Delta\nu_j$ and is the difference in energy between the particular j level concerned and the ground level.

In the absence of experimental data, the overall multiplet separation is deduced from the formula

$$\Delta\nu = R\alpha^2 \frac{(Z-\sigma)^4 (2L+1)}{n^3 l (l+1)(2l+1)},$$

R where R is the Rydberg constant,

α α the Sommerfeld fine-structure constant,

L, l and L the resultant of all the individual l's.

This expression differs from that obtained by Laporte and Sommerfeld in the addition of the term $\alpha(j)$. The two terms in the numerator arise from the fact that the angular momentum and magnetic moment vectors are not parallel. The first term includes the contribution of the part of the magnetic moment which is parallel to the angular momentum j, whilst the other, a second order term, is due to the component of magnetic moment perpendicular to the angular moment vector, and gives rise to second order Zeeman terms. This $\alpha(j)$ term is particularly large where the l and s vectors are large, with the resultant j small. The two cases in which this holds are Eu^{+++} and Sm^{+++}, and Van Vleck claims that the previous discrepancy from Hund's formula in the case of these ions is completely removed by the inclusion of this term.

§ 2. THE GYROMAGNETIC EFFECT

M A change M in the magnetic moment of a substance should be accompanied
U by a change U in the angular momentum, the ratio being given by $M/U = g.e/2m$. Thus whilst, on the one hand, susceptibility determinations should give a mean value of $g^2 j(j+1)$, on the other hand, the gyromagnetic ratio enables one to calculate the mean value of g.

The author and others have measured this change of angular moment, which is small, for ferromagnetic bodies, and for all substances investigated $g = 2$ within the limits of experimental error. This, interpreted on modern theory, indicates that ferromagnetism is due to the spin of the electron. For paramagnetic materials, theory and experiment indicate that the magnetic moment is due, in general, to

* O. Laporte and A. Sommerfeld, *Zeit. für Phys.* **40**, 333 (1926); Laporte, *ibid.* **47**, 761 (1928).
† J. H. Van Vleck, *Phys. Rev.* **31**, 587 (1928); *ibid.* **34**, 1494 (1929).

the resultant of orbital and spin moments. The writer has recently developed a method for measuring this ratio for paramagnetic substances* and results have been obtained on the Dy^{+++} ion in Dy_2O_3. The state of this ion according to Hund is ^6H, the j value being $(l+s) = 15/2$†. The number of Weiss magnetons calculated by means of equations (1) and (2) from the above state is 53, whereas susceptibility measurements give 52. The measurements of the gyromagnetic ratio for this ion give the value of g as $1{\cdot}28 \pm 7$ per cent., in excellent agreement with the theoretical value 4/3.

§ 3. COMPARISON WITH EXPERIMENT

It remains to discuss other members of the several groups and to compare theory with experimental data in so far as they exist. In the case of the first transition series, none of the above theories of susceptibility shows any agreement with the experimental data. In order to explain the magneton numbers, one has to make the arbitrary assumption that all the ions are in S-states‡.

Preliminary measurements on the gyromagnetic ratio have been made by the writer on Mn^{++} and Cr^{+++}, though owing to the relatively small susceptibilities of these substances as compared with that of Dy^{+++}, it will be necessary to obtain greater sensitivity before more exact results can be obtained. The amplitude of the angular momentum varies inversely as g and the measurements made so far for these two ions give $g > 2$. In the case of Mn^{++} all theories agree well with the susceptibility measurements, there being d^5 electrons which give a ^6S-state for which $g = 2$. For Cr^{+++} the original Hund theory gives g as 2/5 whilst calculation from the Laporte-Sommerfeld expression gives a mean value of g as 1·4. Cr^{+++} is, therefore, another ion for which a direct determination of the gyromagnetic effect is required.

For the rare earth group, Van Vleck made his comparison of theory with experiment by taking the susceptibility results at room temperature and assuming the Curie law $\chi T =$ constant. But Cabrera and Duperier§ have measured the susceptibilities of most of these ions, in some cases both for the oxide and the anhydrous sulphates, and found that in general the susceptibilities follow the law

$$(\chi + K)(T + \Delta) = C \qquad \text{.....(4),}$$

where K is independent of the temperature.

If these are classified according to increasing number of $4f$ electrons, the susceptibilities fall naturally into three types. Of the first six, Ce^{+++} has not been measured, and the ion corresponding to the element 61 is as yet unknown. The remaining four have a negative value of K, corresponding to a constant superposed paramagnetism which increases to a maximum at Eu^{+++}. For Sm^{+++} the $(\chi + K)$

* *Proc. R.S.* (in press).

† For ions in the second half of the rare earth group, which includes Dy^{+++}, the Van Vleck values do not differ appreciably from those of Hund. This is due to the fact that the multiplets, which are wide ($\sim$ 10,000 cm.$^{-1}$), are inverted, so that the Boltzmann factors for their components of lowest j are negligible.

‡ For a complete discussion of this point see E. C. Stoner, *Phil. Mag.* **8**, 250 (1929).

§ B. Cabrera and A. Duperier, *Comptes Rendus*, **188**, 1640 (1929).

value is so small and Δ so large that the total susceptibility is approximately independent of temperature and only preliminary values are given. For the next five, which have the greatest susceptibilities, $K = 0$, and the modified Curie law is followed. For Gd^{+++}, which occupies the position analogous to that occupied by Mn^{++} in the iron group, $\Delta = 0$. For the two remaining substances, Tu^{+++} and Yb^{+++}, K is positive. In general, the K values for the first type, i.e. in the first half of the group, are numerically greater for the sulphate than for the oxide, whilst in the third type the reverse is the case.

Van Vleck applied his theory to the room-temperature susceptibility-measurements on the rare earths, and completely removed the discrepancy for Eu^{+++} and Sm^{+++}, which were the two exceptions to the Hund equation, and improved the agreement in the case of the other members of the first half of the group. In view of the measurements of Cabrera and Duperier mentioned above, it is worth while to apply equation (3) to the experimental results at different temperatures. For $Eu_2(SO_4)_3$ the agreement between 150° and 600° abs. is quite close, the calculated susceptibility varying from the experimental values by only 1 or 2 per cent. At 300° abs., the $\alpha(j)$ term is responsible for about 75 per cent. of the susceptibility, this proportion decreasing with increasing temperature*. For the oxide, the χ-T curve lies below that of the sulphate by a constant difference of about 10 per cent. of the susceptibility at room temperature.

In the case of Sm^{+++}, equation (3) gives an approximately constant susceptibility between 300° and 600° abs., but there is an upward trend of the susceptibility with decreasing temperature, whereas experiment† shows an approximately constant value. In order that equation (3) may yield a constant susceptibility the first term must disappear. This obtains when the majority of the electrons are in a lowest level which has j or $g = 0$, unless it is an S term. Until more definite information about the shape of the χ-T curve is available, it does not seem profitable to discuss it further.

As has already been pointed out, K is negative in the first half of the group, and positive in the second half. This result may be connected with the fact that owing to the Boltzmann factor the apparent magneton number increases with temperature for erect terms, and decreases for inverted terms.

Considering the assumptions involved the agreement appears to be fairly satisfactory. In the first place, there is little or no experimental confirmation for the electron distributions, and further, there is the uncertainty of deducing the correct multiplet separations and screening constants. For other ions, the Van Vleck and Hund expressions agree sufficiently well with susceptibility determinations for the difference to be attributed to experimental error.

Measurements of the gyromagnetic ratio on Eu^{+++} should be of interest, and experiments are in progress on Eu_2O_3. The Sommerfeld-Laporte theory gives a mean value of $g = 3/2$‡. On the assumption that Van Vleck's $\alpha(j)$ term in

* The temperature range of Cabrera and Duperier's measurement is not specified.

† E. H. Williams, *Phys. Rev.* **12**, 165 (1918).

‡ The ground level is ^7F and all states except the lowest have $g = 3/2$.

equation (3) does not contribute towards the angular momentum measured, the effective mean value of g should be about 6·4. Measurements of the gyromagnetic amplitude should throw light on this point.

Finally we may consider the susceptibilities of the ions of the second and third transition series. The chlorides have been investigated by Cabrera and Duperier*. Only one ion, Ru^{+++}, shows appreciable paramagnetism (specific susceptibility $= 9{\cdot}5 \times 10^{-6}$), the rest having positive or negative susceptibilities of the order 10^{-7}. Ru^{+++} is the only one which has an odd number of electrons.

Measurements for these six ions were made over a range of temperature of 100° and are expressible by the empirical formula (4) above, K being positive for Ru^{+++}.

On the other hand, the measurements have been made over a limited range of temperature so that it is difficult to determine accurately the deviation from the usual law $\chi(T + \Delta) = \text{const.}$

In spite of the difficulty of assigning the electron distribution to these ions in the absence of spectroscopic evidence, it is obvious that they are quite different from the members of either the first or fourth transition series. Even in the case of Ru^{+++} the deepest levels deduced from the distributions d^5 and sd^4 both give susceptibilities greater than the experimental value.

On the whole it appears that the modified Curie law is the exception rather than the rule for the four transition series. Generally speaking, it is only in cases where the susceptibility is large that the law is obeyed. Even in these cases, the departure from this law may be masked by the magnitude of the magnetization. Stoner† notes two exceptions in the iron group, e.g. Ni^{+++} and Cu^{++}, and these are two which have smaller susceptibilities. It seems essential to make measurements over as wide a temperature-range as possible in order to throw light on this point.

* B. Cabrera and A. Duperier, *Comptes Rendus*, **185**, 414 (1927).
† *Loc. cit.*

THE CHANGE IN SIZE OF A FERROMAGNETIC AT THE CURIE POINT

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Communicated by R. H. Fowler, F.R.S.

ABSTRACT. The changes of size of iron and nickel at their Curie points are too great to be explained by purely magnetic forces. On Heisenberg's theory of ferromagnetism, magnetization is accompanied by a change in the strength of binding between atoms, and the resulting changes of size of crystals of iron and nickel at their Curie points are of the same order of magnitude as those observed.

The calculations for iron make use of a modification of Heisenberg's original theory, in which the restriction that each atom possesses only one electron concerned in magnetic effects is removed. A consideration of the specific heat discontinuity at the Curie point shows, however, that the theory is far from satisfactory.

The change in size which accompanies magnetization varies continuously from the Curie point to saturation, but does not affect measurements of magnetostriction at ordinary temperatures.

§ 1. INTRODUCTION

When the temperature of a ferromagnetic crystal passes through the Curie point, the crystal shows an anomalous expansion or contraction. This phenomenon could not be interpreted on Weiss's molecular field theory, since in order to account for it one required to know how the strength of the molecular field depends upon the size of the crystal, and this could not be determined, the physical origin of the molecular field being quite unknown.

In Heisenberg's* theory of ferromagnetism the molecular field is replaced by the "exchange" interaction of the quantum theory, which leads to homopolar binding between the atoms in the crystal. The change of size at the Curie point may be directly interpreted as the strain produced by the change of strength of binding which accompanies the change-over from the paramagnetic state (electron spins orientated at random) to the magnetized state (electron spins orientated parallel).

Attention was drawn to the phenomenon by Fowler and Kapitza†, who calculated the change of size on Heisenberg's theory, and made a rough estimate of its magnitude in iron. The experimental values which they used were, however, incorrect‡, and it is the main object of this paper to give the correct data. The data

* W. Heisenberg, *Zeit. für Phys.* **49**, 619 (1928).

† R. H. Fowler and P. Kapitza, *Proc. R.S.* **124**, 1 (1929).

‡ This was pointed out by Dr W. L. Webster, to whom I am much indebted for references to the correct data.

quoted by Fowler and Kapitza relate not to the Curie point, but to the temperature at which an allotropic transformation occurs (Fe β, body-centred cubic $\rightarrow$ Fe γ, face-centred cubic). In the purest specimens this transition temperature lies about 160° above the Curie point, but in others the interval can apparently be very much smaller, so that the two temperatures can easily be confused. The volume change at the higher temperature is discontinuous, whereas that at the Curie point is continuous, and much smaller than the former. The magnitude of the effect is, in fact, almost of the same order as the ordinary magnetostriction, and it is necessary to show that the effect is not entirely due to the latter. The conclusion is reached that the effect cannot be entirely due to purely magnetic forces.

It is also necessary to revise the theoretical estimate of the value. In doing this, we make use of the modified theory, which has recently been given by Heisenberg*, for the case when there is more than one magnetic electron per atom (iron clearly requires such a theory). It is only possible to estimate the change of size roughly, but the correct observed values for iron and nickel are found to be of an order of magnitude acceptable to the theory. The larger erroneous values which Fowler and Kapitza had to account for would have put a great strain on the possibilities of the theory.

Finally, the relation of the Curie-point change of size to the magnetostriction at ordinary temperatures is considered. Data are given for iron and nickel.

§ 2. EXPERIMENTAL DATA

Magnetic measurements are most directly interpretable when made on single crystals, but unfortunately no experiments appear to have been made on the change of size of single crystals at the Curie point. We must therefore make use of measurements made on polycrystalline specimens.

(*a*) *Iron.* Benedicks† measured the linear expansion of a strip of pure electrolytic iron in the temperature range 700° C.–950° C. by a differential method, in which the expansion of the iron was compared with that of similar strips of gold or palladium, which have nearly the same coefficients of expansion as iron. Comparison of the curves obtained with gold and palladium shows that the expansion of these metals is quite regular in the temperature range considered. The size of the strip was 89 mm. × 4 mm. × 0·16 mm. The heating was effected by an electric furnace, which produced a magnetic field along the length of the strip of about 10 gauss (the field varied with temperature). Throughout all the measurements, the strip was under a tension of 34 gm./mm.^{2}. Rough measurements were made of the intensity of magnetization in the specimen, and the Curie point was found to be 768° C.

Part of one of the experimental curves is shown in Fig. 1. The ordinates represent the excess δl of the length l of the iron strip, over that of a gold strip, where $l = 89$ mm. It is seen that there is no discontinuity in length at the Curie point, $l, \delta l$

* *Probleme der modernen Physik*, p. 114 (Hirzel, Leipzig, 1928).

† C. Benedicks, *Journal of the Iron and Steel Institute*, **89**, 407 (1914).

but that in the magnetized state an increase in length, which disappears at the Curie point, is superposed upon the ordinary thermal expansion. It should be noted, however, that the coefficient of expansion does not actually become negative*. To find the magnitude of this change of length, it is necessary to produce back the expansion curves above the Curie point. This has been done for three of

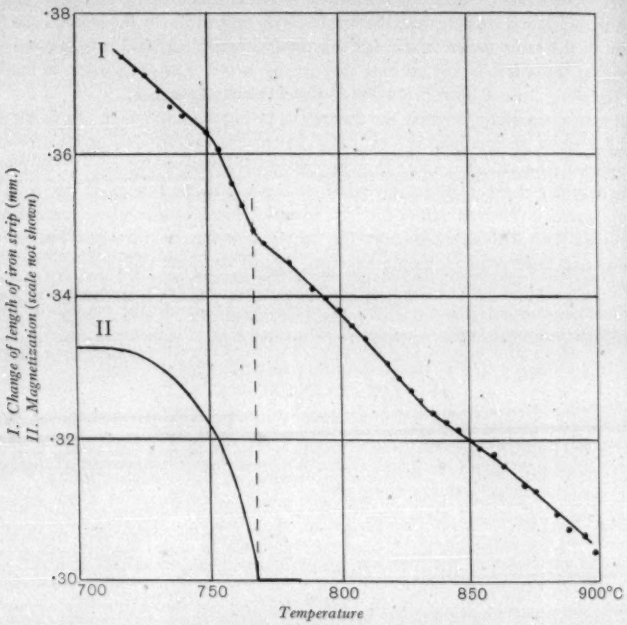


Fig. 1. Effects of temperature on iron.

Benedicks' curves, which have been produced back to 50° below the Curie point (it does not seem safe to extrapolate further), and the following value is obtained for the mean value of the increase in length in the magnetized state:

$$\delta l/l = + (1{\cdot}2 \pm 0{\cdot}4) \times 10^{-4}.$$

Benedicks also measured the ordinary magnetostriction at various temperatures, i.e. the change of length resulting from the application of the magnetic field. This was done very simply by the mere switching off of the current in the heating coils,

* The well-marked change of slope at about 835° C. is interesting. Near this temperature there is also a change in the paramagnetic constants.

and observation of the change of length. As will appear later, we shall only require to know the value at 718° C. (i.e. 50° below the Curie point), which was found to be

$$\delta l/l = 6 \times 10^{-6}.$$

(*b*) *Nickel.* The coefficient of expansion of nickel in the neighbourhood of the Curie point was measured by Colby*, who used an electrolytically prepared specimen, 8·2 mm. in thickness, with a Curie point at 374° C. The specimen was heated by means of an electric furnace, the coils of which were so arranged as to produce no magnetic field, and the coefficient of expansion was measured at various temperatures by an optical method.

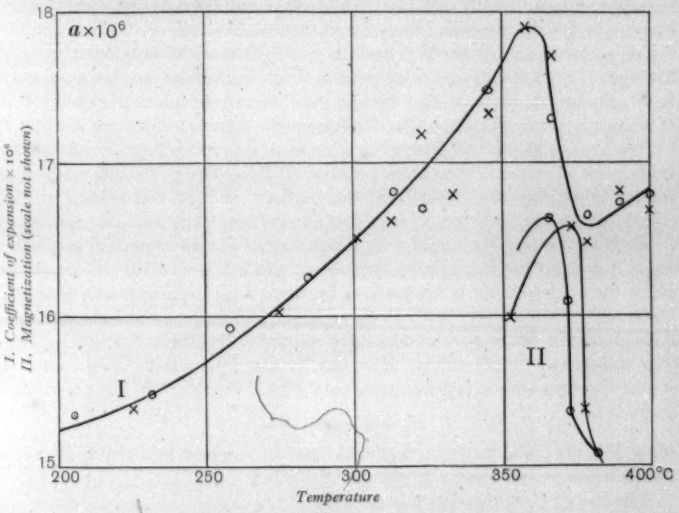


Fig. 2. Effects of temperature on nickel.

The results of two series of experiments are shown in Fig. 2. The ordinates represent the coefficient of expansion. It is seen that as in the case of iron, below the Curie point, nickel shows an anomalous change of length which disappears at the Curie point. But whereas iron shows an anomalous expansion, nickel shows an anomalous contraction in the magnetized state. The length is given by the area under the expansion curve, and the anomalous change of length is given by the area of the hump in the curve. At 220° C. the estimated change in length per unit length is as follows:

$$\delta l/l = -(0{\cdot}9 \pm 0{\cdot}3) \times 10^{-4}.$$

* W. F. Colby, *Phys. Rev.* **30**, 506 (1910).

§ 3. CORRECTION FOR MAGNETIC FORCES

The above results show that the Curie-point change of length is larger than the change produced by the application of a field at ordinary temperatures (the ordinary magnetostriction) but of the same order of magnitude. The variation of the latter change in different directions in a single crystal of iron is as great as 35×10^{-6} per unit length.

In this paper the assumption is made that the magnetostriction at ordinary temperatures arises from the purely magnetic interactions of dipoles and quadrupoles situated at the lattice points of the crystals; the dipoles and quadrupoles are assumed to be "rigidly" attached to each other with their axes parallel. Akulov* has shown that the magnetic interactions between atomic dipoles will account for the magnetostriction in single crystals in cases of saturated magnetization; and Mahajani† has shown that the assumption of orienting quadrupoles, rigidly attached to the dipoles, accounts satisfactorily for the observed deviations of the direction of magnetization in single crystals of iron from the direction of the applied field.

We have therefore to calculate the change of length which will occur at the Curie point when the ferromagnetic possesses (i) dipole energy and (ii) quadrupole energy, in addition to the usual (iii) elastic energy and (iv) thermal energy. In calculating the length changes arising from (i) and (ii), it is permissible to neglect (iv).

(i) *Dipole energy*. We consider the dipole energy of a polycrystalline specimen, and will suppose that it is split up into a large number of small regions in each of
I which the magnetization is saturated to intensity I, and approximately uniform. These regions will not necessarily be the small single crystals of which the specimen is composed; in fact, experimentally there appears to be little connection between their volume and the crystalline "grain size"‡. The dipoles in the neighbourhood
H_i of a lattice point will be approximately parallel, and the force H_i at the point will be given by

$$H_i = H_s + \tfrac{4}{3}\pi I + H,$$

H, H_s where H is the (bulk) magnetic field, and H_s is the magnetic field arising from the dipoles inside a small sphere surrounding the point.

U_d The energy U_d of the system is given by

$$U_d = -\tfrac{1}{2}\int (I, H_i)\, d\tau.$$

H itself arises from the system of dipoles, and will be proportional to I, so that we
α may write

$$|H| = \alpha |I|.$$

The value of α will vary from point to point, and will depend upon the shape and arrangement of the small fully magnetized regions. If, for example, the regions are infinite plane laminae, magnetized alternately parallel and anti-parallel to their common normal, then

$$\alpha = -4\pi.$$

* N. Akulov, *Zeit. für Phys.* **52**, 389 (1928–9); **59**, 254 (1930).
† G. S. Mahajani, *Phil. Trans.* **228**, 63 (1929).
‡ This conclusion is reached through a study of the Barkhausen effect.

But such an arrangement is very unlikely, as we should expect the regions to be such as to possess minimum magnetic energy, subject, perhaps, to conditions of minimum volume or maximum surface area per unit volume. The magnetic energy is actually a minimum when the regions are long filaments, magnetized along their length, for α then vanishes. In any case, we may replace α by its average value, which almost certainly lies between 0 and -4π. We obtain, therefore, for the energy

$$U_d = -\tfrac{1}{2}\int (I, H_s + \beta I)\, d\tau,$$

where β is a constant lying between $\frac{4}{3}\pi$ and $-\frac{8}{3}\pi$. β

For a cubic crystal (as in the cases of iron and nickel), H_s will vanish. But in order to investigate the stresses arising from the dipole energy, we need to know the energy of an arbitrarily strained crystal, for which H_s will no longer vanish. H_s will, in general, be a linear function of the strain components, and the stress will be obtained by differentiation of U_d with respect to these strain components. In general, therefore, the stress, and hence the resulting strain, will be anisotropic, in correspondence with the fact that the magnetostriction varies in different directions in a single crystal.

Now consider, in particular, a polycrystalline specimen, unmagnetized in bulk. We may suppose that the crystal axes are oriented at random, and further that the directions of magnetization in the small magnetized regions are also at random. In this case, the strain produced by the magnetic forces must be isotropic, since all directions are equivalent, and we may therefore take

$$H_s = 0.$$

The energy is then $$U_d = -\tfrac{1}{2}\beta I^2 V,$$

where V is the volume of the specimen. V

The elastic energy ω is given by ω

$$2\omega = \frac{1}{\kappa_0}\left(\frac{\delta V}{V}\right)^2 V,$$

where κ_0 is the compressibility. The size is determined by κ_0

$$\frac{\partial}{\partial \delta V}(\delta U_d + \omega) = 0,$$

whence $$\delta V/V = -\tfrac{1}{2}\kappa_0 \beta I^2,$$

and $$\delta l/l = -\tfrac{1}{6}\kappa_0 \beta I^2.$$

For iron, $$\kappa_0 = 0{\cdot}6 \times 10^{-12},$$

$$I = 1710,$$

and therefore $$|\delta l/l| < 2{\cdot}6 \times 10^{-6}.$$

For nickel, $$I = 510,$$

so that $\delta l/l$ is even smaller. Thus in both cases, the dipole forces produce no appreciable change of length in a polycrystalline specimen which is unmagnetized in bulk.

U_q

(ii) *Quadrupole energy*. We shall now deal with quadrupole forces, and again we need only know the quadrupole energy U_q of an unstrained cubic crystal. This has been calculated for the case of iron (body-centred cubic lattice) by Mahajani*, who found for the energy

$$U_q = \frac{\gamma}{d^5}(s_2^2 s_3^2 + s_3^2 s_1^2 + s_1^2 s_2^2 - \tfrac{1}{5}),$$

s_1, s_2, s_3, d γ

where s_1, s_2, s_3 are the direction cosines of the quadrupoles, d is the lattice constant, and γ is a constant.

For a polycrystal, unmagnetized in bulk, the direction (s_1, s_2, s_3) will be at random, and we must replace $(s_2^2 s_3^2 + s_3^2 s_1^2 + s_1^2 s_2^2)$ by its average value. Thus

$$U_q = \frac{\gamma}{d^5}(s_2^2 s_3^2 + s_3^2 s_1^2 + s_1^2 s_2^2 - \tfrac{1}{5}) = 0.$$

Thus the quadruple energy of a bulk-unmagnetized polycrystal vanishes, and so can lead to no change of length at the Curie point. We assume that this result holds also for nickel, with a face-centred cubic lattice.

We have found that the magnetic forces lead to no appreciable change of length at the Curie point when the state changes from the completely unmagnetized (i.e. paramagnetic) to the fully magnetized on the micro-scale, the specimen being still unmagnetized on the macro-scale. This was actually the change of state in Colby's experiments. But in Benedicks' experiments, the specimen was placed in a field of about 10 gauss, which at high temperatures is sufficient to produce nearly saturated bulk-magnetization; the mechanical tension in the specimen would also assist in the attainment of saturation. We must therefore add to the above correction that corresponding to the change-over from the bulk-unmagnetized to the bulk-magnetized state. In the calculation of this change of length, it would no longer be sufficient to consider only isotropic strains, and it would be necessary to calculate U_d and U_q for an arbitrary strain†. But this calculation can be avoided, since the magnetostriction which Benedicks measured (viz. the change of length produced by application of the field) is precisely the required correction; this change of length must therefore be subtracted from the total change of length at the Curie point.

It is thus seen that the magnetic forces cannot account for the observed change of length at the Curie point. The values of this change, corrected for magnetic forces, are

(*a*) for iron,

$$\delta l/l = (+ 1{\cdot}1 \pm 0{\cdot}4) \times 10^{-4} \text{ at } T = 718^\circ \text{ C.}; \; T_c = 768^\circ \text{ C.};$$

(*b*) for nickel,

$$\delta l/l = (- 0{\cdot}9 \pm 0{\cdot}3) \times 10^{-4} \text{ at } T = 220^\circ \text{ C.}; \; T_c = 374^\circ \text{ C.}$$

* *Loc. cit.*

† In the calculation of U_d, which has been made by Akulov (*loc. cit.*), H_i will no longer vanish.

§4. HEISENBERG'S MODIFIED THEORY

In its original form, Heisenberg's* theory applied to the case in which there is one effective electron per atom, and the changes of size at the Curie point have been calculated by Fowler and Kapitza†, by the use of this form of the theory. Heisenberg‡ has since modified his theory for the case of more than one effective electron per atom, and the modification can easily be introduced into the calculations of the Curie point change of length. No essentially new feature is introduced, and it is unnecessary to give the calculations in full detail. For details of the method, reference should be made to the paper by Fowler and Kapitza, whose notation is used here.

Heisenberg considers a crystal of $2n$ atoms, each with z "nearest neighbours." Each atom possesses y effective electrons, and the total number of electrons is $2N$, where n, z, y

$$2N = 2ny. \qquad N$$

It is assumed that the exchange interactions between electrons belonging to the same atom are much greater than those between electrons belonging to different atoms. It is further assumed that the exchange energy corresponding to a simple transposition of a pair of electrons belonging to neighbouring atoms has the same value J_0 for all such pairs, and is negligible for pairs of electrons belonging to atoms which are not closest neighbours. The mean energy $\bar{E}$ of all states of the system with the *eigen*-value s for the total spin of the electrons is found to be J_0 $\bar{E}$ s

$$\bar{E} = -zy\frac{s^2+N^2}{2N}J_0 + \text{const.},$$

and the mean square deviation $\overline{\Delta E^2}$ of the energy from its mean value is $\overline{\Delta E^2}$

$$\overline{\Delta E^2} = zy\frac{(N^2-s^2)(3N^2-s^2)}{4N^3}J_0^2.$$

If m is the component of s in the direction of the external magnetic field H, and m μ is the mass of an electron, the energy of a state with *eigen*-values m and s is μ

$$\bar{E} + \overline{\Delta E} - \frac{eh}{4\pi\mu c}H.2m.$$

A Gaussian distribution of energy levels being assumed, the partition function K can now be formed, and reduced to the form K

$$K = \sum_{m=-N}^{N} f_m \exp\left\{\left[\alpha + y\beta\left(1-\frac{\beta}{z}\right)\frac{m_0}{N} + \frac{y\beta^2 m_0^3}{2zN^3}\right]m\right\}$$
$$\times \exp\left\{\frac{y\beta N}{2} + \frac{3y\beta^2 N}{8z} - y\beta\left(1-\frac{\beta}{z}\right)\frac{m_0^2}{2N} - \frac{3y\beta^2 m_0^4}{8zN^3}\right\},$$

where

$$\alpha = \frac{1}{kT}\frac{eh}{2\pi\mu c}H; \qquad \alpha$$

$$\beta = \frac{zJ_0}{kT}; \qquad \beta$$

* W. Heisenberg, *Zeit. für Phys.* **49**, 619 (1928). † *Loc. cit.*

‡ *Probleme der modernen Physik*, p. 114 (Hirzel, Leipzig, 1928).

f_m m_0 f_m is the number of states with the *eigen*-value m for the component of s in a fixed direction, and m_0 is the most probable value of m.

Now each atom has total spin $\frac{1}{2}y$, and its component in any direction can have the values $\frac{1}{2}y, \frac{1}{2}y-1, \ldots, -\frac{1}{2}y$. f_m is therefore the coefficient of ξ^m in

$$(\xi^{\frac{1}{2}y} + \xi^{\frac{1}{2}y-1} + \ldots + \xi^{-\frac{1}{2}y})^{2n}.$$

We obtain* for K,

$$K = [e^{xy} + e^{x(y-2)} + \ldots + e^{-xy}]^{2n} \times \exp\left\{\frac{y\beta N}{2} + \frac{3y\beta^2 N}{8z} - y\beta\left(1-\frac{\beta}{z}\right)\frac{m_0^2}{2N} - \frac{3y\beta^2 m_0^4}{8zN^3}\right\},$$

x, ζ where

$$2x = \alpha + y\beta\left(1-\frac{\beta}{z}\right)\zeta + \frac{y\beta^2}{2z}\zeta^3 \qquad \ldots\ldots(1),$$

and ζ is written for m_0/N, the ratio of the magnetization to the value when all the electron spins are parallel. More conveniently,

$$\log K = 2n\left\{\log\left[e^{xy} + e^{x(y-2)} + \ldots + e^{-xy}\right] + \frac{1}{4}y^2\beta + \frac{3}{16}\frac{y^2\beta^2}{z} - \frac{1}{4}y^2\beta\left(1-\frac{\beta}{z}\right)\zeta^2 - \frac{3}{16}\frac{y^2\beta^2}{z}\zeta^4\right\} \quad (2).$$

m_0 is given by

$$m_0 = \frac{\partial}{\partial\alpha}(\log K),$$

or

$$\zeta = \frac{1}{y}\cdot\frac{ye^{xy} + (y-2)e^{x(y-2)} + \ldots - ye^{-xy}}{e^{xy} + e^{x(y-2)} + \ldots + e^{-xy}} \qquad \ldots\ldots(3);$$

the right-hand side is the new Langevin function. When $y = 1$ it reduces to $\tanh x$.

Equations (1), (2) and (3) represent the modified theory, and in order to test it I have calculated the change of specific heat at the Curie point. It will be seen later that it is permissible to neglect all terms containing z explicitly, and with this simplification the specific heat changes are found to be:

(*a*) For iron, the value of y being taken as 3, $\Delta C = 2{\cdot}2R = 4{\cdot}4$ calories per degree per gram-atom.

The observed value† is 6·8.

(*b*) For nickel, the value of y being taken as 1, $\Delta C = 1{\cdot}5R = 3$ calories per degree per gram-atom.

The observed value† is 1·7.

The agreement is unsatisfactory, and suggests that the assumptions upon which the theory is based are unsound. In order to account for the high value for iron, it seems necessary to suppose that the electrons are free to orient individually, and not in groups of y as in the present theory. This condition would obtain if, as

* Heisenberg obtains the equation

$$K = F.(e^{xy} + e^{x(y-1)} + \ldots + e^{-xy})^{2n},$$

which disagrees with the expression given above. I think that Heisenberg's expression is incorrect, since it contains both odd and even powers of e^x, although it is derived from an expression containing only even powers.

† See Fowler and Kapitza, *loc. cit.*

has been suggested*, the electrons involved are conduction electrons, but the suggestion involves several difficulties. For example, it cannot explain the ferromagnetism of minerals which are relatively bad conductors, although some of these also show large specific heat changes at the Curie point.

In the paramagnetic state, when ζ and x are small, the equations become

$$\left.\begin{aligned} 2x &= \alpha + y\beta\,(1-\beta/z)\,\zeta \\ \zeta &= \tfrac{1}{3}\,(y+2)\,x \end{aligned}\right\},$$

whence

$$\zeta = \frac{\alpha}{6/(y+2) - y\beta\,(1-\beta/z)},$$

and therefore the value β_c of β at the Curie point is given by β_c

$$\beta_c = \frac{z}{2}\left[1 - \left\{1 - \frac{24}{zy\,(y+2)}\right\}^{\frac{1}{2}}\right] \qquad \text{.....(4),}$$

and the corresponding Curie temperature T_c is given by T_c

$$T_c = \frac{2J_0}{k}\left[1 - \left\{1 - \frac{24}{zy\,(y+2)}\right\}^{\frac{1}{2}}\right]^{-1}. \qquad \text{.....(5).}$$

As a necessary condition for ferromagnetism, we must have

$$z \geqslant 24/y\,(y+2).$$

This condition is satisfied by all types of lattice when $y > 1$.

When z is large, these formulae become

$$\beta_c = \frac{6}{y\,(y+2)} \qquad \text{.....(4'),}$$

$$T_c = \frac{zJ_0}{k}\cdot\frac{y\,(y+2)}{6} \qquad \text{.....(5'),}$$

and the formula for ζ just expresses the Weiss law, with the gram-molecular susceptibility χ_M, where χ_M

$$\chi_M = \frac{n_0}{k}\,\frac{y\,(y+2)}{3}\left(\frac{eh}{4\pi\mu c}\right)^2\cdot\frac{1}{T-T_c},$$

n_0 being Avogadro's number. This is the usual expression. n_0

§ 5. THE CURIE-POINT CHANGE OF SIZE

To take into account the forces which are not involved in the magnetic energy, we use the corrected partition function K' given by K'

$$\log K' = 2n\left\{\log\left[e^{xy} + e^{x(y-2)} + \ldots + e^{-xy}\right] - \frac{1}{4}y^2\beta\left(1-\frac{\beta}{z}\right)\zeta^2 - \frac{3}{16}\,\frac{y^2\beta^2}{z}\,\zeta^4\right\} - \frac{F}{kT},$$

where F is the rest energy of the crystal. We neglect the energy of the thermal motion. F

* J. Dorfmann and R. Jaanus, *Zeit. für Phys.* **54**, 277 (1929); J. Dorfmann and I. Kikoin, *ibid.* **54**, 289 (1929).

The size is determined by

$$\frac{\partial}{\partial V}\log K'(H, V, T) = 0,$$

or

$$2ny^2\frac{\partial\beta}{\partial V}\left\{\frac{1}{4}\left(1-\frac{2\beta}{z}\right)\zeta^2+\frac{1}{8}\frac{\beta}{z}\zeta^4\right\}-\frac{1}{kT}\frac{\partial F}{\partial V}=0.$$

Above the Curie point the first term vanishes, and therefore the increase in volume in the magnetized state is given by

$$\frac{\delta V}{V}=2nzy^2\kappa_0\frac{\partial J_0}{\partial V}\left\{\frac{1}{4}\left(1-\frac{2\beta}{z}\right)\zeta^2+\frac{1}{8}\frac{\beta}{z}\zeta^4\right\} \qquad \text{.....(6),}$$

κ_0 where κ_0 is the compressibility, given by

$$\frac{1}{\kappa_0}=V\frac{\partial^2 F}{\partial V^2}.$$

§ 6. NUMERICAL ESTIMATES FOR IRON AND NICKEL

It has not yet proved possible to calculate the exchange energy J_0, which must therefore be regarded as a constant to be determined experimentally; for this purpose equation (5) is convenient. $\partial J_0/\partial V$ is also unknown, but we can estimate its order of magnitude when the magnitude of J_0 is known. We shall use (6) to determine $\partial J_0/\partial V$.

(*a*) *Iron*. It is clear from equation (6) that the sign of $\partial J_0/\partial V$ determines the sign of $\delta V/V$. Now J_0 is positive, and it is surprising, as Fowler and Kapitza pointed out, to find that for iron $\delta V/V$, and hence $\partial J_0/\partial V$, are positive also. But they did not point out an important consequence of this fact. In Heisenberg's theory it is assumed that the exchange energy corresponding to an exchange of electrons belonging to atoms other than "closest neighbours" is negligible, the supposition being that J_0 decreases rapidly (exponentially) with interatomic distance. This must certainly be true for large enough distances, but it is now seen that J_0 is, in fact, increasing at the interatomic distance of closest neighbours. Iron has a body-centred cubic lattice, and each atom has 8 closest neighbours at distance $d\sqrt{3}/2$, where d is the lattice constant. There are 50 more atoms at distance $d\sqrt{3}$ or less. If J_0 is increasing at distance $d\sqrt{3}/2$, it seems highly improbable that the effect of these 50 other atoms can be neglected. This conclusion, that at small distances the strength of homopolar binding does not decrease so rapidly as is commonly supposed, is important in problems of crystal structure.

It appears, therefore, that in the preceding formulae we must suppose z to be considerably greater than 8. Now z occurs explicitly in formulae (1) to (4) on account only of the Gaussian distribution of energy levels, and the effect of increasing z is to decrease the effect of the Gaussian distribution. To the order of accuracy which we require here, we can neglect the Gaussian distribution altogether, for the numerical errors involved are at most 10 per cent. This results in a considerable simplification of the formulae, such as was made in the construction of equations (4′) and (5′).

The data for iron are as follows:

$$T_c = 768°\text{ C.};\quad \kappa_0 = 0{\cdot}6 \times 10^{-12};\quad 2n/V = 8{\cdot}6 \times 10^{22}.$$

The most probable value of y is 3.

At 718° C. $\zeta = 0{\cdot}4$, from (1) and (3),

$$\delta l/l = +\,1{\cdot}1 \times 10^{-4},$$

and therefore the volume change $\delta V/V$ is

$$+\,3{\cdot}3 \times 10^{-4}.$$

Substituting these values in (6), we get

$$z\,\partial J_0/\partial V = 1{\cdot}7 \times 10^{-14}.$$

From (5′)
$$zJ_0 = 5{\cdot}7 \times 10^{-14}.$$

The distance a between an atom and its nearest neighbour is

$$\tfrac{1}{2}\sqrt{3} \times 2{\cdot}8 \times 10^{-8},$$

and the binding force $\partial J_0/\partial a$ is given by

$$z\,\frac{\partial J_0}{\partial a} = 2{\cdot}1 \times 10^{-6}.$$

(*b*) *Nickel.* For nickel, $\partial J_0/\partial V$ is negative, but it is reasonable to suppose that in this case also we ought to take z greater than the number of *closest* neighbours, which is 12 since nickel has a face-centred cubic lattice. We shall therefore again neglect the effect of the Gaussian distribution.

The data are as follows:

$$T_c = 374°\text{ C.};\quad \kappa_0 = 0{\cdot}6 \times 10^{-12};\quad 2n/V = 9{\cdot}4 \times 10^{22};\quad y = 1.$$

At 220° C. $\zeta = 0{\cdot}7$ and $\delta l/l = -\,0{\cdot}9 \times 10^{-4}$,

and therefore
$$\delta V/V = -\,2{\cdot}7 \times 10^{-4}.$$

Substituting these values in (6), we get

$$z\,\frac{\partial J_0}{\partial V} = -\,3{\cdot}9 \times 10^{-14}.$$

We have also
$$zJ_0 = 1{\cdot}8 \times 10^{-13}.$$

The distance a between an atom and its closest neighbours is

$$2^{-\frac{1}{2}} \times 3{\cdot}5 \times 10^{-8},$$

and the binding force $\partial J_0/\partial a$ is given by

$$z\,\frac{\partial J_0}{\partial a} = -\,4{\cdot}7 \times 10^{-6}.$$

Theoretical estimate of $\partial J_0/\partial a$. For two hydrogen atoms at distance a, Heitler and London* obtain for J_0

$$J_0 = \sum_{n=-1}^{3} A_n\,(a/a_0)^n\,.\,e^{-2a/a_0},$$

* W. Heitler and F. London, *Zeit. für Phys.* **44**, 455 (1927).

a_0 where the coefficients A_n are all of the same order of magnitude, and a_0 is the "radius of the electron orbits." Let us assume an expression of the same form for the exchange energy which enters into ferromagnetism. For the outer orbits in a crystal, a/a_0 probably lies between 1 and 2, and therefore $(a/J_0)\,\partial J_0/\partial a$ is of order of magnitude 1. It is impossible to determine the sign, but it might well be positive.

For iron and nickel the values of $(a/J_0)\,\partial J_0/\partial a$ are 0·9 and $-$ 0·6, and therefore the changes of length of these metals at the Curie point are of the order of magnitude required by the theory.

§7. RELATION TO MAGNETOSTRICTION AT ORDINARY TEMPERATURES

It has been pointed out that the Curie-point change of size is a continuous change, reaching completion only when the magnetization ζ becomes 1, and therefore extending over all temperatures below the Curie point. It is necessary to consider whether it can affect magnetostriction at ordinary temperatures.

In the magnetization of a ferromagnetic by the application of an external magnetic field, the primary effect of the field is to orient the direction of magnetization in each of the fully magnetized regions we have already discussed, the completion of the process being marked by the attainment of bulk-saturation. During the process, the small-scale magnetization does not alter appreciably. Further increase of the external field leads to a very small linear increase of ζ.

The greater part of the magnetostriction takes place during the first stage, i.e. before saturation is reached. The Curie-point change of size, being isotropic, is unaffected by changes of orientation, and so makes no contribution to the magnetostriction in the first stage. After saturation has been reached, the magnetostriction varies linearly with ζ^2, the greater contribution coming from the Curie-point change. The variations of the magnetostriction above saturation ought therefore to be approximately isotropic, even in single crystals, unlike the magnetostriction below saturation. The effect, however, is small, and I have been unable to find any experimental data.

§8. ACKNOWLEDGMENT

In conclusion, I should like to thank Mr R. H. Fowler, at whose suggestion this investigation was undertaken, for his interest throughout the work.

THE INTERRELATIONS OF MAGNETIZATION AND TEMPERATURE IN CRYSTALS

By W. PEDDIE

ABSTRACT. The paper deals with the question of an equation of thermomagnetic state. The possible existence of such an equation, in a form analogous to Van der Waals' thermo-mechanical equation of state, was indicated by Curie, and has been explicitly formulated and tested by Dr Ashworth. Two equations of a type different from that of Ashworth are discussed.

§ 1

The interrelations of magnetization and temperature in crystals, as in other bodies, can be worked out only in terms of the kinetic theory. In the cases of gases, liquids, or solutions, the difficulties are of no greater order than are those which beset the ordinary applications of the kinetic theory. But it is quite a different matter when we have to deal with a magnetic crystal, or a random congeries of crystals. Applications have been made by Frivold, but the mathematical difficulties were such as to compel limitation to extreme cases.

In addition to the mathematical difficulties, there is also uncertainty arising from ignorance of the constitution of molecules of atoms in respect of the origin of their magnetic qualities. Do the electrical circulations which give rise to these consist in the orbital motions of the exterior electrons, or have they a nuclear origin? Are they sub-orbital, consisting in the spin of the electrons themselves? There are some indications that the latter view may be correct.

§ 2

Just as in the initial development of the kinetic theory of gases, it was found necessary as a first step to postulate that the atoms were hard, smooth, spherical, elastic bodies, small in relation to their average distance apart; so, in magnetic theory, we have the initial postulate that the equivalent atomic magnets can be regarded as ideal magnets, of constant magnetic moment, and small in relation to their least distance apart on the crystalline lattice. And, just as these tremendously restrictive postulates of the theory of gases led, nevertheless, to important correspondences with observed results, so the immense restriction of the magnetic postulates does not prevent general applicability of the results to the properties of magnetic crystals. Difficulty in connection with the last postulate disappears if the

electrical circulation is that of the inner electrons, or is on a nuclear or sub-nuclear scale. And a test of the nature of the atomic, or sub-atomic, conditioning of the circulation may be founded on a comparison of observed effects with results obtained by retention, on the one hand, of the severely restrictive initial postulates, or, on the other hand, by their modification.

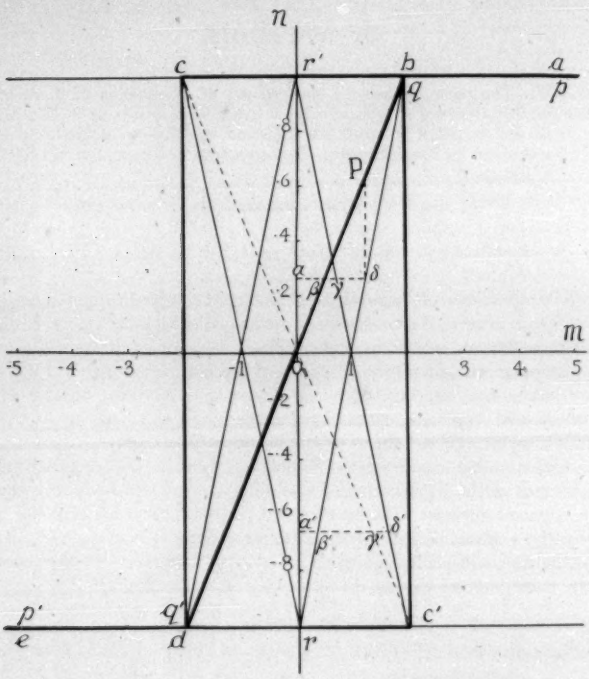


Fig. 1. Degree of saturation for varying external fields, when $l=0$.

§ 3

The general equation of thermomagnetic state, i.e. the law relating magnetic quality to the strength of the external field and to temperature, is, as Frivold found, outside present possibilities of attainment. It is therefore necessary to proceed by a process of averaging, just as Waterston and Joule did in ordinary kinetic theory. And, just as their procedure led to qualitatively correct results, so, in the case of magnetism, qualitative correspondence with observation may perhaps be expected.

If H be the component of the external field in the direction of uniform magnetization, while $F\cos\phi$ is the similar component of the internal field arising from the action of all the other co-directed molecular or atomic magnets upon any one of their number situated on the crystalline lattice, supposed to be infinitely extended

H

F

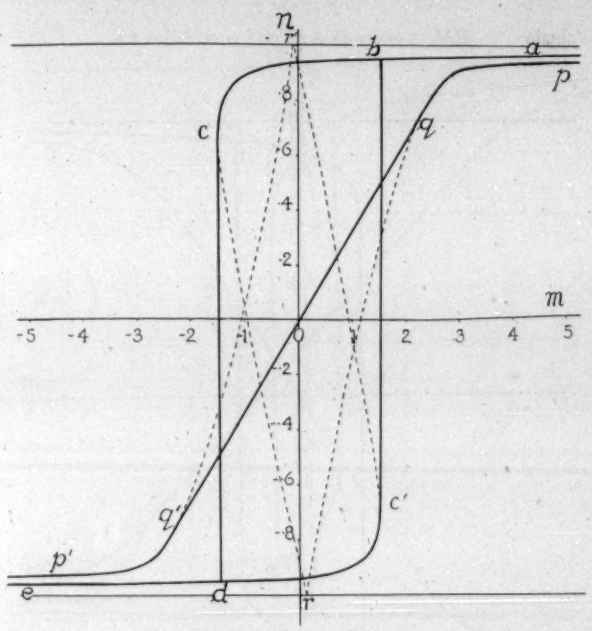


Fig. 2. Degree of saturation when $l=0{\cdot}1$.

and homogeneous, the work done by thermal action in deflecting every magnet from alignment with that direction through an average angle ϕ, is

ϕ

$$-\int_{\infty}^{\phi}(H+F\cos\phi)\,dM_0\cos\phi,$$

where M_0 is the fixed magnetic moment of each magnet supposed to be ideal. On the presumption that there is equipartitioning of energy amongst the magnetic and thermal freedoms this gives, provided that F is independent of ϕ,

M_0

$$HM_0(1-\cos\phi)+FM_0\tfrac{1}{2}(1-\cos^2\phi)=kT,$$

where k is the corresponding atomic gas constant and T the absolute temperature. If the condition of equipartition be not satisfied, k may differ from that constant,

k, T

and the results of observation seem to indicate that it has actually a considerably smaller value. The field F, proportional to M_0, is fixed whenever the space lattice and its mode of occupation by the atoms is given along with the direction of magnetization with reference to the lattice framework. We may therefore put

f $$F = 2M_0 f,$$

and so obtain $$HM_0(1 - \cos\phi) + M_0^2 f(1 - \cos^2\phi) = kT$$

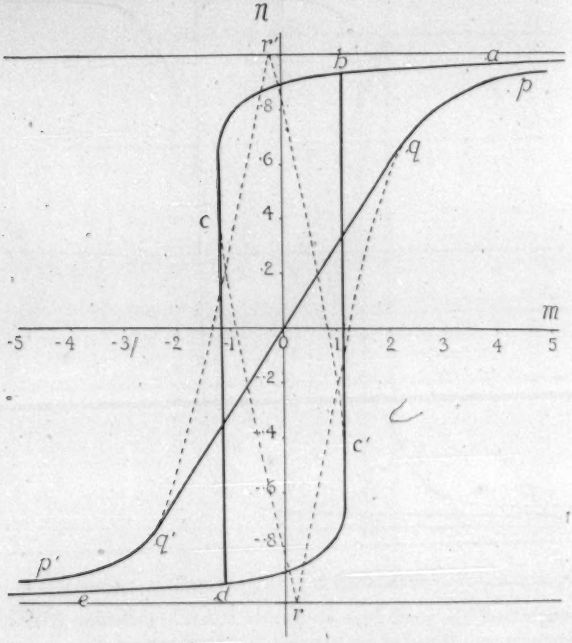


Fig. 3. Degree of saturation when $l = 0{\cdot}2$.

as the equation of thermomagnetic state on the assumption that we are dealing with an infinitely extended homogeneous magnetic crystal, and that the action of each magnetic atom is representable by that of a single small ideal magnet suitably situated.

I, I_0 Now ϕ being the average inclination of the magnets to the direction of magnetization, we have $\cos\phi = I/I_0$, where I is the intensity of magnetization and I_0 is its saturation value. Thus the equation becomes

$$HM_0(1 - I/I_0) + M_0^2 f(1 - I^2/I_0^2) = kT.$$

The value of f is calculable whenever the lattice is specified. There is a cone of directions, symmetrically related to the lattice, in which f is zero. Within that cone it is positive, and the internal field aids magnetization; outside it, the internal field opposes magnetization.

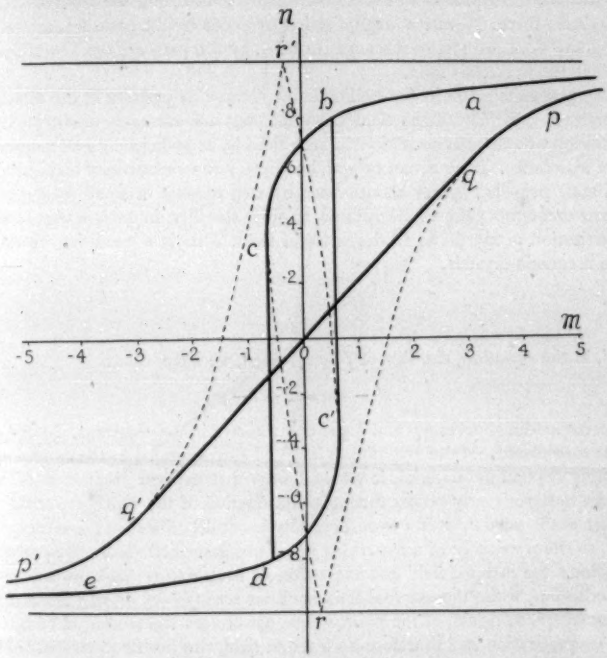


Fig. 4. Degree of saturation when $l = 0{\cdot}5$.

§ 4

The above equation is representable by a hyperbola referred to oblique axes if values of I/I_0 be plotted against values of H. In the accompanying figures the curve *abcr* represents the applicable portion, which must lie between the limits $n = \pm 1$, of the hyperbola

$$n^2 + mn = m + 1 - l,$$

where $m = H/M_0 f$, $n = I/I_0$ and $l = kT/M_0^2 f$. The values of l which are used in the various figures are given thereunder. The curve *edc'r'*, obtained by rotation of *abcr* through 180° round o, applies when the field and the magnetization are

m, n, l

reversed. The portions *cr* and *c'r'* of the curves, having negative slope, correspond to instability, and therefore not to actual conditions. If we take initially a strong positive field and magnetization, and gradually reduce both, the part *abc* is followed until, at *c*, instability ensues and the rectilinear part *cd* is instantaneously described. The ordinate *Ob* represents the residual magnetization, and the abscissa at *c* gives the coercive force. Negative magnetization proceeds by the path *de*; and, on withdrawal and final reversal of the negative field, by the path *edc'ba*. The closed loop *bcd'c'b* is the hysteresis loop.

One postulate made in the evaluation of *f*, taken as positive in the direction of magnetization, is that all the elementary magnets are averagely co-directed. Thus a condition of zero magnetization in zero field is, at sufficiently low temperature, one of instability. In an actual crystal, however, non-homogeneity on a sufficiently small scale probably brings about a step-by-step passage of small scale groups of magnets throughout the whole volume, to final stability, in such a way as to give magnetization proportional to the external field. This is a condition observed in single magnetic crystals.

§ 5

If, in the equation, the sign of *f* be reversed, we have

$$n^2 - mn = 1 + l - m,$$

the corresponding curves *pqr* and *p'q'r'* being shown in the diagrams. No hysteresis can be manifested. If the hypotheses adopted corresponded to the conditions in a randomly crystalline material, it would follow that cyclical dissipation of energy through hysteresis only occurs throughout a fraction of the whole material.

Just as the portion *cr* of a hyperbola *abcr* is not described by the representative point, so the portion *qr* of a hyperbola *pqr* is not described. Thus, for example, at the point *r* the internal field and the action of temperature tend towards positive magnetization, while the external field is either zero ($l = 0$) or also tends towards demagnetization. Again, at the point where *pqr* crosses the *m*-axis, although there is no magnetization and therefore no internal field, the positive external field prevents the crossing point from being a position of equilibrium. The external field corresponding to any given position on *qr* compels displacement of the representative point parallel to *On* until it reaches the line *Oq* which is tangential to *pqr* at *q*. The general conditions are readily traceable in Fig. 1 ($l = 0$).

Here the hyperbola coincides with the asymptotes *qp* and *qr*. At the point β, the positive magnetizing field $\alpha\beta$ is co-directed with the internal field $\alpha\gamma$, so that the total field $\alpha\delta$ and the negative magnetization are destroyed. At the point δ', representative of positive magnetization, the positive external field $\alpha'\delta'$ exceeds the demagnetizing field $\alpha'\beta'$, so that the resultant field $\beta'\gamma'$ causes additional magnetization, the course of which is represented by $\delta'P$. *P* is the position of stability, under the given external field, at which the external field is balanced by the internal field. The line *qq'*, together with the highly stable ranges *qp* and *q'p'*, extending to

infinity, represent the whole course of the tracing point when the internal field opposes the magnetization at zero absolute temperature.

In Fig. 2 ($l = 0{\cdot}1$) the value of the external field which corresponds to any given magnetization when the internal field opposes it is greater than that shown in the first figure. The difference of the corresponding abscissae in the two diagrams is

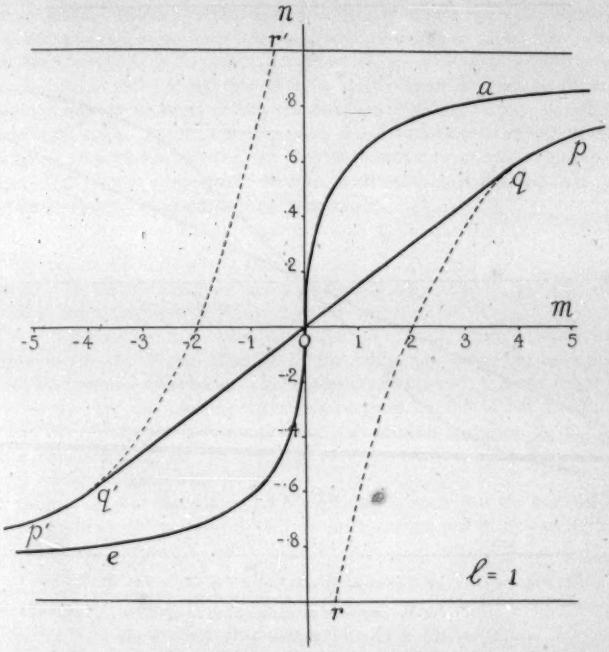


Fig. 5. Degree of saturation when $l = 1$.

the field equivalent of the temperature effect. Reasoning similar to that previously given indicates that the tracing point leaves the hyperbola at the point q and that Oq, tangential to pqr, gives the course of magnetization at weaker fields.

This seems to be in accordance with the results of observations on magnetic crystals. The parts of the observed paths adjacent to the origin appear to be linear, and to leave the curved paths tangentially.

Hysteresis still occurs when the internal field aids magnetization, but is reduced in magnitude. This reduction goes on progressively as the temperature rises until, finally, the hysteresis vanishes when, at $l = 1$, Fig. 5, the critical temperature is reached.

At a temperature higher than the critical value, e.g. $l = 2$, Fig. 6, both curves present the anhysteretic character and possess an initial rectilinear part.

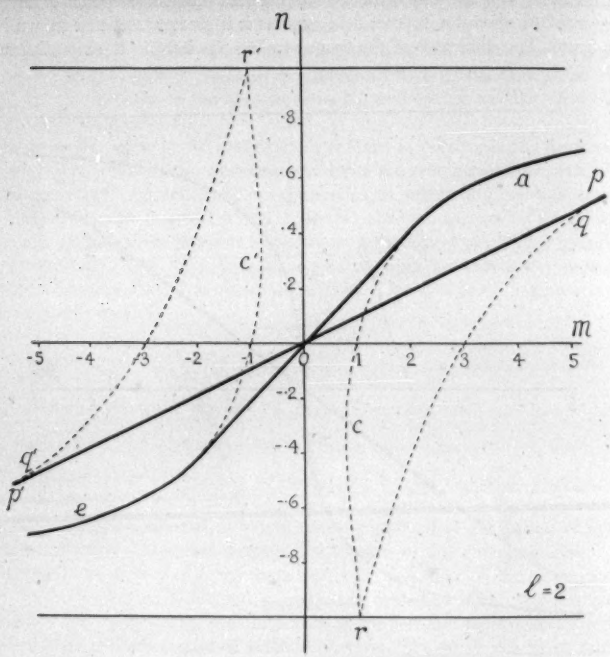


Fig. 6. Degree of saturation when $l = 2$.

§ 6

It appears that a good general account of the phenomena of magnetization in crystals, including cases involving instability, can be given by means of a postulate which involves an equation of the second degree only.

The equation

$$l = m(1 - n) \pm (1 - n^2)$$

is a reduced equation of thermomagnetic state in which the critical field is half of the maximum internal field, while the critical temperature is that at which the average molecular translatory energy is equal to the maximum work of reversal of magnetization in the critical field. Neither of these quantities is definite until the direction of magnetization in the crystal and the nature of the lattice is specified.

The mean of all the individually possible values would be the observable quantities in a randomly crystalline solid.

§ 7

It is necessary to note that the square of the quantity a_0, the semi-length of the molecular magnet, enters into the expression for the factor f in a cubic crystal. In the preceding work this has been postulated to be constant; or, rather, the postulate is that Ma_0^2, where M is the effective moment, is proportional to I, for it is made to involve $\cos\phi$. Now if we deal with an ideal magnet of moment M_0 which maintains randomly an angle ϕ with the direction of magnetization, the effective moment is $M_0\cos\phi$. But the effective value of a_0 is also altered; and, if we take it too as being proportional to $\cos\phi$, the internal field is proportional to $\cos^3\phi$. a_0 M

The effect of that assumption, together with another, is detailed in a paper published in 1927*. In equation (9) of that paper,

$$\left(m+\frac{n^2}{3}\right)\left(l-\frac{n}{3}\right)=\frac{8l}{9},$$

the factor 9 in the denominator has inadvertently been omitted, and this affects the scale of the accompanying figure.

The above equation is a generalized equation of state, which presents strong analogies to van der Waals' equation of thermodynamic state, but nevertheless exhibits fundamental differences. One of the resemblances is that the factor $8/27$ enters into the expression for the critical temperature; another is that the equation is a cubic in n. But the above equation was only put into that form by the choice of units which depend on the direction of magnetization. This limitation can however readily be removed.

If we assume that the molecular constitution is such that the internal field contains a term involving $\cos\phi$ as well as one involving $\cos^2\phi$, we can write the fundamental postulate as

$$kT=HM_0(1-\cos\phi)+M_0^2a_0^2f[\beta(1-\cos^3\phi)+\alpha(1-\cos^2\phi)], \qquad \alpha,\beta$$

and so obtain, by putting

$$\cos\phi=n,\quad H=mM_0a_0^2f\beta,\quad kT=lM_0^2a_0^2f\beta,$$

the reduced equation

$$n^3+n^2\frac{\alpha}{\beta}+nm-\left(\frac{\alpha}{\beta}+1+m-l\right)=0.$$

The conditions for three equal roots, n_c, are c

$$3n_c=-\alpha/\beta,\quad 3n_c^2=m_c=\alpha^2/3\beta^2,\quad l_c=(1+\alpha/3\beta)^2.$$

If we now take $\alpha=-\beta$, with $\beta=1$, we find

$$I_c=\tfrac{1}{3}I_0,\quad H_c=\tfrac{1}{3}M_0a_0^2f,\quad kT_c=\tfrac{8}{27}M_0^2a_0^2f,$$

or, taken per unit volume,

$$T_c=\frac{8}{27}\frac{I_0}{R}\cdot M_0a_0^2f.$$

* W. Peddie, *Proc. R.S.E.* **47**, 165 (1927).

Now Ashworth, in connection with his discussion of the thermomagnetic equation of state*, as based on the analogy of van der Waals' equation, finds†

$$T_c = \tfrac{8}{27} I_0 b,$$

b where b differs very little in numerical value from a small integer in the cases of iron, nickel, cobalt, and Heussler's alloy. But $M_0 a_0^2 f$ is the maximum value of the internal field, i.e. the value at zero absolute temperature; so that, on this view, Ashworth's result would appear to indicate that, in these substances, the maximum values of the internal fields are small integral multiples of the gas constant taken per unit volume. This points to fields of the order of 10^7 gauss, if there be equipartition amongst the thermal and the magnetic freedoms. But, if the latter be sufficiently shielded, the fields may be of ordinary magnitude.

* J. R. Ashworth, *Phil. Mag.* **30**, Nov. 1915; **33**, April 1917.
† J. R. Ashworth, *Nature*, Sept. 12, p. 397 (1925).

THE ATOMIC MOMENTS OF IRON COBALT AND NICKEL AS DETERMINED FROM THE MAGNETIC SATURATION OF THE FERRO-COBALTS AND NICKEL-COBALTS*

By Prof. PIERRE WEISS and Asst. Prof. R. FORRER,
Institut de Physique, Université de Strasbourg

ABSTRACT. The atomic moments of iron, cobalt and nickel have been deduced from saturation data relating to 25 ferrocobalts and 9 nickel-cobalts at ordinary temperatures and to their temperature-coefficients down to the temperature of liquid air. It is found that each of these moments may have several different values. The atomic moments of the alloys, as functions of atomic composition, exhibit discontinuities which are connected with changes in crystal structure.

§ 1. THE FERROCOBALTS

Preuss† has found that alloys whose composition is in the neighbourhood of Fe_2Co are more highly magnetizable than pure iron, their intensity of magnetization per unit volume at saturation being greater by about 10 per cent. This result has been confirmed and even exceeded by our own observations. For alloys containing from 35 per cent. to 45 per cent. of cobalt the saturation intensity per unit volume is practically constant, and is 12·4 per cent. greater than that of iron. The alloy with 28 per cent. of cobalt gives a practically equal difference, namely 12 per cent., in spite of the considerably lower proportion of cobalt.

The absolute saturation n, shown in the upper full-line curve in Fig. 1 as a function of atomic composition, may be defined as the average number of magnetons per atom of alloy, i.e. the specific intensity at saturation, at the absolute zero of temperature, divided by the number of gram-atoms of iron and cobalt per unit mass and by the magneton number 1125·6. n

The absolute saturation follows a law less simple than the two linear relations between Fe and Fe_2Co and between Fe_2Co and Co which were given by Preuss. In order that the absolute magnetization $\sigma_{0,\infty}$ may be derived from the specific magnetization $\sigma_{T,H}$ which obtains in a field H at absolute temperature T, a double extrapolation for $T = 0$ and $H = \infty$ must be effected. The formulae employed for this purpose are σ, T, H

$$\sigma_{T,H} = \sigma_{T,\infty}\,(1 - a/T),$$

and

$$\sigma_{T,\infty} = \sigma_{0,\infty}\,(1 - AT^2),$$

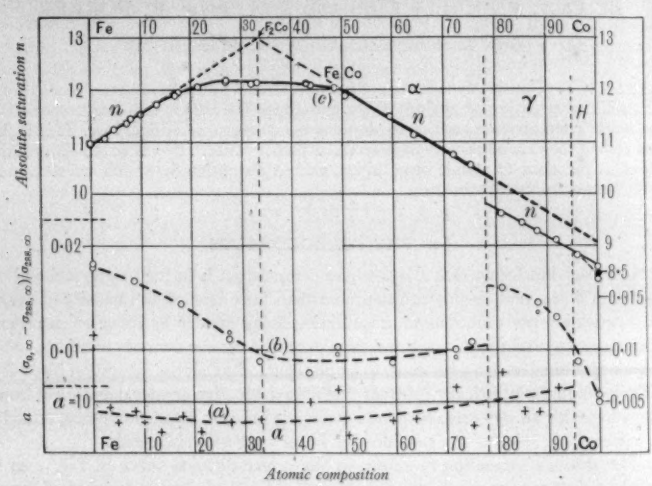
where a and A are constants. The experimental data concerned in these two formulae are shown by the two lower broken-line curves in Fig. 1. The extrapolated a, A

* Translated by the Editor.

† *Zurich thesis* (1912).

differences, which amount to less than 0·2 per cent., do not give rise to appreciable uncertainty.

The properties of the ferrocobalts are complicated by the fact that they crystallize in three different crystal lattices. At low temperatures for alloys containing from 0 per cent. to 78 per cent. of Co the stable structure is the centred cube (α-ferrocobalt), for those containing from 78 to 95 per cent. it is the face-centred cube (γ-ferrocobalt), and for those containing from 95 to 100 per cent. it is a hexagonal lattice of maximum density (H-ferrocobalt)*.



Atomic composition

Fig. 1. Ferrocobalts: (*a*) the temperature-coefficient a; (*b*) the quantity $(\sigma_{0,\infty} - \sigma_{288,\infty})/\sigma_{288,\infty}$, where the first suffix of σ denotes T and the second H; (*c*) the absolute saturation n.

When an alloy is produced by the mixture without modification of two metals of fixed atomic moment, the mean moment varies linearly with the composition. A non-linear variation indicates a condition in which at least three moments are concerned; for instance, it may arise from the presence of one constituent of fixed atomic moment and another occurring in variable proportions in two different states characterized by different moments.

There are in the α-ferrocobalts two regions in which the moment varies in a strictly linear manner, namely those in which there is from 0 per cent. to 13 per cent. and from 50 to 78 per cent. of cobalt. They are separated by a region, from 13 to 50 per cent., in which the variation is non-linear at high saturation.

The two straight lines meet at a point whose abscissa is 33·3 per cent., corresponding to Fe_2Co, while its ordinate represents 13 magnetons; their gradients are

* Hakar Masumoto, *Tohoku Sci. Rep.* **15**, 449 (1926).

equal and opposite. The first straight line begins at the known iron-point at 11 magnetons and, if we assume that the other constituent with fixed moment is cobalt itself and not a compound, we find by extrapolation a form of cobalt with 17 magnetons.

The second straight line reaches the abscissa corresponding to 100 per cent. of cobalt at the point where $n = 9$, with a degree of accuracy limited by the difficulties of analysis and by the fact that resort must be had to extrapolation beyond the α region. Cobalt, in α-ferrocobalts rich in that element, has therefore an atomic moment of 9 magnetons. If the straight line be produced as far as the iron ordinate, a form of iron with 15 magnetons is found.

In the γ region a further straight line may be observed which, when produced to the cobalt ordinate, gives us another moment that is less than 9 and may be estimated at 8·67. If this straight line be produced backwards to the iron ordinate, a moment of 14 magnetons is obtained for γ-iron. This result is confirmed by the fact that in the case of the ferro-nickels a linear relation observed by Peschard between Ni and Fe_2Ni_3 gives this same form of iron with 14 magnetons.

Pure cobalt in the *H* condition is magnetically hard on account of its anisotropy, and the observed values of the intensity of magnetization are too low. We have found in this case that $n = 8{\cdot}37$. On the other hand, tempered cobalt at temperatures above 470°, where the γ condition is stable, is a mixture of *H*- and γ-cobalts. Since γ-cobalt has a higher moment we obtain in this way a value of 8·60, which is too high. The interpretation of these facts has been yielded by a result obtained by Seiji Kaya* in a study of the magnetization of single crystals of *H*-cobalt. It is possible to deduce from his measurements the atomic moment of 8·5 magnetons in the direction of the hexagonal axis, which is an axis of high susceptibility. We shall find later a precise confirmation of this result in the investigation of the nickel-cobalts.

§ 2. THE NICKEL-COBALTS†

A preliminary study of the nickel-cobalts‡ indicated a linear variation of atomic moment with composition in samples containing from 0 per cent. to 70 per cent. of cobalt; the graph begins at the point where $n = 3$ and is directed towards the point where $n = 9$. From 70 per cent. onwards the alloys are very hard magnetically, and saturation could not be attained.

We now know the cause of this hardness. It is connected with the lower degree of symmetry which characterizes the alloys rich in cobalt; those containing from 68 to 100 per cent. of cobalt are stable at low temperatures in a hexagonal crystal lattice of maximum density (*H*-nickel-cobalts), while the alloys containing less cobalt crystallize in a face-centred cubic lattice (γ-nickel-cobalts)§.

* Seiji Kaya, *Tohoku Sci. Rep.* **17**, 1158 (1928).

† The measurements on nickel-cobalts were made by Francis Birch.

‡ P. Weiss and O. Bloch, *Comptes Rendus*, **156**, 941 (1911); O. Bloch, *Zurich thesis* (1912).

§ Hakar Masumoto, *Tohoku Sci. Rep.* **15**, 449 (1926); T. Kasé, *ibid.* **16**, 491 (1927).

As in the case of the ferrocobalts the data, Fig. 2, relating to the approximation to magnetic saturation, as a function of the applied field H and the absolute temperature T, are represented by the two lower broken-line curves. The difference between the saturation intensity at absolute zero and that at ordinary temperatures, expressed as a fraction of the latter quantity, varies smoothly throughout the interval. The coefficient of magnetic hardness varies smoothly in the γ region and reaches, in the H-nickel-cobalts, very high values of the order of 200, not shown in the figure. The extrapolations at $T = 0$ and $H = \infty$ can be made for the whole series of

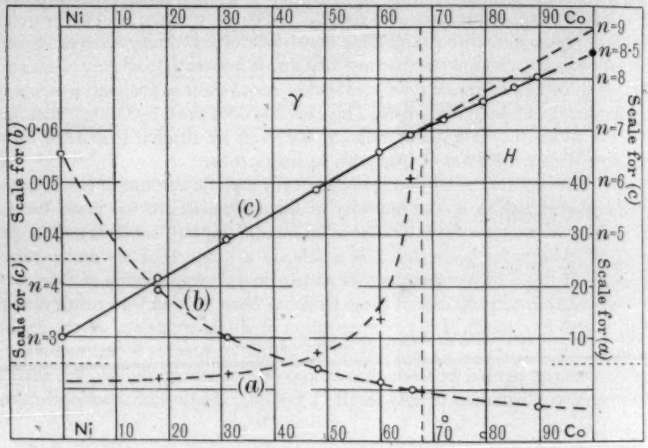


Fig. 2. Nickel-cobalts: (*a*) the temperature-coefficient a; (*b*) the quantity $(\sigma_{0,\infty} - \sigma_{288,\infty})/\sigma_{288,\infty}$, where the first suffix of σ denotes T and the second H; (*c*) the absolute saturation n.

alloys without the introduction of appreciable uncertainty. Only pure cobalt is too hard magnetically for the magnetization at $H = \infty$ to be obtainable.

The absolute saturation in magnetons is represented by the upper full-line curve in Fig. 2. It comprises two regions of linear variation in the regions γ and H. The first passes through the known point $n = 3$ for nickel, and points towards the integer $n = 9$ for cobalt. The second gives $n = 8{\cdot}5$ exactly for cobalt. This is precisely the moment which, in discussing the ferrocobalts, we have deduced from the measurements made by Seiji Kaya on a single crystal of H-cobalt in the direction of high susceptibility. When produced backwards as far as the nickel ordinate, this straight line gives a moment in the neighbourhood of 4, though the precision is not great on account of the distant extrapolation.

§ 3. CONCLUSIONS

To sum up, we may say that this investigation of the ferrocobalts and nickel-cobalts indicates that, like the ions of the iron group, the atoms of iron, nickel and cobalt are capable of assuming several different atomic moments. The change of moment is not always associated with a change in the crystal lattice.

Thus, in alloys belonging to different ranges of atomic composition, α-iron has been found with moments of 11 and 15 magnetons, and γ-iron with a moment of 14 magnetons. When cobalt occurs in a ferrocobalt crystallized in centred cubes it must be regarded as α-cobalt, a form which is unknown for pure cobalt. Each of the linear regions in the moment-curves of the α-ferrocobalts gives, therefore, a moment of α-cobalt. In this way moments of 17 and 9 magnetons have been found.

It is remarkable that cobalt with a face-centred cubic lattice, i.e. γ-cobalt, has not the same moment when the metal with which it is alloyed is iron as when this is nickel. In the first case the moment is 8·67, in the second it is 9. Even if the value 8·67, which is the less precisely known, were not rigorously exact, the fact would remain that the two moments are different.

In *H*-cobalt and *H*-nickel-cobalts the moment of cobalt is 8·5 magnetons.

From the measurements on the γ-nickel-cobalts the moment of nickel has been found to be 3, the value known long previously for the pure metal. The moment of nickel in the *H*-nickel-cobalts has been determined as 4, though with an accuracy which is not high on account of the distant extrapolation. Even if the latter numerical value be regarded as doubtful, it is nevertheless certain that nickel has two different moments in the nickel-cobalts.

Out of the ten moments which we have determined by means of these alloys, eight are integral multiples of the experimental magneton, 1125·6, and two are mixed numbers containing simple rational fractions, namely $8\frac{2}{3}$ and $8\frac{1}{2}$. It is known that the experimental magneton is, to within a few thousandths, one-fifth of a Bohr magneton. The atomic moments deduced from saturation data at low temperatures are, therefore, like those derived by spectrum-analysis, rational fractions of the natural unit furnished by the quantum theory. The general occurrence of the sub-multiple $\frac{1}{5}$ remains to be explained.

The study of ferrocobalts and nickel-cobalts brings out a further interesting fact. At the transition from the α-ferrocobalts to the γ-ferrocobalts, at a composition of 78 per cent., the atomic moment exhibits a marked discontinuity. On the other hand, the variation appears to be continuous at the transition from the γ-ferro-cobalts to the *H*-ferrocobalts, and certainly is so at the transition from the γ-nickel-cobalts to the *H*-nickel-cobalts. These facts may be compared with the change from 8 to 12 in the number of neighbouring atoms on transition from the centred cube (α) to the face-centred cube (γ), and the absence of change in the number (12) of neighbouring atoms on transition from the face-centred cube to the hexagonal lattice. Further, if the atoms are comparable to spheres of the same radius, the density increases on transition from the centred cube to the face-centred cube and remains unchanged on transition from the face-centred cube to the hexagonal lattice.

A NEW RELATION BETWEEN MAGNETIC AND ELECTRIC PHENOMENA*

By Prof. WALTHER GERLACH, München

ABSTRACT. The paper describes investigations of (*a*) the variation with temperature of the temperature-coefficient of the resistance of nickel, this quantity being related to the specific heat; (*b*) the resistance-changes produced in nickel by the application of a longitudinal magnetic field; and (*c*) an e.m.f. which arises when a ferromagnetic body, placed in a magnetic field, is subjected to a temperature gradient.

§ 1. INTRODUCTION

The relations between electrical and ferromagnetic properties have again obtained special significance, since the theoretical treatment of the two problems is relevant to the same fundamental treatment by wave mechanics of the electrons in metal. Three questions of this kind have lately been studied more closely in my Institute: (*a*) the change of the resistance with temperature; (*b*) the change of the resistance through a homogeneous longitudinal magnetic field; (*c*) a newly discovered thermogalvanometric effect: a potential difference which arises through the longitudinal magnetic field in a ferromagnetic conductor in the presence of a temperature difference.

Special attention was given to the fact that all measurements must be carried out on the same wire, and in each electrical measurement the magnetization curve of the same wire was also taken. Experiments have so far been carried out on nickel.

§ 2. THE CHANGE OF RESISTANCE WITH TEMPERATURE

In this connection a very large number of measurements are already available. The established fact is: at temperatures above the Curie point nickel behaves like any other metallic conductor, in that the temperature-coefficient of the resistance is constant for a greater range of temperature. Immediately below the Curie point the resistance very quickly falls with falling temperature. Fig. 1 represents such a curve. The measurements so far made are not sufficient for the exact determination of the true temperature-coefficient of the resistance, above all in the neighbourhood of the Curie point.

The true temperature-coefficient as a function of the temperature was determined by a sensitive differential method, and the resistance-change was measured with temperature-increments of only a few degrees (about 1°–3° C.) between 0° C. and 400° C. As a result a curve of the temperature-coefficient as a function of the temperature was obtained, as shown in Fig. 2 (full line). The course of this curve

* Translated by H. C. Booth, National Physical Laboratory.

agrees everywhere very closely with the known curve as given by P. Weiss for the true specific heat in nickel as a function of the temperature (dotted curve in Fig. 2).

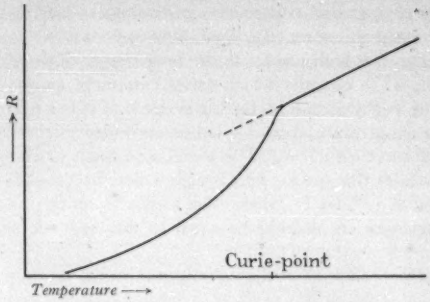


Fig. 1. Resistance as a function of temperature.

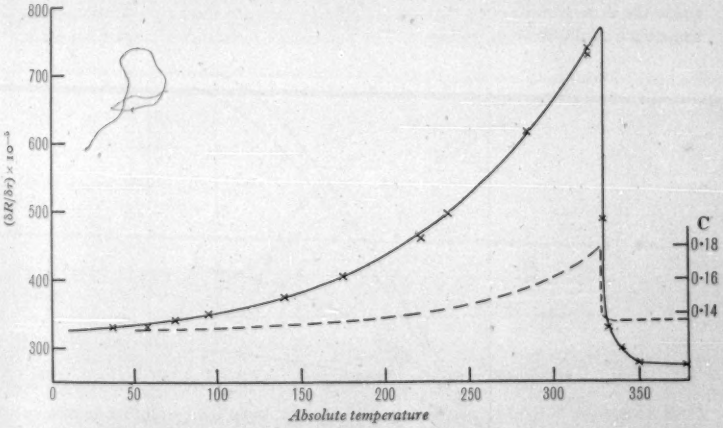


Fig. 2. ×× Temperature-coefficient as a function of the temperature. --- Specific heat.

The physical significance of the relation found for dR/dT as a function of the absolute temperature T is seen from the following considerations. The resistance can be dimensionally represented in accordance with the simple electron theory by $R = A.E$. Here A is a quantity which contains the free wave-length, the charge

R

T

A, E

and the velocity of the conduction of electrons. We therefore assume that the well-known Dorfman* experiment indicates that the magnetic energy represents an increment in the energy of the conducting electrons. For a ferromagnetic body, therefore, E is to be regarded as consisting of two parts, one, E_0, independent of the magnetization and the other, E_m, dependent upon it.

E_0
E_m

The differential of R with respect to the temperature T therefore gives in the quantity $-A.dE_m/dT$ a quantity which varies extremely quickly for changes of temperature in the neighbourhood of the Curie point. If A and E_0 are to be regarded as dependent on temperature, then this temperature-dependence is single-valued. The course of the curve $dR/dT = f(T)$ is therefore actually given by dE_m/dT. The direct connection with this specific heat becomes clear because dE_m/dT, according to Weiss, is equal to $\frac{1}{2}N.d\sigma^2/d\tau$, where N is a constant of the internal field, σ the spontaneous magnetization, and this is equal to the magnetic part of the true specific heat.

N, σ, τ

The quantitative evaluation of the investigation is not yet finished.

§3. RESISTANCE-CHANGE OF NICKEL IN LONGITUDINAL FIELD

(*a*) *At temperatures far below the Curie point.* The resistance-change in the longitudinal field as a function of the magnetizing field-strength and of the temperature is especially well-known through numerous English investigations. We have made the arrangement such that we are able to measure the magnetization at the same time as the resistance-change. The percentage resistance-change from 0·1 to

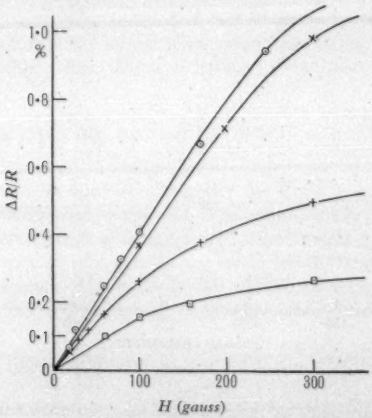


Fig. 3. Magnetic resistance-changes as a function of the field-strength. (The lower curves correspond to the higher temperature.)

* J. Dorfman and R. Jaanus, *Zeit. für Phys.* **54**, 277 (1929).

1 per cent. can be measured with an accuracy of a few per cent. There was no uniform connection of the magnetization with the magnetic resistance-change. In weak fields the magnetization increases much more quickly than the resistance-change. The resistance-change is here noticeably proportional to the external field-strength (Fig. 3). On the other hand the resistance-change of the magnetization is linear from about the beginning of the knee of the magnetization curve up to

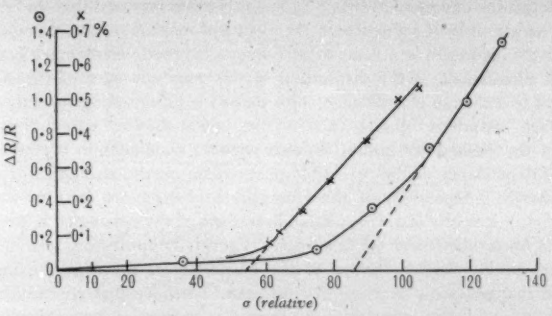


Fig. 4. Magnetic resistance-change as a function of the magnetization.
⊙ Temperature at about 290° K.
× ,, ,, ,, 470° K.

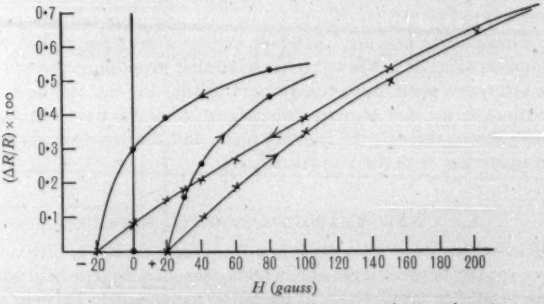


Fig. 5. Hysteresis: ··· magnetization, × × resistance-change.

saturation. Fig. 4 gives as an example the percentage change of resistance dR/R as a function of the field and of the magnetization for about 20° C. and 200° C. If the straight parts are extrapolated till the line cuts the σ_0 axis, one obtains at various temperatures the value of σ_0 whose product by the absolute temperature, $\sigma_0 T$, is a constant.

The change of resistance shows a hysteresis which is much smaller than the hysteresis of magnetization. Fig. 5 gives an example of this.

The linear relation between change of resistance and magnetization above the knee, and relative to small resistance-changes in the case of weak magnetization, shows that only a certain part of the ferromagnetic magnetization phenomena are connected with a change in the resistance. I would like to put forward the hypothesis that here a distinction between reversible and irreversible magnetization phenomena is shown, which in R. Forrer's work plays a great rôle in the course of the magnetization curves. According to this it is to be assumed that the irreversible phenomena are without influence on the electrical resistance, and therefore on the phenomena which exist in a rotation of Forrer's magnetic elementary bodies. The reversible phenomena of the disposition of the magnetic axes of the elementary body leads to a change of resistance. This indeed is illuminating for only the latter part implies "magnetic" change of structure, whilst the first part indicates only a change of the elementary bodies as such without modification of the structure. Since the dependence of the reversible phenomena on the strength of the field is not absolutely independent of the irreversible phenomena (on account of the internal field), it is obvious why a small hysteresis of the resistance is still present.

(*b*) *At temperatures near the Curie point.* Especially interesting relations present themselves in the neighbourhood of the Curie point, for here the increase of resistance changes into a decrease in resistance. I believe that we can also understand this effect in connection with the magnetic properties in the neighbourhood of the Curie point*. Here, as a result of an external field, a true magnetization is created. Therefore through an external magnetic field an internal magnetic condition is created which, without an external field, is only present at low temperatures. According to Fig. 1 the resistance below the Curie point falls off sharply with increasing temperature: one may therefore expect a decrease of the resistance through magnetization. It is also to be remarked that this diminution of resistance exhibits no saturation point, but a steady increase with the external field.

The relations in the case of transverse magnetization we have not considered; they are complicated through the Lorentz force, and therefore we cannot expect any simple connection with the magnetization.

§4. A NEW THERMOMAGNETIC EFFECT

If a fall of temperature takes place in a ferromagnetic body, then an electromotive force appears between its ends in a homogeneous magnetic field whose lines of force run parallel to the direction of the fall of temperature. This electromotive force shows about the same degree of dependence on the magnetization as the change of resistance.

T_1 T_2 If a piece of nickel wire is kept at a temperature T_1 of about 20° C. (see Fig. 6), and if the temperature T_2 of the other end is raised, then a magnetic field parallel to the lines of temperature-fall creates an electromotive force which (cf. Fig. 7 *A*) steadily increases and reaches an approximately constant value as soon as T_2 exceeds the Curie point. If T_1 be now increased while T_2 is kept at any given

* W. Gerlach, *Zeit. für Phys.* **59**, 847 (1930).

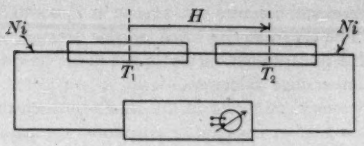


Fig. 6. Diagram of experimental arrangements.

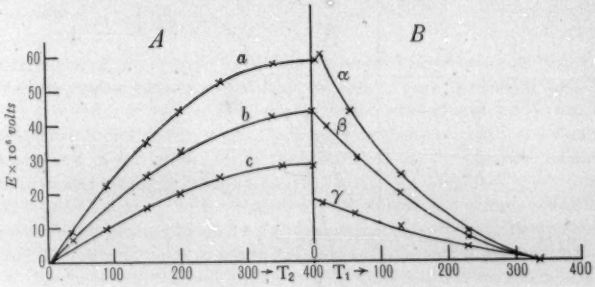


Fig. 7. E as a function of the temperature difference.

A: $T_1 = 15°$ C.; a 416 gauss; b 208 gauss; c 104 gauss.
B: $T_2 >$ Curie point; α 520 gauss; β 208 gauss; γ 62 gauss.

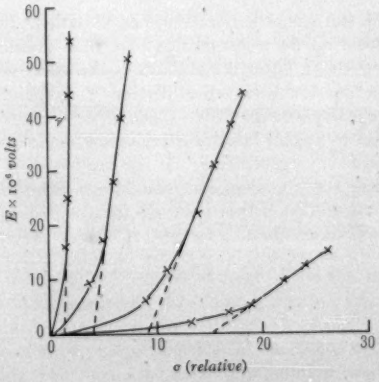


Fig. 8. E as a function of the magnetization.

temperature above the Curie point, then the electromotive force created by the magnetic field decreases and becomes zero as soon as T_1 is equal to the temperature of the Curie point. The electromotive force remains constant while the field effect remains constant. It is independent of the course of the temperature fall and only dependent on the temperature difference.

This effect is obviously connected in the same way with the magnetization as is the change of resistance at constant temperature: compare the course of the curves of field-strength of $\Delta R/R$ and E in Fig. 3 and Fig. 7 *A*, but particularly

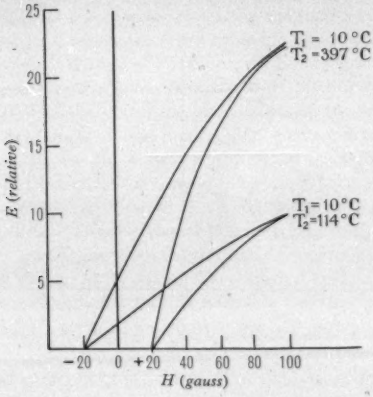


Fig. 9. Hysteresis of the longitudinal electromotive force E.

a dependence of the two magnetic effects on magnetization. In Fig. 8 the values of Fig. 7 as dependent on the magnetization have been transferred: here also at high values of σ we have a linear law connecting the effect with magnetization. Also the small hysteresis of the new effect perfectly corresponds to the hysteresis of resistance-change (compare Fig. 9).

We may consequently assume that this effect is also connected with the reversible part of the ferromagnetic magnetization. So far, the results of my investigation, which I have carried out in conjunction with Messrs Schneiderhan and Broili, show a very close connection between the electrical and magnetic phenomena; we hope that they will contribute to a further explanation of ferromagnetism.

METHODS OF EXPERIMENTING IN STRONG MAGNETIC FIELDS

By P. KAPITZA, Ph.D., F.R.S.

ABSTRACT. The paper describes the design of apparatus for the production of intense magnetic fields of brief duration, and indicates some of the experiments which were made with such fields.

The model of the atom as now given in modern physics is essentially of an electrodynamic character, that is, it consists of moving charged bodies. All the properties of a given atom are due to the number and way in which the electrons move round the nucleus. By disturbing this motion by any means we shall alter most of the properties of the atom such as, for instance, its spectra, coherence forces, magnetic moment, etc.

The magnetic field is the most efficient disturbing agent of the motion of the electrons, and it is of the greatest interest and importance to study the disturbances produced by magnetic fields in individual atoms and in atomic aggregates such as crystals. For instance, it is well known that the results which were obtained from the study of the Zeeman Effect led to the classification and understanding of the spectra of atoms. There are quite definite indications that the most interesting region of the magnetic disturbance of the atom is reached when the strength of the magnetic field attains the same value as the field in the atom produced by the moving electrons. If an atom is exposed to such magnetic fields the motion of the electrons will be altered very markedly, as the coupling energy between the electrons will be of the same order as the perturbation produced by the field. However, if we begin to consider the magnitude of these magnetic fields we find that even for the most loosely bound electrons it should be of the order of 1,000,000 gauss. Ordinary methods of obtaining magnetic fields, such as the use of electromagnets, limit the field to a value not much higher than 50,000 gauss. This limit is due to the fact that iron gets saturated. The magnetic field produced by electromagnets increases very slowly in strength with increasing size of the magnet. The largest magnet ever built is that of Prof. Cotton, which is a most wonderful engineering construction of enormous size. The diameter of the iron cores reaches 1 metre and a man can easily stand between the pole pieces, whilst a special power station is required to supply the required current. Even this huge electromagnet cannot produce a field much stronger than 60,000 gauss in a space sufficiently large to make experiments.

The importance of strong magnetic fields led the author to attempt to develop a method of obtaining them by making the time of duration very short. As we shall see later, by means of the sacrifice of the length of time of existence the magnetic

fields can be made very much stronger, and a large region of important physical investigation can be covered.

The general idea of the method is as follows: If you take a coil and pass a current through it, then the magnetic field produced in the coil will be proportional to the current, but the increasing of the current is stopped by a natural limit which is due to the heat produced in the coil by the current. To reduce this heating two methods can be used; the first is to cool the coil to a very low temperature, when the resistance is reduced considerably, and even in certain metals to zero, when the metal begins to be supra-conducting. A difficulty in this case will be that the magnetic field produced by the coil will destroy the supra-conducting state and also very rapidly increase the resistance to a value very close to that at room temperature. The second method is to cool the coil by carrying away the heat; but still experiments and calculations show that even with the most efficient cooling of the coil it is difficult to get a field of more than 50,000 or 60,000 gauss.

The principal idea of the author's method is to produce the magnetic field for such a short time that the coil has practically no time to overheat. We have chosen this time to be of the order of 0·01 sec. For instance, if we want to produce a field of 1,000,000 gauss in a very efficiently designed coil with an inside diameter of 1 cm., a power of 50,000 kilowatts is required; in one second the coil will be heated to 10,000° C., and to keep the coil cool by means of water-cooling is practically impossible. If, however, we allow the current to pass for only 0·01 sec. the coil will only increase in temperature by about 1000° C., a quite permissible temperature rise.

There are a number of experimental difficulties which still lie before us in obtaining and applying this strong magnetic field of short duration: the first one is the large amount of power necessary. It is evident that it is an easier and cheaper proposition to obtain the huge amount of power for a short time than to use it continuously. We have found two methods suitable for our experiments. Our first method was to use accumulator batteries; these specially made accumulators had a very small capacity and a small internal resistance, and were rather rigidly built. By charging these accumulators for a few minutes and discharging them in 0·01 sec., we could easily obtain impulses of current up to 2000 or 3000 kw. All the preliminary experiments were carried out with these accumulators, and fields of 100,000 gauss were produced, but further increase in magnetic field was difficult as it was found rather difficult to break sufficiently suddenly the continuous currents of several thousand ampères supplied by accumulators. Consequently, when it was decided to go to larger powers the accumulators were replaced by a special generator. The generator chosen was similar to a single-phase a.c. turbo-alternator. This type was found to be of great advantage for obtaining the strong impulses required in our experiments. Firstly, the armature can easily be made very strong, and as it makes a large number of revolutions it possesses a good amount of kinetic energy, so that when the generator is short-circuited for 0·01 sec. sufficient kinetic energy is available to be converted into electrical energy. Secondly, with an a.c. machine only a half wave of the current is used, and if a synchronously adjusted break is

adopted, it is possible to interrupt the current at a point very close to zero, and the problem of the interruption of the current is thus simplified.

It is well known in electrical engineering practice that this type of machine when short-circuited can deliver very large impulses of current, and usually these machines are made in such a way that large currents cannot be obtained by an accidental short-circuit, which might otherwise result in a serious accident. The machine which we used, however, was designed in the opposite way: it is actually built to give these large impulses on being short-circuited. This required a considerable

Fig. 1. Perspective view of apparatus.

revision in the design of the machine, and it was especially important to consider all the electrodynamical forces which occurred, since they might easily result in a mechanical breakdown of the windings. The machine actually constructed was of the size of the usual turbo-alternator for delivering 2000 kw. at continuous rating. On being short-circuited on the test bench it gave us 220,000 kw. When it is short-circuited on a coil of equal impedance only half of the power will be available: half of it will be lost in the machine and the other half will go to the coil. In this way the required 50,000 kw. are obtainable. The machine is shown in Fig. 1.

The only drawback of using such a machine is that the current never remains constant in the coil, and thus the magnetic field also will vary as is seen on oscillogram Fig. 2. However, it was found possible with a certain design of armature to give such a shape to the excitation magnetic field that the output

current wave had a flat top, and during several thousandths of a second the magnetic field actually remained constant as seen on oscillogram Fig. 3.

The second problem we had to face was the designing of a special switch to make and break the current synchronously with the current wave. This was rather a complicated engineering problem, as the current had to remain on for only 0·01 sec. and the time we had for making or breaking would be a few ten-thousandths of a second, during which time a copper plate had to separate by several millimetres from a brush. The acceleration required to move the copper plate 1 kg. in weight this distance is about 1000 times that of the gravity field, and the force required is over a ton. An exceptionally strong and carefully designed cam shaft mechanism is used for this apparatus. There are also several accessory parts used during the break of the current, such as condensers to avoid over tension during a too sudden break and an air blast in the gap of the switch to make the break more efficient. The switch is shown in Fig. 4.

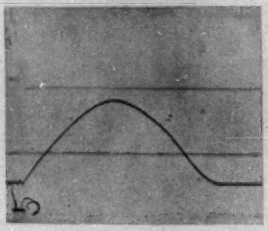


Fig. 2. Current in coil, with ordinary excitation field.

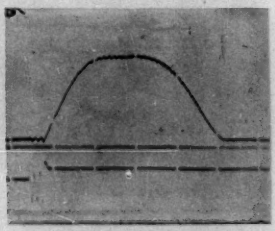


Fig. 3. Current in coil, with excitation field designed to give flat-topped wave-form.

The most difficult part of the whole apparatus to design was the coil itself, in which the magnetic field was produced. The density of the current in this coil reached a value of 500,000 amp./cm.^{2}, and the electrodynamical forces which try to enlarge the diameter of the coil are so large that at first most of our coils broke with a great explosion. It was necessary to work out a method to reinforce these coils with steel bands and to find a coil of such a shape that the electrodynamical forces together with the reaction forces of the reinforcement would be reduced to a uniform (hydrostatic) pressure on the copper. The force on the outside reinforcement of the coil which is actually now in use, and in which fields of over 300,000 gauss have been obtained, reached the value of 140 tons.

There are also a number of automatic devices and oscillographs which are used to record the currents in the coil, and thus to measure the magnetic fields. The experiments are carried out automatically by a number of timing devices which all act on the pressing of a single button. The apparatus is placed in a large hall at one end of which is the generator which is accelerated by a 80 h.p. electric motor to the required speed. The strong current is brought to the other end of the coil by means

of six thick cables. The distance between the machine and the coil has been chosen to be 20 metres for the following reason: when the machine is short-circuited the angular velocity of the armature, which weighs $2\frac{1}{2}$ tons, is reduced by 10 per cent. in 0·01 sec. This results in a strong couple which tends to turn the whole machine on its foundation and produces a shaking of the ground. It is evident that it would be very awkward, at any moment when an experiment was being made, for such an earthquake to disturb the apparatus, but by placing the machine far from the coil we bring it about that the earthquake actually arrives at the coil and measuring-apparatus after 0·01 sec., when the experiment is all over.

At present we have limited our experiment to field strengths up to 300,000 gauss, which can be reached in a volume of 3 cm.^{2}.

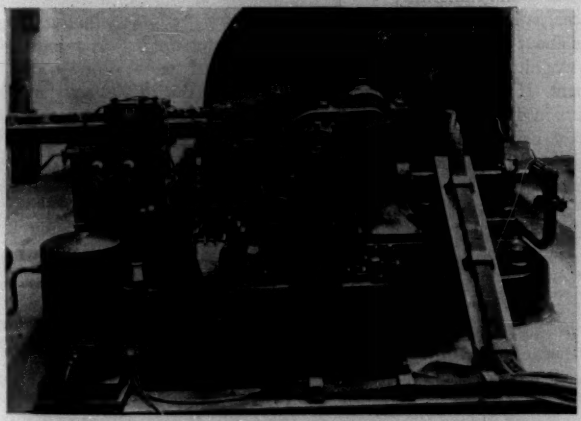


Fig. 4. Synchronous circuit-breaker.

We have now to consider the methods used to make experiments during 0·01 sec. It is evident that for the magnetic phenomena occurring in the atom this is a very long time, and all these phenomena have plenty of time to establish themselves. The difficulty is only in finding the methods of observing and measuring these phenomena. Actually, however, the loss in time in most experiments is compensated by gain in magnitude of the phenomena observed. For instance, in the study of the Zeeman Effect, the splitting of the lines was so large that an ordinary prism spectograph could be used which had a large luminosity, and the time of exposure could be reduced to 0·01 sec. In ordinary weaker magnetic fields the time of exposure is sometimes several seconds, and échelons or gratings of large dispersive power, which have a much smaller luminosity, have to be used.

Up to the present, besides the Zeeman Effect, we have covered the following ground in magnetic research.

We have studied the change of resistance of various metals in magnetic fields. Usually the change of resistance amounts to a small fraction of 1 per cent., but in our case, with the strong magnetic fields, the resistance of most of the metals increased by 20 to 30 per cent.* and could be measured by an ordinary oscillograph, especially as for these short times large currents could be sent through the samples of metals without any danger of heating them. It was found in this case that in strong magnetic fields the law of change of resistance was different from that in weaker fields. In weaker fields the resistance increases proportionally to the square of the magnetic field; in stronger fields we found that a linear law obtained.

We have also measured the susceptibility of certain metals. For this purpose a special balance was made having a frequency of about 2000 to 3000 ~. This balance was found sufficiently sensitive to measure the susceptibility of most of the substances, as in the case of strong magnetic fields the force which acts on a substance is about 100 times larger than usual, and amounts to several grams.

The magnitude of the force reached in strong fields can be illustrated by a simple experiment. If we place in the coil a Dewar flask filled with liquid air, and if a glass rod 3 mm. in diameter is placed in the flask, the liquid oxygen in liquid air, owing to its strong paramagnetism, is pulled into the Dewar flask when the field is on, and the glass rod is forced out, the force being sufficient to throw the glass rod to a height of about 7 or 8 metres. If the Dewar flask is filled with liquid oxygen only, the force of attraction due to a field of 300,000 gauss produces a pressure of several atmospheres and the Dewar flask breaks.

It was also found possible to make an apparatus to study the magnetostriction in metals during short periods. At present this phenomenon is only known for ferromagnetic substances, but in strong magnetic fields we find it specially marked in bismuth and some other substances, such as tin and graphite, which have a crystal structure of a low symmetry. We found that in bismuth crystals in strong magnetic fields the crystal increases in length in the direction of the trigonal axis, but decreases perpendicularly to it.

The great advantage of studying these phenomena in 0·01 sec. is that the main disturbance, which comes from the temperature change, is avoided, as during this short space of time the temperature is practically constant.

A very good example of the use of short duration fields with good success was afforded by the application of these fields to the α-ray tracks obtained in a Wilson expansion chamber. In strong magnetic fields, owing to the charge of the α-particle, the track is bent, and by measurement of the curvature along the track it is possible to find out how a single α-particle loses its velocity when passing through a gas.

Thus we can see that the magnetic field of short duration has a wide scope of application and can be used in nearly every case where the experiment is possible in ordinary magnetic fields, but special apparatus and methods have to be devised for the work.

* The resistance of bismuth increases in certain cases 2000 times.

MAGNETOSTRICTION AND CHANGE OF RESISTANCE IN SINGLE CRYSTALS OF IRON AND NICKEL

By W. L. WEBSTER, Ph.D.,

Clerk Maxwell Scholar of Cambridge University

ABSTRACT. The process of magnetization must consist of two parts, first the switching over of the magnetization in small elements of volume from one direction of easiest magnetization to another, and then a gradual change from this direction to that of the applied field. These two stages involve different distortions of the crystal, and an attempt has been made to associate with them the principal characteristics of the phenomena of resistance-change and magnetostriction.

§ 1. INTRODUCTION

In two recent papers Akulov* has shown, for a single crystal of iron magnetized to its saturation value, that it is possible to account for the longitudinal and transverse changes of length accompanying changes in the direction of the magnetization relative to the crystal structure by means of forces arising from magnetic dipoles. And further, he has shown that there is a direct proportionality between the energies involved in such length changes and the energies required to produce the corresponding change of direction of the magnetization, the latter forces being the quadrupole terms postulated by Mahajani†. This must mean simply that the dipole and quadrupole effects are both due to the deviation of the same carrier of magnetic moment from its normal position of minimum energy in the crystal.

Akulov made no attempt to deal with magnetostriction in the region below complete magnetic saturation. In this paper an attempt is made to give a qualitative explanation of the effects in this unsaturated state.

§ 2. THE DEMAGNETIZED STATE

In order to deal with the unsaturated state it is necessary to know the magnetic condition of the metal in an apparently unmagnetized state. The phenomenon of spontaneous magnetism‡ leaves little doubt that an apparently unmagnetized body at ordinary temperatures consists of small volumes magnetized in different directions but adding up to give a zero magnetic moment to the whole. The orientation of the direction of magnetization of these small volumes cannot be entirely at random, for the magnetic intensity in each must be very nearly the full intensity in equilibrium with the temperature, and the Mahajani molecular field must therefore orientate

* N. Akulov, *Zeit. für Phys.* **25**, 3ℰ9 (1928); **54**, 582 (1929).

† G. S. Mahajani, *Phil. Trans.* **228**, 63 (1929).

‡ P. Weiss and R. Forrer, *Ann. de Physique*, **5**, 153 (1926).

them, as far as is consistent with a continuity of flux, along the directions of easiest magnetization. For iron the magnetization will be along the three cubic axes, and as these must all be equivalent, it can be assumed that there are equal volumes magnetized along each. The details will admittedly not be perfectly sharp, but the actual condition must be a close approximation to that described above.

The reason for the existence of such a chaos lies in the demagnetizing force due to the external shape of the body under consideration. This is a purely accidental effect from the point of view of the mechanism of ferromagnetism, and one must imagine that, could it be eliminated, the normal state of a ferromagnetic body in the absence of any external field would be complete saturation parallel to one of the directions of easiest magnetization. The unmagnetized state is essentially unnatural to a perfect, uniform, ferromagnetic crystal from which all extraneous influences have been removed. For such a crystal the only important variable would be the direction of magnetization, as the magnetic intensity at ordinary temperatures can only be altered to a comparatively small extent and that only with very large magnetic fields. Akulov then, who calculates the change of length accompanying a change in the direction of magnetization from its normal (100) direction, and deals only with complete saturation, gives what must be considered a reasonable description of the phenomenon of magnetostriction in an ideal crystal at low temperatures. He finds naturally that the results are in satisfactory agreement with the measurements on single crystals at ordinary temperatures, where the intensity of spontaneous magnetization is still within 2 or 3 per cent. of its maximum value, and under such conditions (i.e. with an external field sufficiently large to annul the demagnetizing force completely) that a state of uniform saturation is produced.

For the purposes of the present paper, which will discuss effects occurring in the region where the bulk magnetization is not complete, it is necessary to decide what the actual state of the body will be for different degrees of magnetization. It will then be possible to investigate the effect of the departure from the ideal state on the various phenomena which occur in this region.

§ 3. THE PROCESS OF MAGNETIZATION

An unmagnetized crystal of iron has already been described as consisting of an aggregate of a large number of small elements, each fully magnetized, but so oriented at random along (100) directions that there is no resultant moment. When bulk magnetization is made to appear, by the application of a magnetic field, some of these small elements must change their direction of magnetization in order to produce the resultant moment. This change of orientation may take place in three different ways. The magnetization may be reversed; it may change from one cubic axis to another; or it may depart altogether from a cubic axis. The first two of these processes are essentially the same. They may require a small magnetic force to break up the stability of the circuits of magnetic flux which must exist in unmagnetized iron, but no further work, as the change of direction of magnetization from one cubic axis to another involves no net change in the energy of the crystal. The third process, however, brings into play the Mahajani molecular field and requires fields

of the order of several hundred gauss. There will however be no hysteresis loss as in a complete cycle of magnetization the Mahajani force produces no change of energy.

The coercive force and hysteresis loss that are observed even in single crystals probably correspond to the energy lost in breaking up the circuits of flux in the iron. It is found* that the more nearly perfect the crystal the smaller is its hysteresis loss, a result due possibly to the encouragement of larger circuits of flux with a corresponding decrease in their stability.

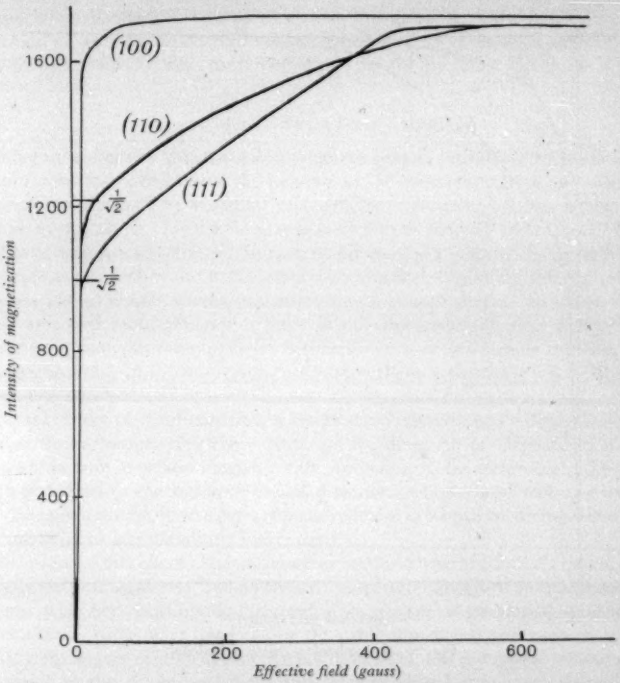


Fig. 1. Magnetization curves for iron.

There are, then, two fundamentally different ways in which bulk magnetization occurs. The first is a change from one cubic axis to another and takes place in fields of a few gauss†; the second is the departure from a cubic direction and occurs only

* K. Honda and S. Kaya, *Sc. Rep. Tohoku*, **15**, 728 (1926); W. Gerlach, *Zeit. für Phys.* **39**, 327 (1926).

† W. Gerlach, *Zeit. für Phys.* **38**, 828 (1926); K. Honda, H. Masumoto and S. Kaya, *Sc. Rep. Tohoku*, **17**, 118 (1928).

in comparatively large fields. These may be associated respectively with the initial rapid rise up to and the slow increase after the knee of the ordinary I/H curve. That this view is substantially correct is borne out by the fact that the Barkhausen effect, which is caused by the sudden change of magnetization in small but finite regions, occurs only during the initial part of the magnetization curve up to the knee. The second process, being more of the nature of an elastic deformation against the Mahajani force, must be continuous.

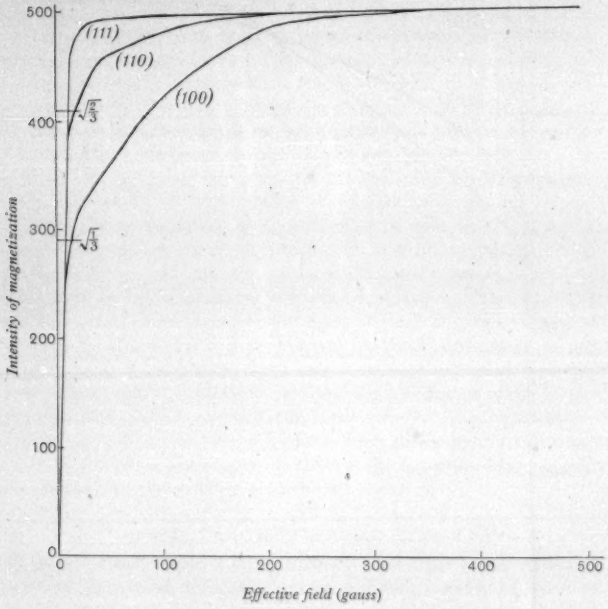


Fig. 2. Magnetization curves for nickel.

The value of the intensity of magnetization at which the break occurs will depend on the direction of the magnetic field relative to the crystal axes. When all the small elements in the crystal of iron have had their direction of magnetization changed to coincide as far as possible with the cubic directions nearest to the applied field, consistently with the condition that there should be no transverse magnetization, then any further increase of bulk magnetization must be produced by forced deviations from these cubic axes toward the direction of the applied field, and the knee will appear.

With an applied field along a cubic axis, the second process will not appear at all and the initial rapid rise should continue right up to complete saturation. For the (110) and (111) directions the transition should take place at intensities $\sqrt{\frac{1}{2}}$ and $\sqrt{\frac{1}{3}}$ of the saturation values.

In the case of nickel, where the (111) axis is that of easiest magnetization, this will be the direction for which there is no break in the I/H curve; while for the (110) and (100) axes the break will be at $\sqrt{\frac{2}{3}}$ and $\sqrt{\frac{1}{3}}$ of the saturation.

Experimental curves for these directions are given for iron* and nickel† in Figs. 1 and 2. In view of the difficulty of obtaining perfect single crystals of these metals sharply defined breaks cannot be expected, and the actual agreement is good.

§ 4. MAGNETOSTRICTION

With this picture of the process of magnetization it is possible to see what information may be gained about the phenomena of magnetostriction and magnetic change of resistance. The variation of length with intensity of magnetization is shown in Figs. 3 and 4‡ for the three simple directions in iron§ and nickel‖. In the direction of easiest magnetization in each metal there is a gradual change of length, in one case an expansion and in the other a contraction, beginning with quite small intensities and gradually increasing as saturation is approached. In the direction farthest removed from the above there is no appreciable change of length till approximately half-saturation, when a contraction sets in and rapidly increases with the magnetization. In the intermediate direction there is obviously a compounding of these two effects.

The treatment of magnetostriction by Akulov is based on the distortion of the crystal lattice accompanying the rotation of the direction of magnetization away from the direction of easiest magnetization, and explains the difference in length of unit rods parallel to the different crystal directions when completely magnetized; but it does not account, for example, for the increase in length occurring when a rod of iron parallel to a cubic axis is magnetized.

The origin of this effect appears, however, in the course of Akulov's calculations, for he shows that a cube of iron supposed completely devoid of magnetization on an atomic scale becomes slightly distorted when it acquires its normal spontaneous magnetization. It becomes longer along the cubic axis of magnetization than along those at right angles. The magnitude is not calculated, but it must be at least of the same order as the deviational distortion. Demagnetized iron must therefore be

* K. Honda and S. Kaya, *Sc. Rep. Tohoku*, **15**, 721 (1926).

† S. Kaya, *Sc. Rep. Tohoku*, **17**, 639 (1928).

‡ In the papers on the change of resistance and magnetostriction in nickel, quoted in the present paper, the intensities of magnetization were not given. They have been derived by the author from the effective magnetic fields by means of the magnetization curves of Kaya. As the same piece of nickel was not used in all three experiments there may be an appreciable error in the magnetization scale of Figs. 4 and 6 for values of the intensity up to the knee.

§ W. L. Webster, *Proc. R.S.* **109**, 570 (1925).

‖ Y. Mashiyama, *Sc. Rep. Tohoku*, **17**, 945 (1928).

considered as a mosaic made up of asymmetrical small volumes with their direction of distortion distributed equally along the three cubic axes. The resultant dimensions of a cube of demagnetized iron will be symmetrical but will be a complicated average between the two dimensions of the distorted magnetized cube. When magnetization takes place along a cubic axis there must be a longitudinal expansion, with a lateral contraction gradually increasing as the various small volumes become parallel.

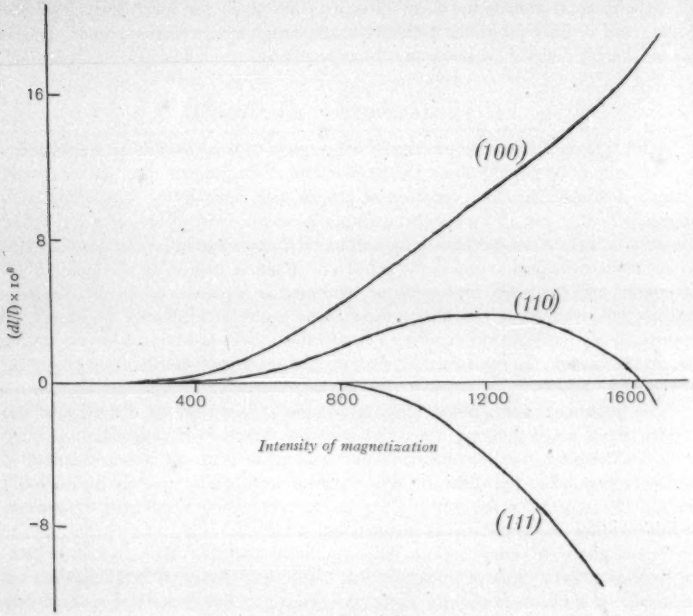


Fig. 3. Magnetostriction curves for iron.

For the (110) direction, as magnetization proceeds there is at first a concentration of the axes of distortion of the many small volumes along the two cubic axes nearest the direction of magnetization. There must therefore be an initial expansion continuing till the third axis has been completely abandoned. Further increase of magnetic intensity can only be produced by deviation from the cubic axes, and the corresponding contraction is sufficiently large to overwhelm the expansion due to increasing uniformity.

With the (111) axis, the three cubic directions are symmetrically disposed and there is no real increase in uniformity before the deviational distortion appears;

consequently no initial expansion is observed. It may be pointed out that the contraction should set in for all directions at a point corresponding to the bend in the I/H curve, a result with which the experiments are in reasonable agreement.

For nickel the rôles of the (100) and (111) axes are interchanged, and the distortion produced by the increase of uniformity becomes a contraction. The two kinds of distortion can, however, still be separated. It is particularly noticeable that

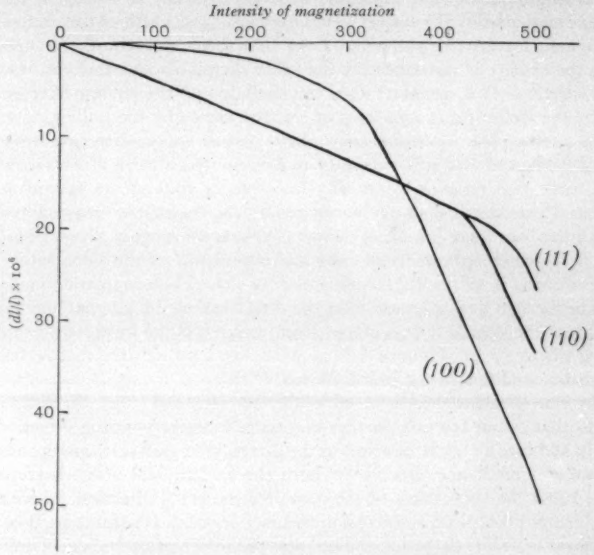


Fig. 4. Magnetostriction curves for nickel.

there is relatively little change in length for the (100) axis below intensities 300 c.g.s. units of magnetization. For the (110) direction, the effect of the deviation term is shown by the sharp increase in slope at 420 c.g.s. units, where from the I/H curve it is clear that the deviation is first brought into play.

§ 5. MAGNETORESISTANCE

In the light of the process of magnetization suggested above, it is possible to arrive at a qualitative explanation of the principal features of the magnetoresistance phenomenon, in so far as its appearance can be associated with a definite change of condition of the metal that is sufficiently likely to cause a change in the electrical properties. That such a connection between the magnetic and electrical properties

should exist is not surprising, since the work of Dorfman* on the Curie-point discontinuity of the Peltier effect has shown that in nickel the electrons responsible for the two properties are identical. Any disturbance of the magnetic condition of these electrons must react to some extent on the conductivity.

Experimental results for the longitudinal effect are shown for iron† and nickel‡ in Figs. 5 and 6. For iron the most remarkable fact is that, in spite of the enormous change of length for the (100) direction, there is practically no change of resistance. Rods were measured by the author which gave less than a fifth of the change shown in the figure. Further, it was pointed out that there is a remarkable parallelism between the change of resistance for the other directions and their contraction in magnetostriction. It is necessary then to conclude that the change of resistance is caused by the distortion of some sort of electron lattice by the pulling away of the magnetic axes of the electrons from their normal cubic direction against the Mahajani field. And also it is necessary to assume that a cube of iron magnetized along a cubic axis remains electrically isotropic in spite of its magnetostrictive distortion. These conclusions are borne out by the transverse§ magnetoresistance effect. In this case there is a small change of resistance roughly proportional to the applied field, which appears in all cases and is probably of the same nature as the change occurring in all metals, ferromagnetic or not. There is in addition a change depending for sign and magnitude on the directions of the current and magnetic field. This is the definitely crystalline effect, and it has the important property of vanishing whenever the magnetic field is parallel to a cubic axis, that is, whenever the magnetic condition of the iron is normal.

In the case of nickel there is an effect for the (100) and (110) directions exactly similar to that found for iron for the magnetically corresponding directions, but there is in addition a very large effect in the direction of easiest magnetization. This new change of resistance appears for both the longitudinal and transverse phenomena. From the appearance of the curve for the (111) direction, where the resistance change takes place in the region within 5 per cent. of saturation, this change might appear to be due to the acquisition, near the region of saturation, of uniformity and continuity in the magnetic condition of the crystal. Such an explanation would almost certainly involve a diminution‖ of resistance rather than the increase actually shown, so that it is better to look elsewhere for the origin of this effect.

In the case of the transverse phenomenon, there is also a change of resistance with magnetization along the (111) axis, but in this case it is a decrease in resistance. This fact suggests that both the changes that were found for this direction are akin to the changes of length in the direction of easiest magnetization that occur in nickel and iron. That is, they are due to the electrical anisotropy of nickel magnetized along its magnetic axis. If this be so, then the appearance of the curve, which is not

* J. Dorfman, *Zeit. für Phys.* **54**, 277 (1929).
† W. L. Webster, *Proc. R.S.* **113**, 196 (1926).
‡ S. Kaya, *Sc. Rep. Tohoku*, **17**, 1027 (1928).
§ W. L. Webster, *Proc. R.S.* **114**, 611 (1927).
‖ W. Gerlach, *Zeit. für Phys.* **59**, 847 (1930).

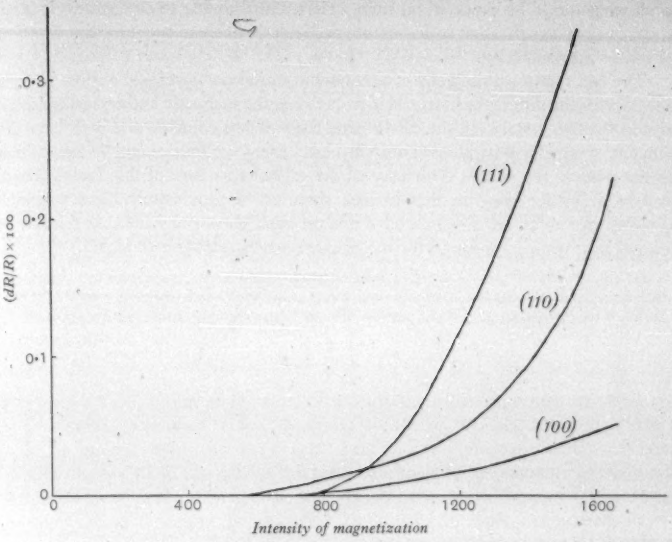


Fig. 5. Magnetoresistance curves for iron.

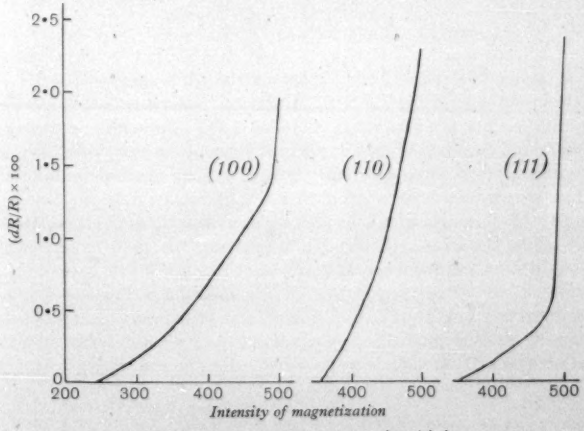


Fig. 6. Magnetoresistance curves for nickel.

at all what would be expected on such a view, must be due to comparatively large errors in the magnetization scale—errors particularly likely to occur for the direction of easiest magnetization.

The fact that the anisotropy occurs only in nickel and not in iron must be connected with the difference in the relation between the magnetic and crystallographic axes in the two metals. In the case of iron the two sets coincide and both have the simplest symmetry possible, whereas in nickel there are four magnetic axes which do not possess the simple symmetry of the crystal structure of this metal. There may be a further cause in that in iron there are approximately three magnetic electrons per atom, all of which have not yet been shown to have that relation to conductivity which was found for the single magnetic electron of nickel.

OBSERVATIONS ON THE SPECIFIC HEATS OF FERROMAGNETIC SUBSTANCES

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ABSTRACT. Attention is drawn to the variation of the specific heats of ferromagnetic substances with temperature. In particular, the magnetic and thermal behaviour of a ferromagnetic compound of manganese and arsenic is described, and it is shown that, in this case, the maximum value of the specific heat coincides with the maximum value of dI_0^2/dT. The variation of the specific heats of ferromagnetic substances with temperature is then contrasted with that expected on the Weiss and on the Heisenberg theories of ferromagnetism.

The specific heats of ferromagnetic substances have recently attracted considerable attention. It was shown by Weiss*, on the basis of his hypothesis of an internal field, that the specific heat of a ferromagnetic substance should exhibit characteristic changes in the neighbourhood of its magnetic critical point. Below this temperature the specific heat should be increased because it is necessary to supply heat to cause the substance to lose its intrinsic magnetism, i.e. the magnetism associated with it even when it is not exposed to the action of an external magnetic field. The increment, s, of specific heat which thus arises at any temperature is given by

s

$$s = -\frac{1}{2.J}\cdot\frac{1}{\rho}.N.\frac{dI_0^2}{dT} \text{ cal. per gm. per degree C.},$$

where N is the constant of the internal field, I_0 the intensity of intrinsic magnetization per c.c., J the mechanical equivalent of heat, ρ the density of the substance and T the absolute temperature. The experiments made by Weiss and his collaborators† to test this theory were not in many respects conclusive and need not be elaborated here. The above formula leads us to expect that the specific heat of a ferromagnetic body should rise to a maximum at the magnetic critical temperature and should then decrease very rapidly, owing to the sudden disappearance of the magnetism at that temperature. Now, the experiments of Sucksmith and Potter‡ on the behaviour of nickel showed that whilst there was a very close connection between the specific heat of a ferromagnetic substance and its magnetic properties, the connection was not precisely that predicted by the Weiss theory. They found, for example, in the case of pure nickel that the specific heat rose to a maximum as Weiss predicted, but the increased specific heat extended over a range of some 26° C. above the magnetic

N, I_0
J, ρ
T

* P. Weiss, *Journ. de Phys.* **7**, 249 (1908).
† P. Weiss, A. Piccard et A. Carrard, *Arch. Sci. Phys. Nat.* **42**, 378 (1916); **43**, 22, 113, 199 (1917).
‡ W. Sucksmith and H. H. Potter, *Proc. R.S.* A, **112**, 157 (1926).

critical temperature. Unfortunately, they were not able to determine whether the maximum value of the specific heat coincided with the maximum value of dI_0^2/dT or not. In the case of manganese arsenide, a substance of chemical formula MnAs which possesses a convenient and sharply defined magnetic critical temperature in the neighbourhood of 45° C., the author* found that the specific heat rose to a sharp maximum at 42·2° C. and then decreased rapidly. The form of the specific heat curve was similar to that which would be expected on the Weiss theory. The experiments on the magnetic properties were, however, not so favourable to the theory. The sharpness with which the magnetic critical point was defined meant that the curves of dI_0/dT and dI_0^2/dT were somewhat similar in shape to the specific-heat curve, but whereas the first curve exhibited a maximum at 42·2° C., the second exhibited a maximum at 41·5° C. The difference, 0·7° C., appeared to be far outside the limits of experimental error. It was therefore felt that the variation of specific heat with temperature was more nearly proportional to dI_0/dT than to dI_0^2/dT. At the same time it was felt that this conclusion was also justified on theoretical grounds, and the author concluded in agreement with previous workers that, although there was an intimate connection between the specific heat of the ferromagnetic substance and its magnetic properties, it was not that given by the Weiss theory.

The state of our knowledge of this subject could clearly not be regarded as satisfactory, and it was hoped that additional information might be obtainable by an examination of the magnetocaloric phenomena exhibited by manganese arsenide. When a ferromagnetic substance is suddenly inserted into a magnetic field, thermodynamical reasoning† shows that if the process is reversible, an adiabatic rise ΔT in temperature results, according to the equation

$$\Delta T = -\frac{T}{(s)_H} \cdot \frac{\partial I_0}{\partial T} \cdot \Delta H,$$

H, s where ΔH is the increase in field strength and $(s)_H$ the specific heat of the substance when the field is constant. For a positive increment of H, ΔT is positive, since $\partial I_0/\partial T$ is negative. A similar adiabatic fall in temperature should occur when the field is decreased suddenly. A preliminary calculation with the values of $(s)_H$ and dI_0/dT to hand for manganese arsenide showed that in the neighbourhood of 42° to 43° C. a field of even 3000 gauss when suddenly applied should produce a rise in temperature of about 1° C. Now manganese arsenide exhibits a temperature hysteresis. On losing its magnetism at 45° C. it does not become ferromagnetic again until it has been cooled to below 34° C., and if it is heated, say, to 43° C. and then cooled, its magnetic properties remain practically constant over several degrees. Hence, it would be expected that if the substance were suddenly placed in a magnetic field its temperature would be raised and its magnetic properties more or less permanently changed. In other words, its magnetic properties after it has been

* L. F. Bates, *Proc. R.S.* A, **117**, 680 (1927).

† P. Weiss et A. Piccard, *Comptes Rendus*, **166**, 352 (1918); P. Weiss et G. Foex, *Le Magnétisme*, p. 148 (1926).

placed in and then withdrawn from a magnetic field, should correspond to those at a temperature higher than that which it initially possessed. It might be thought that, on this view, introduction of the substance into a magnetic field would result in a rise in temperature, because of the rapid change of magnetization with increase in temperature, whilst withdrawal from an equal field would result in no appreciable fall of temperature, since the variation of magnetization with a small decrease in temperature is very small. Reference to the expression given above reminds us, however, that the specific heat must also be taken into account, and in the first case the specific heat is larger than in the second, so that we cannot say much about the relative changes of temperature, except that if the Weiss theory of the specific heat is correct the rise in temperature should be greater than the fall. These remarks are intended to apply, of course, only to those temperature regions where the hysteresis is observed.

It seemed, therefore, pertinent to enquire whether an effect of this kind could have entered into the magnetic experiments which the author carried out when testing the validity of the Weiss theory. Since the magnitude of the effect was then unknown and the need for eliminating it was not realized, the experiments were actually carried out in such a manner that disturbances due to the effect must certainly have arisen. In these earlier experiments, the arsenide was placed in a thin copper tube 6·3 cm. long, an induction helix being wound upon the whole length of the tube. The latter was mounted, in series with a compensating helix wound upon a similar tube, between the rounded pole-pieces of an electromagnet. A field of 1960 gauss was usually employed for the induction measurements. The specimen and compensating helix were maintained at suitable temperatures by means of a well-stirred and electrically heated oil bath. The magnetic induction of the specimen was determined from the deflection of a ballistic galvanometer when the specimen was rapidly rotated through 180°, or from the deflection when the magnetic field was switched on or off. In the second case the disturbances were clearly bound to occur. In the first case, since no particular reason was seen for maintaining the magnetic field except when observations were actually being recorded, the field was switched off between measurements. Moreover, even when a field was maintained constant during a complete set of observations, it was not uniform over a large area, and as the specimen rotated certain portions of it passed from a strong into a weaker field and then re-entered a field of the original strength. Here, again, there was the possibility that mere rotation of the specimen may have produced important changes. In fact, it was observed in regions where the magnetization changed rapidly that the galvanometer deflection produced by the first rotation was always markedly greater than those produced by later rotations. It was, of course, probable that this was in part due to the fact that the substance was in the form of a powder, and, of course, to ordinary hysteresis effects in which heat is generated. Therefore, prior to the recording of galvanometer deflections, such rotations were carried out many times, in the expectation that the substance would acquire a steadier state. In this case, too, the magnetic induction actually recorded would be characteristic of a temperature higher than that of the liquid in the surrounding bath. Hence the recorded values of dI_0^2/dT would exhibit a maximum at

a recorded temperature lower than that at which the maximum would occur in the absence of any magnetocaloric effect.

It was therefore concluded that the presence of the effect just described provided a possible explanation for most of the lack of agreement between the experimental results and the Weiss theory, and new experiments have been carried out to obtain the variation of I_0^2 with temperature in the absence of such disturbances. The arsenide was packed inside a small thin-walled glass tube of ellipsoidal shape on which an induction helix was wound. This, together with a compensating helix, was placed in a magnetic field produced by an electromagnet with flat pole-pieces,

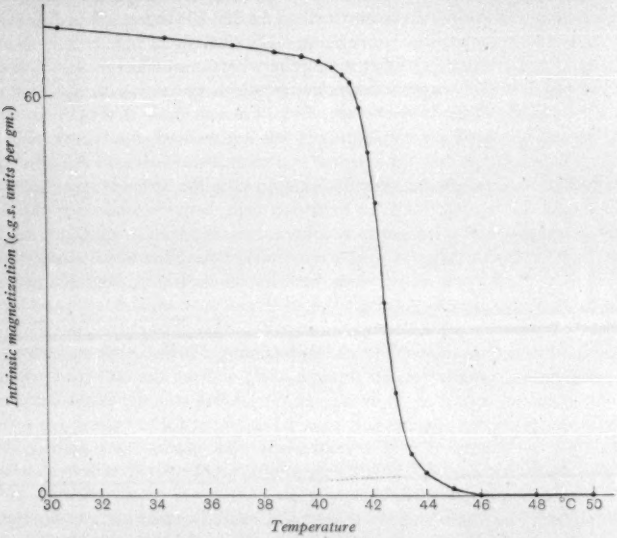

Fig. 1. Variation of intrinsic magnetization with temperature.

and the induction was measured by the deflection of a ballistic galvanometer when the specimen was suddenly rotated through 180°. The substance was therefore maintained in a constant field during the rotation, except for certain minor changes produced by the shape of the specimen. The substance was first cooled to 0° C., when the field was switched on; the field was then maintained constant throughout the whole series of observations. Even with these arrangements and procedure, distinct differences between the galvanometer deflections for first and subsequent rotations at a specified temperature were observed when the temperature was in the region where the magnetization changed rapidly. These were, of course, partly due to ordinary hysteresis heat changes. It was possible to compute, without

likelihood of appreciable error, the deflection which would result if all such disturbances were completely absent. To enable I_0 to be determined from the deflections obtained with a constant field, separate experiments were carried out with various fields, to obtain the relations between the deflection and I_0. Actually, with the fields at the author's disposal, which, unfortunately, did not exceed 4000 gauss,

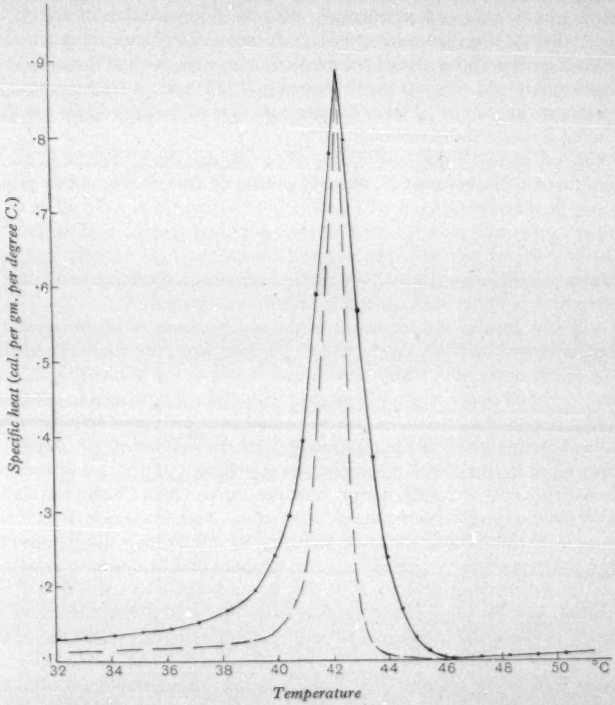

Fig. 2. Variation of specific heat (full line) and of dI_0^2/dT (broken line) with temperature.

it was found that the values of I_0 over the range which is important in this investigation could be found by multiplying the deflections obtained with a single field by a constant numerical factor. The variation of I_0 with temperature as thus determined is shown in Fig. 1.

In Fig. 2 are plotted the values of the specific heat obtained in an earlier research, the continuous curve being the curve of specific heat. The broken curve represents the behaviour of dI_0^2/dT obtained from the curve of Fig. 1. It is quite clear that the

specific-heat maximum and that of dI_0^2/dT occur at the same temperature. The maximum values of course are not likely to be highly accurate in both cases. Reference should be made to the earlier paper for further details of the specific-heat determinations. There is, however, no doubt that the specific-heat curve differs in several particulars from the curve of dI_0^2/dT*. It is clear that appreciable specific-heat changes occur before dI_0^2/dT has appreciably changed, and even if the marked changes in specific heat end concurrently with the disappearance of dI_0^2/dT, there is no doubt that the specific heat is considerably greater just before the magnetization disappears than the Weiss theory would lead us to expect. For the sake of completeness we may add that the curve showing dI_0/dT against temperature gives a sharp maximum at a slightly higher temperature than dI_0^2/dT, and falls less sharply after passing through the maximum.

We may calculate the maximum value which the specific heat should possess on the Weiss theory. The value of N, recently obtained† from susceptibility measurements, may be taken to be 3390, ρ to be 6·02 gm. per cm.^{3}, and $\frac{1}{2}dI_0^2/dT$ at 42·2° C. to be 1560 c.g.s. units per gm., so that the additional specific heat at 42·2° C. is found to be 0·67 cal. per gm. per degree. Hence the total specific heat at this temperature should be 0·77, whilst the maximum measured specific heat is 0·8. The close agreement is rather striking and is probably accidental.

We will now turn to the treatment of the specific heats of ferromagnetic substances as developed by R. H. Fowler and P. Kapitza‡ along the lines of Heisenberg's theory of ferromagnetism. Unfortunately the theory is not sufficiently developed to tell us the actual shape of the specific-heat curve of a ferromagnetic substance in the neighbourhood of its magnetic critical point, but it predicts a step-down in the specific heat at this point of approximately 3 cal. per gm. mol. per degree. The step-down given by the Weiss theory and by experiment in the case of manganese arsenide is of the order of 100 cal. per gm. mol. per degree. Now Fowler and Kapitza's expression for the specific heat contains two factors. One of these is $d\xi^2/dT$, where ξ is the ratio of the intrinsic magnetization, I_0, to the limiting magnetization, I_s, which the substance may acquire at very low temperatures or under extremely high fields. At the magnetic critical point T_c this factor is taken as equal to $3/T_c$. The experimental data for manganese arsenide show that this is not far wrong. The discrepancy between the experimental and theoretical results must therefore be attributed to the remaining factor, which depends on the interaction integrals of two similar interacting systems each containing one magnetizable electron, calculated from the perturbation theory. The obvious inference is that the magnetism of the arsenide is due to interacting systems each containing more than one magnetizable electron. Fowler and Kapitza suggest that a theory to take account of more than one such electron will differ from that already developed by them only in complication of detail, and will give a value for the step-down of two or three times

* For convenience in comparison the maxima and minima of the two curves have been arranged to have equal values.

† L. F. Bates, *Phil. Mag.* **8**, 714 (1929).

‡ R. H. Fowler and P. Kapitza, *Proc. R.S.* A, **129**, 1 (1929).

that mentioned above. It would appear that some more detailed discussion of the more complicated problem is necessary, as the step-down in the case of the arsenide is at least thirty times the value predicted by the theory so far developed. The numerical values obtained by Sucksmith and Potter for the specific heat of nickel are, as we shall see, in rough agreement with Fowler and Kapitza's prediction, but it must be pointed out that their theory is no more successful than the Weiss theory in explaining why the step-down in the case of nickel extends over such a wide temperature range, or why the specific heat continues to change when the magnetism has disappeared. The careful experiments of Sucksmith and Potter therefore force us to conclude that any theory is incomplete which gives a simple term depending only on the magnetization in the expression for the specific heat. We might get

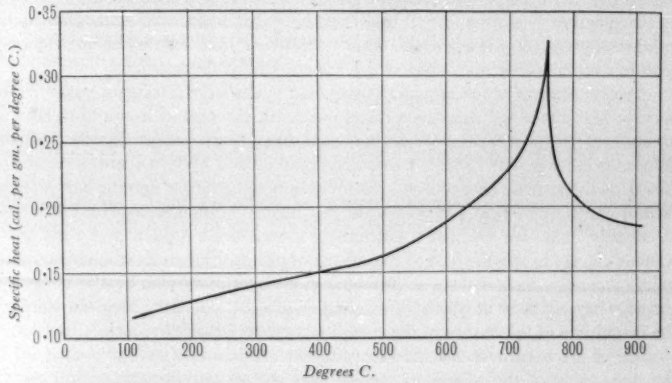

Fig. 3. Klinkhardt's specific-heat/temperature curve for pure iron.

over the difficulty to some extent by taking into consideration the fact that whilst the specific-heat changes continue there is a very small change in the magnetization still in progress. The actual step-down for nickel, calculated from the curves of specific heat of nickel given by Sucksmith and Potter, varies from about 0·9 to about 1·34 cal. per gm. mol. per degree, depending on the specimen of nickel. The corresponding value given by Weiss and his collaborators is 1·67, whilst Klinkhardt, whose work is referred to below, gives 1·47. Now, Fowler and Kapitza remark that they would expect the step-down to be rather less than 3, so that the above numbers may be taken to indicate that the magnetic behaviour of nickel is due to the interaction of systems each containing one magnetizable electron.

In the case of iron, Weiss, Piccard and Carrard give 6·8 cal. per mol. per degree for the step-down. An interesting series of measurements on iron was carried out by Klinkhardt*, who mounted his specimen on quartz hooks in an electric furnace

* H. Klinkhardt, *Ann. der Phys.* **84**, 167 (1927).

and placed it inside a highly evacuated vessel. The furnace served to raise the specimen to any required initial temperature, and when the furnace conditions were such that the temperature of the specimen was rising slowly and regularly, additional heat was supplied by electron bombardment. This additional energy was supplied for about ½ to 1 minute, when the bombardment was stopped, and the rise of temperature, amounting to from 3° to 5° C., was measured with a thermocouple. His curve, obtained for chemically pure iron—apparently not of the highest degree of purity—is reproduced in Fig. 3. The A_2 point was found at 760° C., which is rather low. The step-down indicated in this case is rather more than 8 cal. per gram-mol. per degree C., and it extends over a range of some 150° C. Unfortunately Klinkhardt does not give any magnetic data, but there is no doubt that his work, too, shows that the specific-heat changes persist far above the temperature at which ferromagnetism has disappeared, and that the specific-heat curves are essentially continuous. In fact, the use of the term "step-down" is a little unfortunate, as we tend to restrict its use to changes which are essentially discontinuous.

Another feature which requires explanation is the large minimum value of the specific heat above the magnetic critical point. In the case of iron and nickel we always find minimum values which give atomic heats much greater than the limiting values for other metals. This is particularly emphasized by Klinkhardt's measurements on the specific heat of iron. His minimum value for the specific heat of Feβ is approximately 10·3 cal. per gram-atom per degree C., and his value for Feγ is 8·6.

Finally, then, we see that a satisfactory theory must explain firstly the continuous change in specific heat as the substance passes through its magnetic critical point; secondly, the existence of pronounced changes of specific heat at temperatures far beyond those at which the external magnetic properties disappear; thirdly, the magnitude of the change in the case of manganese arsenide; fourthly, the coincidence of the maximum specific heat with the maximum of dI_0^2/dT; and, lastly, the high value of the minimum specific heat above the magnetic critical point. I hope that I have succeeded in showing exactly how far these requirements are met by current theories.

I wish to take this opportunity of thanking Professor E. N. da C. Andrade for his helpful suggestions in connection with this paper.

SOME RELATIONSHIPS AMONG THE FERROMAGNETIC CONSTANTS

By J. R. ASHWORTH, D.Sc.

ABSTRACT. Attention is directed in this note to a ferromagnetic equation written after the model of Van der Waals' equation of state for fluids. It is shown that the ratios of the absolute critical temperatures to the maximum intensities of magnetization are nearly the consecutive numbers 2, 3, 4, 5, 6 for iron, cobalt, nickel, Heusler's alloy and magnetite respectively. A formula for the discontinuity of the specific heat at the critical temperature is derived which agrees well with experimental determinations, and a simple formula is given connecting this discontinuity with the true specific heat at the critical temperature. Also certain simple relations among the constants of ferromagnetism disclose themselves which show that the ferromagnetic properties of iron and nickel are to one another as those of cobalt and magnetite.

In his paper on magnetism Curie regarded magnetism as analogous to fluids and threw out the suggestion that there may be critical constants for magnetism as for fluids. If this suggestion is developed by writing an equation to ferromagnetism on the model of Van der Waals' equation to fluids as follows,

$$(H + a' I^2)\,(1/I - 1/I_0) = R' T,$$

where

H	is the magnetic field strength,	H
I	is the intensity of magnetization,	I
I_0	is the maximum intensity of magnetization,	I_0
$a' I_0^2$	is the maximum intrinsic field,	a'
R'	is the reciprocal of Curie's constant in volume units, and	R'
T	is the absolute temperature,	T

then certain simple relationships amongst ferromagnetic quantities disclose themselves. Some of these will be set down here with as much brevity as possible.

(1) The above equation gives for the critical temperature θ, θ

$$\frac{\theta}{I_0} = \frac{8}{27} \times \frac{a'}{R'} \qquad \ldots\ldots(1).$$

When numerical values are applied a'/R' has simple integral values to a close approximation as shown in Table 1.

The numbers in the last column progress by uniform steps of nearly 0·3.

(2) *Specific heat and ferromagnetism.* If ΔC is the discontinuity at the critical temperature, then ΔC

$$n.\Delta C.\theta = a' I_0^3 \qquad \ldots\ldots(2),$$

or if ΔC be in heat and mass units, then

$$n.\Delta C.\theta J\rho = a' I_0^3 \qquad \ldots\ldots(3),$$

J, *ρ*, *n* where J is the mechanical equivalent of heat, ρ the density and n a simple number. Thus

$$\Delta C = a' I_0^3 / nJ\rho \qquad \text{.....(4).}$$

Table 1.

	a'/R'	Percentage difference from whole numbers	θ/I_0
Iron	1·97	+ ·5	$0{\cdot}58 = \frac{1058^\circ}{1817}$
Cobalt	3·20	− 6·0	$0{\cdot}95 = \frac{1349^\circ}{1422}$
Nickel	4·02	− 0·6	$1{\cdot}20 = \frac{661^\circ}{552}$
Heusler alloy	5·09	− 1·7	$1{\cdot}50 = \frac{633^\circ}{420}$
Magnetite	6·03	− 0·5	$1{\cdot}79 = \frac{853^\circ}{477}$

From equation (1) we can substitute for $a' I_0^3$ its equivalent $\frac{27}{8} R' I_0^2 \theta$, and we get

$$\Delta C = \tfrac{27}{8} R' I_0^2 / nJ\rho \qquad \text{.....(5);}$$

the truth of this equation is shown in Table 2.

Table 2.

	n	$\frac{27}{8}\frac{R' I_0^2}{nJ\rho}$	By experiment	Percentage difference
Iron	1	0·120	0·120	0
Cobalt	1	0·114	0·098	16
Nickel	2	0·029	0·029	0
Heusler alloy	2	0·037	0·036	2
Magnetite	3/2	0·079	0·079	0

It can be shown experimentally that

$$R' I_0^2 = R/an \qquad \text{.....(6),}$$

R, *a*, *n* where R is the gas constant and $= 83{\cdot}15 \times 10^6$, a is the atomic weight, and an the molecular weight.

Substituting for $R' I_0^2$ in the formula for the discontinuity, we get

$$\Delta C = \tfrac{27}{8} R/Jan^2 \qquad \text{.....(7),}$$

and as $\frac{27}{8} R/J = 6{\cdot}683$, we have

$$\Delta C = 6{\cdot}683/an^2 \qquad \text{.....(8).}$$

In the formula given by Prof. Weiss and amended by H. A. Lorentz the numeral is 4·97 instead of 6·683. The results to which the Weiss-Lorentz formula leads are compared with theory in Table 3.

Table 3.

	Atoms in molecule	Weiss-Lorentz formula	By experiment
Iron	1	0·089	0·120
Nickel	3	0·282	0·285
Magnetite	$\frac{1}{3}$ Fe_3O_4	0·064	0·079

Table 3 shows a considerable difference between theory and experiment for iron and magnetite.

(3) There is a simple relation between the true specific heat at the critical temperature, C_θ, and the discontinuity ΔC. It is

$$C_\theta = \tfrac{3}{2} n . \Delta C \qquad \text{.....(9).}$$

This formula is probably a reduction of the more general formula

$$maC_\theta = 5na . \Delta C \qquad \text{.....(10),}$$

ma and *na* being molecular weights below and above the critical temperature.

The verification of the first formula is given in Table 4.

Table 4.

	n	$\frac{3}{2} n . \Delta C$	By experiment	Difference per cent.
Iron	1	0·300	0·309	− 3
Cobalt	1	0·245	0·270	− 9
Nickel	2	0·145	0·154	− 7
Heusler alloy	2	0·180	0·179	+ 0·5
Magnetite	3/2	0·296	0·299	− 1

By using the equations given above and substituting we arrive at a formula for the true specific heat at the critical temperature. It is

$$C_\theta = 16{\cdot}79/na \qquad \text{.....(11).}$$

For the same ferromagnetic the numerical values of n are the same in all the formulae where it appears. Throughout, theory and experiment agree least satisfactorily in the case of cobalt, for which the numerical data are probably less precise than with other ferromagnetics.

The values of C_θ and ΔC are mainly taken from the work of Prof. Weiss and his collaborators. The reduced formulae for C_θ and ΔC apply to the ferromagnetic elements, where a has a definite value, and need a modification for Heusler alloy and magnetite.

(4) There are some interesting correspondences between the ratios of the constants of the ferromagnetics, quite apart from any theory, which deserve notice. They are set out in Table 5.

Table 5.

	a'	R'	I_0	θ	ΔC	Increase of specific heat $(C_\theta - C_{15})$
Iron Nickel	Ratio 0·085	0·17	3·3	1·6	4·0	4·0
Cobalt Magnetite	Ratio 0·09	0·17	3·0	1·6	1·23	1·25

Some of these correspondences follow from the ratios of θ to I_0 set out in the first section. Evidently the ferromagnetics are closely allied to one another in their constants.

DISCUSSION ON MAGNETISM

OPEN DISCUSSION

Dr E. C. STONER (communicated). Among the more general problems of magnetism there is one to which I would like to draw special attention, and that is the precise relation between para- and ferromagnetism.

In the Langevin-Weiss theory the relation between the ferromagnetic properties below, and the paramagnetic above, the Curie point is clear and simple for an ideal substance consisting of an aggregate of magnetic molecules whose interaction can be represented by the conception of the molecular field. The general character and simplicity of the relations are not essentially changed when the modifications necessitated by the older quantum theory are introduced, nor, I think, from the point of view here considered, by the present interpretation of the significance of the molecular field.

The typical normal paramagnetics are salts. The typical ferromagnetics are metals. Above the Curie point the three ferromagnetic elements do behave, over extended ranges of temperature, as normal paramagnetics (in obeying the Weiss law). There is no evidence of definite structural changes at the Curie point. The question then arises whether there are those simple relations between the magnetic characteristics below and those above which would be anticipated from theory.

The answer seems to be that there are not. Below the Curie point the magnetic properties can be accounted for by attributing the rôle of carrier to electron spin. Above, this does not seem to be possible. Too many electron spins per atom would be required. This suggests the conclusion that orbital moments may then also be involved. Even if this view is accepted, however, there remain many difficulties. There is one in the apparently unreasonable numbers of electron spins required per atom to account for the low-temperature saturation values; and another in that it is difficult to connect these numbers simply with the magnetic moments per atom deduced from the susceptibility measurements above the Curie point.

The possibility of a substance being ferromagnetic depends on the sign of the interchange interaction integral; experimentally the Curie temperature is positive. There is thus a possibility of a series of substances passing from those that are ferromagnetic at fairly high temperatures through those that are ferromagnetic at low temperatures to those that are purely paramagnetic; among these last may be some which have a large negative Curie temperature, so that they appear to possess an almost constant paramagnetism. The investigation of alloys in which a continuous change in the Curie temperature can be made promises to shed much light on the whole problem of paramagnetism.

I have brought forward very briefly these two points—the precise relation between the ferromagnetic and paramagnetic properties of a single substance,

and the relation between the magnetic properties of a series of metals and alloys—as perhaps deserving a fuller consideration and discussion than they have generally received.

Mr S. Evershed. Sir Alfred Ewing has reminded us of the long years that have gone by since the first publication of his theory of the magnetic control of the process of magnetization. I was from the first a disciple, and from that day to this the ideas at the root of the theory have been my constant guide in ferromagnetic research. Indeed, to anyone who finds himself in daily contact with ferromagnetism experimentally, Ewing's theory is indispensable. Experimental research cannot be effectively conducted without the aid of some working hypothesis, something to provide a foothold or a vantage point. A formula may sometimes serve the purpose, but speaking generally nothing is of greater assistance to the experimenter in his daily work than a mental picture of what is happening. And the more complex the phenomena the greater the need for guidance of that kind.

I know nothing in experimental physics more complicated, more confusing, than the actions of ferromagnetic bodies in the course of magnetization. In his opening remarks Sir Alfred Ewing refers to their behaviour as "an attractive tangle of conspicuous magnetic phenomena." It is an apt description, and if the researcher is to avoid losing himself in the tangle he must carry with him the mental picture which Sir Alfred gave us forty years ago. Up to that epoch various imaginary forces had been suggested to account for the extreme complexity disclosed by magnetization curves, and at a time when little or nothing was known of atomic structure the replacement of all those clumsy expedients by the conception of magnetic control was a stroke of genius.

After forty years the position is very different. Within the present century the structure of the atom has been disclosed to us, and we now see that inside the ferromagnetic body there are only three kinds of inter-molecular action: gravitational, electric, and magnetic. Of these, the first is feeble, and in any case gravitation cannot have an orienting effect. The second, electric force, is inoperative between neutral atoms. Magnetism is therefore the only possible origin for the powerful forces controlling the orientation of a mass of magnetic molecules; and since magnetism is created by the motion of electricity, the controlling field can only come from the planetary electrons. Ewing's fruitful idea, therefore, provides a true picture of the ferromagnetic mechanism so far as the broad outline is concerned. But much of the detail is still hidden from us. Hysteresis requires a system composed of more or less self-contained groups of magnetic molecules, or possibly something equivalent in the atomic structure, and at the present time this part of the mechanism can only be vaguely perceived. Nevertheless, seen or unseen, the machinery must be there, for every magnetization curve, every curve of recoil, declares its existence.

Hard as it is to see in our mind's eye what is going on inside a ferromagnetic body during the progress of orientation, it is harder still to provide any alternative in the way of a mathematical framework for the phenomena. In some respects

this is a drawback. To minds with a mathematical bent it may even appear to be a defect in the picture drawn by Ewing. But I rather think the defect lies in our mental equipment; mathematics by all means, but not at the expense of imagination. In putting imagination first and mathematics second, I am conscious of attempting to swim against the prevailing stream of thought, for in recent years there has been an amazing extension of the field of purely mathematical research, an extension so fruitful that it tempts physicists to believe that Nature in all her infinite variety needs nothing but mathematical clothing. Indeed the ultra-modern physicist appears strangely reluctant to contemplate Nature unclad. Clothing she must have. At the very least she must wear a matrix, with here and there a tensor to hold the queer garment together.

Such is the present tendency; but I am convinced that in a subject like ferromagnetism nothing can take the place of mental imagery. We shall never behold reality, but we can at least strive to catch sight of an image of it, however fleeting and imperfect. That, after all, was the method of Faraday. He had no mathematical knowledge. But he had an unclouded imagination, and with the aid of those images which formed so readily in his mind he found his way through the tangle of phenomena to splendid discoveries.

Mr S. Evershed (communicated subsequently). In the papers of Mr F. C. Powell and Dr Bates, the energy absorbed by a ferromagnetic substance when the change takes place from the magnetizable to the non-magnetizable state is considered, and Mr Powell compares observed values with those computed on the basis of Heisenberg's theory. I shall not comment on the theory, beyond remarking that it needs further development if it is to come into line with the confusing but well-ascertained facts of ferromagnetism. Sooner or later every hypothesis bumps into some awkward fact, and whether Heisenberg's theory will survive a shock of that kind remains to be seen.

But in making numerical comparisons between theory and experiment it is well to be sure of the facts, and I think the authors of these two papers have been a little unfortunate in their choice of experimental data. To begin with, the assumption is made that the magnetic change occurs at a critical temperature, the so-called Curie point. No doubt an individual atom of alpha-iron has a definite temperature at which it will transform itself into an atom of the beta kind, provided it is not influenced by what its neighbours are doing. But observed values are necessarily derived from experiments on test-pieces containing millions of atoms, and in such circumstances the loss of magnetism does not occur at a critical temperature. The change of state from alpha to beta takes place progressively over a fairly well-defined range of temperature, a range of about 40° C. in the case of pure iron. Those are the normal conditions of experiment, and I do not know where the Curie point is to be found. I only know it as it appears in equations.

The gradual loss of magnetism as the temperature is raised was first traced by

Hopkinson*, and expressed by a curve connecting magnetic state with temperature. Since his time other workers have obtained similar curves, but owing to the difficulties of experiment few of the published curves are free from ambiguity. So far, the most accurate curves of loss and recovery of magnetism appear to be those obtained by Prof. Honda† and in the Research Laboratory at Acton Lane Works, the latter having been observed for me by Mr Finnis‡. In both cases the magnetometer method was employed and every precaution was taken to ensure freedom from the many sources of error involved in experiments at high temperatures.

In pure iron the curve of loss of magnetism is coincident with the curve of recovery, and the true form of such curves having been determined with certainty it is easy to distinguish between good and bad observation. For example, curve 11 in Fig. 1 in Mr Powell's paper is obviously wrong. A well-marked feature of the true curve of loss of magnetism is the very gradual approach to the zero line, and the abrupt ending of curve 11 is quite impossible. On the other hand, in Dr Bates' paper, the curve in Fig. 1 for a specimen of manganese arsenide is on the face of it a correct representation of the loss of magnetism under the conditions of the experiment. To determine the temperature at which the magnetic molecules began, one after the other, to pass into the non-magnetic state, it would have been necessary to reduce the initial magnetization to a somewhat lower value, and in that case the final approach to zero would have been rather more prolonged.

The earliest attempt to measure the energy associated with the magnetic change was made by Hopkinson and is described in the paper already referred to. He computed the energy by the method which Dr Bates refers to as the "step-down." That is to say, Hopkinson assumed that the vertical distance between the portions of the temperature-energy curve above and below the allotropic change was a measure of the energy absorbed by the transformation. Now if this change occurs at a truly critical temperature this simple way of determining the energy is correct, but not otherwise. Here, in short, is a trap for the unwary, and judging from the published data for the energy absorbed in the change from alpha- to beta-iron (the magnetic change) it seems that everyone, including Hopkinson, has fallen into the trap. Certainly I fell into it myself, but when I discovered where I was I got out again, and set to work to compute the energy correctly. Since the magnetic change occupied a range of temperature I resorted to a step-by-step integration from one end of the range to the other. For each element in the summation it was necessary to know the specific heat of alpha-iron, and that of beta. It was also necessary to know, for every temperature within the range, the proportion of alpha molecules to beta molecules. For the specific heats I used the values obtained by Wüst and his co-workers§, and it would be hard to find any

* John Hopkinson, "Magnetic properties of iron at high temperatures," *Proc. R. S.* A, p. 464 (1889).

† Honda and Murakami, *Science Reports, Tohoku University*, **6**, No. 5 (1918).

‡ S. Evershed, *Journ. I.E.E.* **63**, 725 (1925).

§ Wüst, Meuthen, und Durrer, *Die Temperatur-Warmeinhaltskurven der technischen wichtigen Metalle. Forschungsarbeiten auf dem Gebeite des Ingenieurwesens*, No. 204 (1918).

experimental determinations of the kind conducted with greater skill and precision. The proportion of alpha to beta was obtained directly from a curve expressing loss of magnetism as a percentage of the initial value of the flux density.

The result of this somewhat laborious calculation of the energy absorbed in the transformation from alpha to beta is included in the accompanying table, where the energy, it should be noted, is given in small calories per gram of iron.

Table giving the items of allotropic energy in pure iron.

Temperature C.	Allotropic change	Energy absorbed cal./gm.
700	Precursor effect of unknown origin	4·5
770	Alpha gradually changes to beta	1·4
810	(loss of magnetism)	
919	Beta suddenly changes to gamma	6·7
1405	Gamma suddenly changes to delta	1·9
1528	The iron changes from solid to liquid	49·3

The temperature at which the change from alpha to beta takes place in pure iron is apt to vary by a few degrees, possibly as much as 10° either way. These variations, which appear to be due to the presence of gas dissolved in the metal, do not affect the extent of the range of temperature within which the change occurs. In ordinary iron, pure or of industrial quality, the range is always about 40° C., and although the same attention has not been paid to the other magnetic elements, enough is already known to establish the rule that each has its own characteristic range within which the magnetic change occurs.

The facts, which I have sketched as briefly as possible, all point one way. For a mass of magnetic molecules there is certainly no critical temperature; and in view of the wide, and apparently widening, gap between experimental knowledge of ferromagnetism and the theoretical work of continental physicists, it would be useful at this juncture to have a statement of what is meant by the Curie point, and in what way it is related to the range of temperature occupied by the magnetic change? As one who remembers Hopkinson and all he did for scientific progress I should like to suggest that the temperature range of a ferromagnetic substance should be called the "Hopkinson range," he having been the first to investigate it.

An ambiguity occurs in Mr Powell's paper on p. 398, where he gives, for iron, a calculated value for the observed energy, on the basis of Heisenberg's theory, namely 4·4 calories *per degree per gram-atom*. This result is then compared with an observed value 6·8. Similarly the calculated value for nickel is given as 3 calories *per degree per gram-atom*, and compared with an observed value 1·7. But these two observed values are clearly energy in calories *per gram of metal* and hence, unless there has been some slip in stating the result of calculation, the comparison between theory and experiment falls to the ground. No doubt Mr Powell will clear up this doubtful point before his paper appears in the *Proceedings*. Whatever

the event may be, I should prefer to see figures relating to atoms stated in terms of the actual weight of the atom, since the weight of the hydrogen atom is now known with some precision. Surely the time has come to abandon the clumsy gram-atom, the unit of antiquated chemistry.

Mr R. H. Fowler. I want first to comment on some of the difficulties pointed out by Dr Bates. First of all about the step in the specific heat for Fe which may be as much as 8 cal./gm.-mol. I do not think there is any real difficulty in this. Admittedly one cannot work a theory for iron with one magnetizable electron per atom. But there is no reason why there should not be three per atom, and then very likely the theory will work all right, though the exact details are still lacking in spite of Heisenberg's extensions of his original paper*. Of course the enormous step for MgAs is quite outside the possible range of any existing theory. It is extremely interesting and I must congratulate Dr Bates on finding it.

Then again I think Dr Bates said that the specific heats above the Curie point were too high, being about 10 cal./gm. instead of 6 cal./gm. But as he said that the step in Fe and Ni was not too far out, presumably therefore the specific heat is too high in this sense, both above and below the Curie point. I see nothing strange in this. Such extra specific heats are well known in other substances and probably have nothing whatever to do with magnetism.

I should like to supplement Mr Powell's remarks on his determination of $\partial J_0/\partial a$, especially with regard to the sign (+) for Fe. This is most unexpected as it means that the interaction integral, which increases very rapidly as the distance diminishes at large distances, has passed its maximum and is decreasing again as we put the atoms still closer together. This suggests that an approximate theory, using only nearest neighbours, is in some danger of overlooking terms almost as large arising from next nearest neighbours. This point requires further examination.

In conclusion, I should like to remind you that a real theory of metals should be able to answer the question why an assembly of, say, normal Cu atoms with a suitable given energy-content is a metal and not, for example, a gas. As yet we have only made the very first beginning of an attack on this—the real problem of the theory of metals. This beginning has been made by Slater in his recent work in the *Physical Review* and extended by Block for use in the theory of ferromagnetism. I think this work marks the taking of a new step forward of first class importance.

Prof. A. M. Tyndall said that the results obtained by Mr Sucksmith for the gyromagnetic ratio represent work whose difficulty is far greater than his paper indicates, particularly as regards the extreme standard of purity required in the specimens, which must not be handled, for instance.

Dr G. Temple (communicated, and presented by Mr G. B. Sutherland). In his paper contributed to this discussion Prof. Allen has drawn attention to the quantization of the effective magnetic induction in the rigid rotator. I prove below that this

* See the contribution by Mr F. C. Powell.

result also applies to the hydrogen atom, as was suspected by Prof. Allen. When the principal quantum number, n, is equal to 1, 2 or 3 the result may be obtained by direct integration of the expression for the current density, and these calculations have been actually carried out by Mr G. B. Sutherland of Trinity College. But for higher values of n this method becomes extremely laborious. Fortunately the integrations may be evaded by the device given below, which applies in the most general case.

The effective magnetic induction is defined by analogy with the induction of a current flowing in a closed linear circuit. If j is the strength of the current and L the self-induction of the circuit, the current energy is

$$E = \tfrac{1}{2}Lj^2,$$

and the magnetic induction is

$$N_m = Lj = 2E/j.$$

Prof. Allen takes the same relation, $N_m = 2E/j$, to define the effective magnetic induction in an atom possessing a magnetic moment. In this case E is the total atomic energy in a particular quantum state and j is the total current crossing a semi-infinite plane bounded by the magnetic axis of the atom. The quantization of N_m is expressed by the equation

$$N_m = nh/e,$$

which may be proved as follows.

With the usual notation for the hydrogen atom, the current velocity at the point (r, θ, ϕ) is

$$v = \frac{h}{2\pi m_0} \cdot \frac{m}{r \sin\theta},$$

and is in the direction of increasing ϕ. The element of area in a plane passing through the magnetic axis ($\theta = 0$) is $r\,d\theta\,dr$. Hence the total current crossing the half of this plane on one side of the axis is given in e.s.u. by

$$j = \int_0^\infty \int_0^\pi \rho v . r\,d\theta\,dr,$$

where ρ is the charge density. Since $r^2 \sin\theta\,d\theta\,d\phi\,dr$ is the element $d\tau$ of volume,

$$j = \frac{1}{2\pi} \int \frac{v}{2 \sin\theta} \cdot \rho d\tau = \frac{hm}{4\pi^2 m_0} \int \frac{\rho}{r^2 \sin^2\theta}\, d\tau,$$

the integral being taken over all space.

To evaluate this integral we note that the wave equation,

$$\frac{1}{r^2}\frac{\partial}{\partial r}\left(r^2 \frac{\partial \psi}{\partial r}\right) + \frac{1}{r^2 \sin\theta}\frac{\partial}{\partial \theta}\left(\sin\theta \frac{\partial \psi}{\partial \theta}\right) + \frac{m^2}{r^2 \sin^2\theta}\psi + \frac{8\pi^2 m_0}{h^2}\left(E + \frac{Ze^2}{r}\right)\psi = 0,$$

involves the expression $(r \sin\theta)^{-2}$ for which we require the mean value. Now Schrödinger's solution of this equation shows that the eigen values of

$$\lambda = (8\pi^2 m_0/h^2)E$$

are of the form

$$\lambda_n = -\frac{1}{a^2 n^2},$$

where $(n - m)$ is a positive integer and a, the radius of Bohr's "ground orbit," is $h^2/(4\pi^2 m_0 e^2 Z)$. This result persists even if m is not integral. Thus, if we replace m by $(m + c)$, where c is a small constant, n is replaced by $(n + c)$, and λ_n by

$$-\frac{1}{a^2(n+c)^2} = -\frac{1}{a^2 n^2} + \frac{2c}{a^2 n^3} + \ldots.$$

The new wave operator differs from the old by the addition of the perturbation term

$$\frac{2mc + c^2}{r^2 \sin^2\theta}, \text{ which } = \frac{2mc}{r^2 \sin^2\theta} + \ldots.$$

Now the ordinary perturbation theory indicates that the change in λ_n is equal to the integral

$$\int \frac{2mc}{r^2 \sin^2\theta} \cdot \psi^* \psi \, . \, d\tau,$$

and we know that $\rho = -e\psi^*\psi$.

Hence
$$m \int \frac{\rho}{r^2 \sin^2\theta} \, d\tau = \frac{-e}{a^2 n^3},$$

and
$$j = \frac{-eh}{4\pi^2 m_0 a^2 n^3}$$
$$= \frac{eh\lambda_n}{4\pi^2 m_0 n} = \frac{2eE_n}{nh}.$$

Therefore
$$N_m = 2E/j = nh/e.$$

Dr L. G. Carpenter. With regard to the statement of Dr Bates that a satisfactory theory of the specific heat of ferromagnetic substances must explain, amongst other things, "the large minimum value of the specific heat above the magnetic critical point," it should be remembered that the property of possessing an atomic heat at constant volume which exceeds $3R$ is not confined to ferromagnetics. It occurs to some extent in all metals at high temperatures, and in the case of tungsten, for instance, the atomic heat at constant volume is of the order of 7 or 8 calories at 1200° abs. Incidentally Klinkhardt's value of the atomic heat of iron above the Curie point, to which Dr Bates refers, is the atomic heat at constant pressure. The atomic heat at constant volume will be of the order of 1 calorie less than this.

On the whole, therefore, as Dr Fowler has already pointed out, it does not seem necessary to demand of the theory of the specific heat of ferromagnetic substances that it shall explain the large minimum value of the specific heat above the magnetic critical point.

Dr J. R. Ashworth. Prof. Gerlach's results on the correspondence between the change of electrical resistivity (σ) of nickel with temperature and the variation of the true specific heat as a function of the temperature (t) up to and above the magnetic critical temperature are similar to my own published in the *Philosophical*

*Magazine**. Experiments described there showed that the ratio of $d\sigma/dt$ at air temperature and at the critical temperature, for both nickel and iron, is closely the ratio of the true and specific heat at air temperature and the critical temperature. The conclusion then reached was that "there seems little doubt that the large and increasing change of resistivity up to the critical temperature is due to the increase of the specific heat; and as this in its turn depends on the mutual magnetic forces between the molecules we have an explanation why ferromagnetic substances exhibit an abnormally large change of resistivity with rise of temperature." It was further shown that corresponding states hold at least for nickel and iron approximately in regard to the change of resistivity as well as the change of thermo-electric power with temperature. No doubt the large change of the specific heat with rise of temperature in ferromagnetics is responsible for most of the abnormally large changes in other properties with rise of temperature which ferromagnetics exhibit up to the critical temperature.

The e.m.f. which Prof. Gerlach finds between a magnetized part of a piece of nickel below and an unmagnetized part above the critical temperature seems to be analogous to the e.m.f. found between a magnetized electrode of nickel or iron and an unmagnetized electrode of the same metal when they are in a dilute electrolyte. Hurmuzescu† and Paillot‡ made measurements of this e.m.f. and found it to be of the order 10^{-6} volts in fields of 10,000 gauss. This is of the same order of magnitude as that found by Prof. Gerlach.

The theory of this effect§ shows that an e.m.f. of this magnitude would arise if the magnetized electrode possessed a field of the order of the intrinsic field of a magnet, namely, about 10^7 gauss.

Dr H. R. Lang referred to the magnetic researches of Baron von Reichenbach, the earlier results of which were published in 1845‖. Surprising phenomena had been obtained with magnets, such as a glow of light surrounding the poles. This effect he connected with the aurora borealis. An English translation of the work was published in 1850 by Taylor, Walton, and Maberly.

Prof. B. W. Holman. I also have read the work of Baron von Reichenbach with considerable interest. Although that writer's physics was far more original than classical, his work deserves more attention than it has received, because he was a mineralogist of eminence, and because his work was a lengthy record of alleged experimental results attained by him. It was not a record of mathematical exercises. It might interest some members to know that a worker at Faraday House has systematically investigated the extraordinary phenomena recorded by von Reichenbach—in particular the rendering of galena and quartz responsive to an ordinary permanent magnet. I have been shown a piece of transparent mineral which was apparently quartz (I was not allowed to touch it), and responded to a permanent

* *Phil. Mag.* **43**, 401 (1922). † D. Hurmuzescu, *J. de Phys.* **4**, 118 (1895).
‡ R. Paillot, *Comptes Rendus*, **131**, 1194–5 (1900).
§ J. R. Ashworth, *Mem. Manchester Lit. and Phil. Soc.* No. 11 (1914).
‖ Liebig's *Annalen* (March and May, 1845).

magnet and had pronounced phosphorescence. The phosphorescence was so strong that it could be observed readily when the piece of alleged quartz was placed on the shelf of a cupboard in the laboratory and the doors partially closed. Another point of interest in connection with this work of Baron von Reichenbach was that he selected the minerals—galena, blende, chalcopyrite, etc.—in that order, as those which were most readily rendered "magnetic." It will be noted that the minerals he gives (including others not mentioned here) and the order in which he gives them agree with the order of suitability of such minerals for crystal receiving-sets in wireless.

The complete neglect of this rather fantastic work is paralleled in the complete neglect of the very remarkable and by no means fantastic experimental work of Mr W. M. Mordey on the behaviour of paramagnetic substances in two-phase alternating fields. Mr Mordey demonstrated many of these phenomena before the Royal Society and the Royal Institution a good many years ago. The fact that an electromagnet can be made to repel magnetic substances—and not only to repel them but to repel them with a considerable and easily-measurable force which is not at all proportionate to the paramagnetic constant of the material repelled—is a remarkable fact. Moreover, that when a series of such magnets is placed below a dish or plate of glass, certain materials move rapidly in a horizontal direction, and move in a direction at 180° different from that demanded by electromagnetic theory, appears to be a result of fundamental interest—one which should have excited widespread investigation. As a matter of fact, these and many other peculiar and unpredicted quantitative results obtained by Mr Mordey have been completely ignored by nearly all writers on magnetism. In this connection it is interesting to note that Sir Alfred Ewing, in his paper on paramagnetism and hysteresis, ignores the years of careful work which Mr Mordey has put in on the hysteretic behaviour of metals and minerals in alternating current fields.

It is with a sense of deep disappointment that I have attended this discussion. It seems that fundamentally new results such as those of Mr Mordey and other workers in the field of mineral separation by magnetic methods have met with no attention. The majority of the papers appear to deal more with the elaboration of known phenomena, or the clothing of them in mathematical form. Dr Edmund Stoner, who has written a large book on *Magnetism and Atomic Structure*, and has also contributed to this discussion, likewise completely ignores the work of Mr Mordey, and of other people who have published fundamentally new results in this field, but have, like Mr Mordey, published facts and not mathematical hypotheses.

I would refer those who may, after these remarks, be tempted to acquaint themselves with the experimental findings obtained by Mr Mordey, to his lecture before the Royal Institution in 1924 and to his paper before the South African Institution of Electrical Engineers in 1930.

I hope that my remarks will be taken as constructive, not destructive, criticism. I wish more attention devoted to the work which has been done by investigators in the field of the magnetic separation of minerals, a field of work in which many

novel and interesting results—some of them in direct contradiction to accepted magnetic theory—have been obtained, and are relied upon for the sometimes unscientific purpose of making sufficient profits to pay dividends on the investments concerned. I do not wish my remarks in any way to be taken as belittling the wonderful work which has been carried out by one or two of those who have contributed to this discussion—foremost amongst these, of course, being Prof. Gerlach and Dr Kapitza.

Dr L. F. Bates. In reply to Mr R. H. Fowler and Dr Carpenter I think that I must adhere to the criticism that a satisfactory theory of the specific heats of ferromagnetic substances must explain the large minimum value of the specific heat above the magnetic critical point. It is true that Klinkhardt's value of the atomic heat of iron above the magnetic critical point to which I referred is the atomic heat at constant pressure, and that the atomic heat at constant volume will be somewhat less. But when this deduction is made, the minimum atomic heat still remains very large, and I think it is well to remind ourselves of it.

In regard to Mr Evershed's remarks concerning the use of the term "Curie point," I agree that the term is often somewhat loosely employed. Throughout my contribution to the discussion, I have used the term "magnetic critical point." This has been defined as the temperature at which the value of dI_0^2/dT is a maximum, and also as the temperature at which the intrinsic magnetization disappears. In my opinion it is very desirable to adopt the former definition, and then many of the difficulties mentioned by Mr Evershed disappear.

Mr F. C. Powell. Mr Evershed raises an important question when he points out that the transition from the ferromagnetic to the paramagnetic state does not occur at a critical temperature, but in a temperature range. There is, in fact, actually no temperature which possesses all the properties of the theoretical Curie point. Some of these theoretical properties may be briefly mentioned. Below the Curie point T_c the substance possesses a spontaneous magnetization σ (certainly on the micro-scale, but not necessarily in bulk) which tends to zero as the temperature T approaches T_c, in such a way that $d\sigma^2/dT$ approaches a finite value. Above the Curie point the substance is paramagnetic. One would expect to find discontinuities in the specific heat and the coefficient of thermal expansion arising from the discontinuity in $d\sigma^2/dT$.

T_c, σ

T

The experimental magnetization/temperature curves obtained for iron and nickel show that σ tends to zero in the required way as the temperature is raised, except for very small values of σ, where the curve flattens out. This flattening out may be taken as a first indication of a critical range of temperature in which the normal ferromagnetic properties are replaced by normal paramagnetic properties, in a way which is theoretically inexplicable at present. It should be noted that $d\sigma^2/dT$ and hence the specific heat and coefficient of thermal expansion do not change discontinuously, but rise up sharply to maxima (or descend sharply to minima) just below the critical range. The existence of this critical range shows that some factor has been overlooked by the theory, but it is small and its effect is marked only

in the neighbourhood of the critical temperature. One would not be justified in applying the theory within the critical range, where the factor is important, but presumably the theory may be applied outside that range without serious error.

In calculating the change of specific heat at the Curie point one compares the specific heat where $d\sigma^2/dT$ is zero (i.e. just above the Curie point) with the specific heat where $|\, d\sigma^2/dT \,|$ is greatest (i.e. just below the Curie point); if, owing to the presence of some extra factor, the Curie point is replaced by a critical range, the calculated change of specific heat may be interpreted as the difference between specific heats just above and just below the critical range (provided the latter be not too large). Now this is precisely the experimentally observed quantity which is usually called the change of specific heat at the Curie point, although what is really meant is the difference between the specific heat in the paramagnetic state and the specific heat at the maximum which occurs just *below* the critical range (not *in* the critical range, since the maximum occurs where $|\, d\sigma^2/dT \,|$ is greatest).

It would thus appear that although the presence of some factor overlooked by the theory modifies considerably the transition from the ferromagnetic to the paramagnetic state, the calculated change of specific heat at the Curie point may justly be compared with the experimental values. The occurrence of the Curie point in the theory does not invalidate the theory completely, while the use of the term in describing the experimental results is purely for convenience, and does not imply an unfortunate choice of data. A similar argument holds for any quantity which theoretically shows a discontinuity at the Curie point. It is not clear which temperature ought to be taken for the T_c which occurs in the formulae, but for most purposes it makes no practical difference which temperature in the critical range is selected. Mr Evershed suggests that I have made a slip in stating the experimental values of the specific-heat changes for iron and nickel, but they are, I believe, correctly given. The values were obtained by Weiss, Piccard, and Carrard*. I should like to draw attention to the more recent measurements of Umino†, which unfortunately I had previously overlooked. From these Honda‡ obtains the values for the specific-heat changes given in the following table:

Substance	ΔC_v cal. per degree per gram-atom
Iron	4·6
Nickel	2·1

These values are in much better agreement with the calculated values.

It should be emphasized that in obtaining the theoretical values which I gave, I neglected the Gaussian distribution of energy levels, a step which is justifiable only when z, the number of atoms whose interaction with a given atom must be taken into account, is large. But if, as Heisenberg supposed, interactions with nearest

* Weiss, Piccard, and Carrard, *Arch. Sci. Phys. Geneva*, **42**, 378 (1917); **43**, 22, 113, 119 (1917).
† S. Umino, *Sci. Rep. Tohoku*, **16**, 593, 1009. ‡ K. Honda, *Zeit. für Phys.* **63**, 141 (1930).

neighbours only are important, somewhat lower values for ΔC_v are obtained, as shown in the following table:

Substance	Number of electrons per atom	z	ΔC_v cal. per degree per gram-atom
Iron	3	8	4·1
		∞	4·4
Nickel	1	12	2·2
		∞	3·0

We should expect the experimental values to lie somewhere between the two limiting calculated values. The above experimental values lie in both cases just outside the theoretical range.

THE INDUCTION OF ELECTROMOTIVE FORCES IN A MOVING LIQUID BY A MAGNETIC FIELD, AND ITS APPLICATION TO AN INVESTIGATION OF THE FLOW OF LIQUIDS

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ABSTRACT. A magnetic field induces electromotive forces in a moving liquid, and by investigation of the e.m.fs. produced by a known magnetic field it is possible to obtain information about the distribution of velocities in the liquid. Experiments on the flow through straight tubes show that potential differences of the order of 10^{-4} to 10^{-3} volts, set up by a magnetic field in a moving liquid consisting of an aqueous solution of copper sulphate, can be satisfactorily measured. Observations on the flow through curved tubes have also been made.

§ 1. INTRODUCTION

It follows from Faraday's laws of electromagnetic induction that electromotive forces are induced in a liquid which is in motion in a magnetic field, the e.m.f. per cm. at any point being equal to the vector product of the magnetic force and the velocity. The velocity at any point may therefore be calculated from a knowledge of the induced e.m.f. and the field strength. This fact may be used for investigating the distribution of velocities in a moving liquid, for this distribution can be deduced from observations on the e.m.fs. induced at different points in the liquid by a known magnetic field.

This method of investigating the flow of liquids has the advantage of using the very simple and universally valid relation which exists between the velocity and the induced electromotive force. As the latter depends upon the direction of the velocity as well as its magnitude, the method differentiates between motions in different directions. It also has the advantage that the e.m.fs. are induced instantaneously, so that in the case of unsteady motion velocity-alternations of high frequency are as faithfully represented as those of low frequency*.

The method involves the measurement of electrical potential differences, and its successful application depends upon the magnitudes of the e.m.fs. induced in actual cases and their relation to the magnitudes of any spurious potentials which may exist. For a velocity of 1 cm./sec. the gradient of e.m.f. induced by a field of 10,000 gauss is 10^{-4} volt/cm., whilst a moderately good galvanometer is capable of registering 10^{-6} to 10^{-7} volts. The order of magnitude of the e.m.f. is thus

* The only limitation in this respect is due to the inductance of measuring apparatus.

sufficiently large to offer reasonable scope for accurate measurement. Experiments which have been made on the steady flow through a straight cylindrical tube for which the distribution of velocities is known show that induced potential differences of the order of 10^{-3} volts or greater, set up in a moving liquid consisting of an aqueous solution of copper sulphate, correspond satisfactorily to the theoretical e.m.fs. required by Faraday's laws of induction, spurious effects being comparatively small. In this connection it might also be pointed out that the potential differences dealt with in experiments on the effect of a magnetic field on the electrical resistance of liquid metals are due to e.m.fs. induced in a moving liquid by a magnetic field, and in such experiments induced e.m.fs. as small as 10^{-6} to 10^{-7} volts have been measured without hindrance from spurious effects*.

In applying the above method of investigating the flow of liquids it is in general necessary to introduce "searching" electrodes into the liquid in order to determine the e.m.f., and these may disturb the flow. It is also necessary to use an electrically conducting liquid. These difficulties may be met by the use of fine wires as electrodes and of mercury or aqueous solutions of salts as the moving liquid. In the present experiments an aqueous solution of copper sulphate was used, whilst the electrodes were made of copper. Under such conditions it was found that though no polarisation e.m.fs. are produced at the electrodes, contact potentials of appreciable magnitude are set up. As was indicated above, the induced e.m.f. in these experiments was of the order of 10^{-4} to 10^{-3} volts, and in comparison with this voltage the contact potentials were found to be reasonably constant and did not seriously affect the measurements. A more serious limitation to the present method arises from the fact that the quantity that can be measured directly by experiment is the potential difference between two points in a liquid and not the e.m.f., and these are not equal when electric currents are induced in the liquid. Fortunately in a large number of cases which are of particular interest the induced currents are either small or entirely absent and in such cases the quantitative validity of the method is not impaired. For cases in which the induced currents are expected to be large the method may still be used to give an accurate description of certain features of the motion, such as the frequency of velocity fluctuations in turbulent motion, even though the magnitude of the velocities cannot be accurately determined. It is possible to proceed in certain special cases of this kind by assuming a velocity distribution and allowing for the effect of induced current in calculating the corresponding potential gradient. The calculated potential gradient may then be compared with the observed potential gradient. This procedure, which is described more fully in § 4, is especially successful in the case of flow through straight pipes.

In those cases in which induced currents are produced, the Ampère forces between the magnetic field and the induced currents may modify the original flow, and this is another possible source of trouble from induced currents†. When the

* See accompanying paper by the author, p. 479.

† This secondary effect gives rise to an additional resistance to the flow of the liquid, and the extra work done is equal to the energy dissipated by the induced currents.

liquid is an aqueous solution the induced currents are so small, owing to the high resistivity of the liquid, that this secondary effect of the field is negligible*. Even for good conductors such as mercury the difficulty may be met by the use of low magnetic fields. The corresponding reduction in the magnitude of the induced e.m.f. is compensated by the fact that in the case of mercury it is possible to measure induced e.m.fs. of a much smaller magnitude than in the case of aqueous solutions, owing to the comparative absence of spurious effects arising from contact potentials.

A further discussion of the e.m.fs. and currents induced in various types of flow is given in §§ 2, 3 and 4. General equations for the induced currents are given in § 2, and these are applied to special cases in §§ 3 and 4.

An account of the present experiments is given in § 5. The experiments on the effect of a magnetic field on the electrical resistance of mercury, which also show the practicability of the measurement and interpretation of the e.m.fs. induced by a magnetic field in a moving liquid, are discussed in the subsequent paper.

§ 2. GENERAL DIFFERENTIAL EQUATIONS

u, v, w Let u, v, w be the components of the velocity of the liquid at x, y, z;

α, β, γ α, β, γ those of the magnetic force;

E_x, E_y, E_z E_x, E_y, E_z those of the induced e.m.f. per cm.;

I_x, I_y, I_z I_x, I_y, I_z those of the electric current per sq. cm.;

X, Y, Z X, Y, Z those of the electrostatic potential gradient†;

V V the electrostatic potential; and

σ σ the electrical conductivity of the liquid.

Then, from Faraday's laws of induction,

$$E_x = \beta w - \gamma v,$$
$$E_y = \gamma u - \alpha w,$$
$$E_z = \alpha v - \beta u \qquad \text{.....(1)‡.}$$

From Ohm's law the electric current is given by

$$I_x = \sigma (E_x + X),$$
$$I_y = \sigma (E_y + Y),$$
$$I_z = \sigma (E_z + Z) \qquad \text{.....(2).}$$

From (1) and (2)

$$\frac{\partial I_x}{\partial y} - \frac{\partial I_y}{\partial x} = \sigma \left(\frac{\partial E_x}{\partial y} - \frac{\partial E_y}{\partial x} \right) + \sigma \left(\frac{\partial X}{\partial y} - \frac{\partial Y}{\partial x} \right).$$

* The order of magnitude of the ratio of the Ampère forces to the mechanical forces is represented by $(H^2/\rho\eta) \times 10^{-9}$, where ρ is the resistivity expressed in ohm-cms., and η is the viscosity.

† Due to space and surface charges, set up by the induced e.m.f.

‡ The currents induced in the liquid produce a secondary magnetic field. This field is, however, negligible in comparison with the primary field and in all actual cases α, β and γ may be taken as the components of the primary field.

As the electric potential is single valued $\partial X/\partial y - \partial Y/\partial x = 0$, so that

$$\frac{\partial I_x}{\partial y} - \frac{\partial I_y}{\partial x} = \sigma\left(\frac{\partial E_x}{\partial y} - \frac{\partial E_y}{\partial x}\right) = \sigma\left\{\frac{\partial}{\partial y}(\beta w - \gamma v) - \frac{\partial}{\partial x}(\gamma u - \alpha w)\right\}.$$

The conditions of continuity of velocity and magnetic force are

$$\frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0; \qquad \frac{\partial \alpha}{\partial x} + \frac{\partial \beta}{\partial y} + \frac{\partial \gamma}{\partial z} = 0,$$

and hence

$$\frac{\partial I_x}{\partial y} - \frac{\partial I_y}{\partial x} = \sigma\left\{\left(\alpha\frac{\partial}{\partial x} + \beta\frac{\partial}{\partial y} + \gamma\frac{\partial}{\partial z}\right)w - \left(u\frac{\partial}{\partial x} + v\frac{\partial}{\partial y} + w\frac{\partial}{\partial z}\right)\gamma\right\}.$$

Similarly

$$\frac{\partial I_y}{\partial z} - \frac{\partial I_z}{\partial y} = \sigma\left\{\left(\alpha\frac{\partial}{\partial x} + \beta\frac{\partial}{\partial y} + \gamma\frac{\partial}{\partial z}\right)u - \left(u\frac{\partial}{\partial x} + v\frac{\partial}{\partial y} + w\frac{\partial}{\partial z}\right)\alpha\right\},$$

$$\frac{\partial I_z}{\partial x} - \frac{\partial I_x}{\partial z} = \sigma\left\{\left(\alpha\frac{\partial}{\partial x} + \beta\frac{\partial}{\partial y} + \gamma\frac{\partial}{\partial z}\right)v - \left(u\frac{\partial}{\partial x} + v\frac{\partial}{\partial y} + w\frac{\partial}{\partial z}\right)\beta\right\} \qquad \text{.....(3).}$$

In the case of steady motion the current also obeys the condition of continuity so that

$$\frac{\partial I_x}{\partial x} + \frac{\partial I_y}{\partial y} + \frac{\partial I_z}{\partial z} = 0 \qquad \text{.....(4)*.}$$

The potential difference between any two points is given by

$$V_A - V_B = \int_A^B X\,dx + Y\,dy + Z\,dz \qquad \text{.....(5),}$$

V_A, V_B

where the electric force is given in terms of the induced current and e.m.f. by equation (2).

§ 3. TWO-DIMENSIONAL FLOW

Let us consider two-dimensional flow in the xy plane in which case $w = 0$. We shall also suppose that the magnetic field is uniform and parallel to the z axis. Under such conditions the expressions on the right-hand side of (3) vanish so that no electric current is induced in the liquid†. The induced e.m.f. sets up space and surface charges and the electrostatic field due to these charges exactly neutralizes the e.m.f. The electromotive force between any two points is therefore equal but opposite in direction to the potential difference between the points, and the components of velocity are given by

$$v = -E_x/\gamma = X/\gamma,$$
$$u = \quad E_y/\gamma = -Y/\gamma \qquad \text{.....(6),}$$

* The electrical capacity of the liquid is so small that even for unsteady motion this condition is obeyed to all intents and purposes.

† This may readily be seen from the fact that the total amount of liquid which flows out of a closed circuit in the xy plane is zero. For if u_n be the velocity normal to an element ds of the circuit the integral of the e.m.f. round the circuit $= \oint E_s\,ds = \oint \gamma u_n\,ds = \gamma \oint u_n\,ds = 0$.

the components X and Y of potential gradient being measurable by means of an external circuit.

Thus in the case of two-dimensional flow no complications arise from the effect of induced currents. This case includes several types of flow which are of particular interest, such as the motion past an infinite cylinder or aerofoil.

The expressions on the right-hand side of (3) also vanish, even if w is not zero, provided $\partial w/\partial z = 0$. This includes the case of flow through a pipe in which a circulation in the plane of the section of the pipe is superimposed upon the longitudinal motion. The circulation postulated by Dean* in the case of flow through a curved pipe may therefore be investigated without any uncertainties caused by induced currents.

§ 4. CALCULATION OF POTENTIAL GRADIENT FOR CERTAIN CASES IN WHICH INDUCED CURRENTS ARE PRODUCED

If induced currents are produced in the liquid then the potential gradient depends not only upon the e.m.f. but also upon the electric current. Whilst the e.m.f. at any point depends only upon the velocity and the field at that point the induced electric current theoretically depends on the velocity and field at all other points of the liquid. The potential gradient at a point is therefore not completely determined by the conditions prevailing at that point. In the case of turbulent motion it is, however, probable that the fluctuating part of the potential gradient is mainly dependent on the velocity fluctuations in the immediate neighbourhood of the point concerned, so that the existence of induced currents is not of much consequence. The average potential gradient in turbulent motion, or the steady potential gradient for stream-line motion, may on the other hand be appreciably dependent on the flow in parts of the liquid remote from the point investigated. In such cases it appears that the only way in which the method can be quantitatively applied is to assume a distribution of velocities and calculate the corresponding distribution of induced currents and potential gradient. A comparison of the calculated potential gradient with the observed potential gradient then indicates the accuracy of the distribution of velocities assumed. Such a procedure would, probably, be rather elaborate in most actual cases. We shall consider here the special case of flow through a straight tube and calculate the induced currents and potential gradient produced by a transverse uniform magnetic field. This is the case which has been dealt with in the experiments.

The distribution of velocity over the cross-section of a straight pipe of radius a has been the subject of much investigation. The distribution for stream-line flow is well established and is represented by

$$w = (2Q/\pi a^4)(a^2 - r^2) = k(a^2 - r^2) \qquad \ldots\ldots(7),$$

w r Q where w is the velocity parallel to, and at a distance r from, the axis of the pipe, and Q is the volume of liquid flowing through per second. For turbulent flow the

* *Phil. Mag.* 5, 673 (1928).

velocity distribution is not so definitely established. The velocity (or more precisely the average longitudinal velocity) falls off much less rapidly with increasing value of r than in stream-line flow, and the velocity gradient lies between that represented by (7) and the zero gradient in a constant velocity distribution. A distribution of velocities in which w is proportional to $(a^4 - r^4)$, i.e.

$$w = (3Q/2\pi a^6)(a^4 - r^4) = k'(a^4 - r^4) \quad \text{.....(8),} \qquad k'$$

gives a velocity gradient between these extremes, and though it may not represent the actual gradient in turbulent flow it can be combined with these extreme distributions to give other intermediate distributions. We shall now consider the distribution of potential gradient corresponding to the velocity distributions (7) and (8), produced by a transverse magnetic field.

Let us choose the x axis parallel to the magnetic field and the z axis parallel to the axis of the pipe. Then $\beta = \gamma = 0$, $u = v = 0$. The e.m.f. is confined to the xy plane (at right angles to the velocity) and is independent of z, in which case the equations (3) and (4) for the induced current reduce to

$$\partial I_x/\partial y - \partial I_y/\partial x = \sigma\alpha . \partial w/\partial x,$$
$$\partial I_x/\partial x + \partial I_y/\partial y = 0 \quad \text{.....(9),}$$

where w is given by (7) or (8). The walls of the pipe may be assumed to be insulating so that the electric current normal to the walls is zero. From this boundary condition and equations (9) and (7) it is found by solving a Poisson equation that the induced current for stream-line flow is given by

$$I_r = (k\alpha\sigma/4)(a^2 - r^2)\sin\theta,$$
$$I_\theta = -(k\alpha\sigma/4)(a^2 - 3r^2)\cos\theta \quad \text{.....(10),}$$

where I_r and I_θ are the radial and tangential components respectively. (θ is the angle the radius vector makes with the field, i.e. with the x axis.)

The electromotive force is equal to $\alpha \times w$ and is parallel to the y axis. Its components are therefore

$$E_r = k\alpha(a^2 - r^2)\sin\theta,$$
$$E_\theta = -k\alpha(a^2 - r^2)\cos\theta \quad \text{.....(11).}$$

The components of potential gradient are therefore, by equation (2),

$$R = I_r/\sigma - E_r = -k\alpha(a^2 - r^2)\sin\theta = -(3Q\alpha/2\pi a^4)(a^2 - r^2)\sin\theta, \qquad R$$
$$\Theta = I_\theta/\sigma - E_\theta = k\alpha(3a^2 - r^2)\cos\theta = (Q\alpha/2\pi a^4)(3a^2 - r^2)\cos\theta \quad (12). \qquad \Theta$$

R and Θ can be determined experimentally, and in view of the fact that the velocity distribution upon which (12) is based is well established, the observed potential gradient for steady flow should agree with (12). Observations on the potential gradient produced in this case are described in the next section.

The general nature of the distribution of induced current is represented in Fig. 1. It consists of two "circulations" symmetrical about the y axis, the current vanishing at two points on the x axis at distance $a\sqrt{3}$ from the origin. This

distribution is similar to the hydrodynamic circulation considered by Prof. Dean in the case of flow through a curved pipe provided the axis of curvature of the pipe in that case is chosen parallel to the magnetic field in the present case. Though the differential equations for the two cases are not identical there is a general analogy. The e.m.f. in the present case corresponds to the centrifugal force in the case of the hydrodynamical circulation produced in a curved pipe. Both are greater the greater the longitudinal velocity, so that in both cases the force near the axis is much greater than the force near the walls. This gives rise to the circulation.

The difference between the e.m.f. represented by (11) and the potential gradient represented by (12) is due to the potential gradient caused by induced currents and is

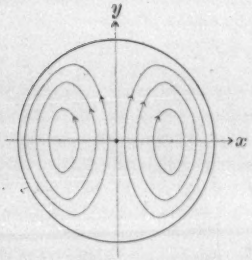

Fig. 1. Distribution of induced current in liquid in a straight pipe.

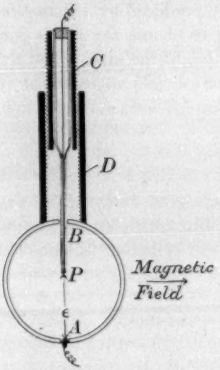

Fig. 2. Cross-section of tube showing electrodes.

about one-third of the total potential gradient. Neglect of the induced currents would therefore lead to appreciable error. As the velocity at the walls is zero, the e.m.f. vanishes at the walls, so that the tangential potential gradient at the walls, required by (12), is *all* due to the induced current.

The distribution of potential gradient corresponding to the velocity distribution for turbulent flow, equation (8), is given by

$$R = (Q\alpha/4\pi a^6)\{2r^4 \sin 5\theta - (7a^4 - 5r^4)\sin\theta\},$$

$$\Theta = (Q\alpha/4\pi a^6)\{-2r^4 \cos 5\theta + (7a^4 - r^4)\cos\theta\} \qquad \ldots\ldots(13).$$

Let us consider the potential gradient along a diameter perpendicular to the field (APB in Fig. 2). It can be shown from (12) and (13) that for both the velocity distributions (7) and (8) the potential difference between the ends of this diameter is given by

V_A, V_B

$$V_A - V_B = H\bar{w}d \qquad \ldots\ldots(14a),$$

$\bar{w}, d, H$ $\bar{w}$ being the mean velocity $Q/\pi a^2$, d the diameter of the tube, and H the strength

of the magnetic field (replacing α). The potential gradient along the diameter at any point is also in both cases proportional to the velocity, w, at that point, so that

$$\partial V_p/\partial \epsilon \propto Hw \qquad \ldots\ldots(14\,b),$$

ϵ being the distance between A and P. Equations ($14\,a$) and ($14\,b$) together determine completely the potential gradient along the diameter AB in terms of the velocity gradient. In the case of a distribution of constant velocity across the tube there are evidently no induced currents produced and the potential gradient at any point is therefore equal in magnitude to the gradient of induced e.m.f. at that point, i.e. equal to Hw and in a direction parallel to AB. It follows that the potential gradient at any point on the diameter AB is in this case also given by equations (14). There is little doubt that for all practical purposes the potential gradient along AB is given by equations (14) for all velocity distributions in which the rate of falling off of the velocity from the axis to the walls is somewhere between that in steady flow and a constant velocity distribution*. Thus in the case of flow through straight pipes the effect of induced currents can be calculated and allowed for and the gradient of velocity can be deduced, by means of equations (14), from observations on the potential gradient along a diameter perpendicular to the field.

§ 5. EXPERIMENTS ON THE FLOW THROUGH A CYLINDRICAL TUBE

Fig. 2 represents the cross-section of the tube and the arrangement of electrodes used, the magnetic field being at right angles to the axis of the tube in the direction shown. The potential differences between the point A at the end of the diameter perpendicular to the field and a point P on this diameter at different distances from A were measured.

The diameter of the glass tube used was 1·075 cm. Two small holes 1 mm. in diameter were bored at A and B. A fixed copper electrode passing through the hole at A was sealed so that its end was flush with the inside walls of the tube. The electrode passing through B was fixed in a brass tube C which could be moved in the direction AB by a screw movement in another brass holder D, the latter being waxed to the walls of the tube. The copper electrode extending into the tube was encased in a tight-fitting glass capillary with thin walls. This served to keep the wire rigid as well as to insulate it up to its end. The external diameter of the capillary was 0·05 cm. and therefore caused little obstruction to the flow. Copper sulphate solution was used as the moving liquid. The strength of the

* The reason for this result may be seen in a general way as follows. For the same mean velocity, $\bar{w}$, the integral of the gradient of e.m.f. along the diameter AB is greater the more rapidly the velocity falls off from the axis to the walls. It exceeds the value $H\bar{w}d$ for a constant velocity distribution by an amount depending on the steepness of this velocity gradient. However, the steeper this gradient the stronger are the induced currents, and the potential difference between A and B due to these must be subtracted from the integral of e.m.f. to give the resultant potential difference $(V_A - V_B)$. The result expressed by ($14\,a$) means that these effects cancel out leaving a resultant potential difference of $H\bar{w}d$ in all cases.

magnetic field used was about 10,000 gauss and in the experiments the change in the potential between A and P on reversal of the field was measured. This change in potential was measured by a potentiometer method. The arrangement is shown in Fig. 3. The resistance R was adjusted after the field had been reversed so that the current traversing the galvanometer G was the same as before the field was reversed, the initial current being partly due to contact potentials. The change in R then gives the potential difference between A and P due to the magnetic field*. In control experiments which were carried out, it was found that without the flow, or with the flow but with the electrodes nearly touching, no measurable potential difference was produced on application of the field. Of course any spurious potential gradient produced by the field that is scalar in nature leads to no error, provided, as in the present experiments, the effect of reversing the field is observed.

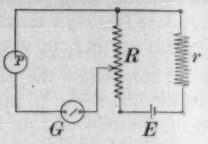

Fig. 3. Potentiometer.

The critical mean velocity for the tube used was about 21 cm./sec., and observations were made on three velocities, one much less than the critical velocity, the other in the neighbourhood of it and the third much greater than the critical velocity. The observed values of $V_A - V_B$ in these cases and the values calculated from (14 a) are given in Table 1. They are seen to be in very satisfactory agreement.

Table 1. Potential difference ($V_A - V_B$) between the ends of a diameter perpendicular to the field. ($H = 9500$ gauss.)

Mean velocity $\overline{w}$ (cm./sec.)	$V_A - V_B$ (millivolts)	
	Calculated value $H\overline{w}d$	Observed value
11·2	1·15	1·18
20·0	2·04	2·02
66·0	6·73	6·81

The estimated probable error in the calculated value due to errors in the measurement of H, $\overline{w}$ and d is about 1 to 2 per cent. The observed values which are given are each the result of several observations differing amongst themselves, as the result of contact potential effects, by about 0·05 to 0·1 millivolt. The estimated probable error in the mean observed values is about 0·03 millivolt. The agreement between these and the calculated values given in the second column of the table shows that there are no unknown sources of error involved in the measurement of induced potential differences under the conditions of the present experiments†.

* The change in R was negligible compared with the resistance of the circuit containing the electrodes so that this change negligibly affected the current through the galvanometer due to contact potentials.

† The difference of 0·08 millivolt between the observed and calculated values for the highest velocity is only a difference of 1 per cent. and may easily be due to the error in estimating the theoretical value.

The potential differences between the fixed electrode at A and the movable electrode P for different positions of the latter on the diameter AB were also measured, and the results show how the velocity varies from the axis of the tube to the walls. The results for the velocity of 11·2 cm./sec. in the region of steady flow and of 66·0 cm./sec. in the region of turbulent flow are represented in Figs. 4

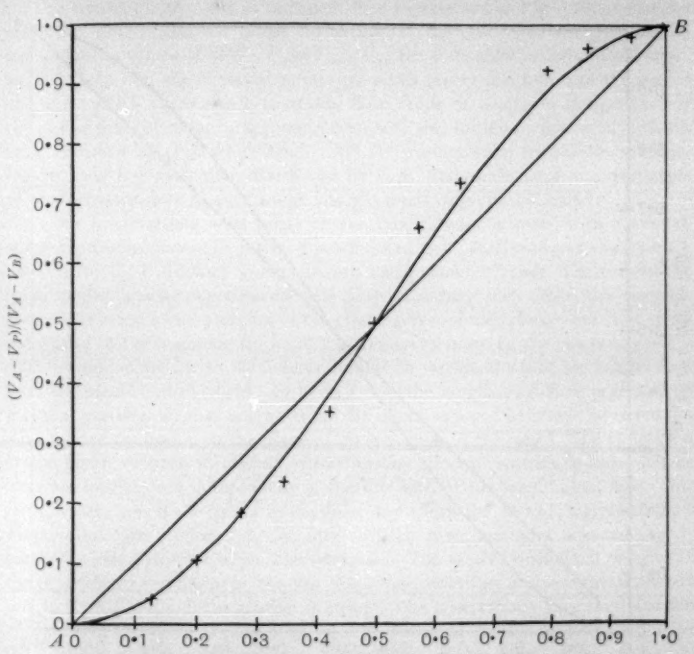

Fig. 4*. Variation of voltage along a diameter for steady flow.

× × observed values. Curved line AB: theoretical values. Straight line AB: values corresponding to a constant velocity across tube.

and 5 respectively. The ordinates represent $(V_A - V_P)/(V_A - V_B)$, and the abscissae the position of P in terms of ϵ/d. On these scales the positions of the observed points involve less error in the case of turbulent flow, since the potential differences measured in this case are on the average about six times greater than those measured in the other case.

The observed points in Fig. 4 clearly show the more rapid flow near the axis

* The observed value of $(V_A - V_B)$ was 1·18 mv. for Fig. 4 and 6·81 mv. for Fig. 5.

of the tube and an approach to a zero velocity near the walls. The theoretical value of $V_A - V_P$ in this case may be calculated from (12), or (14), and is given by

$$V_A - V_P = \int_{-a}^{\epsilon-a} (3QH/2\pi a^4)\,(a^2 - r^2)\,dr$$
$$= 3H\overline{w}d\,(1 - 2\epsilon/3d)\,\epsilon^2/d^2 = 3\,(V_A - V_B)\,(1 - 2\epsilon/3d)\,\epsilon^2/d^2 \ldots(15).$$

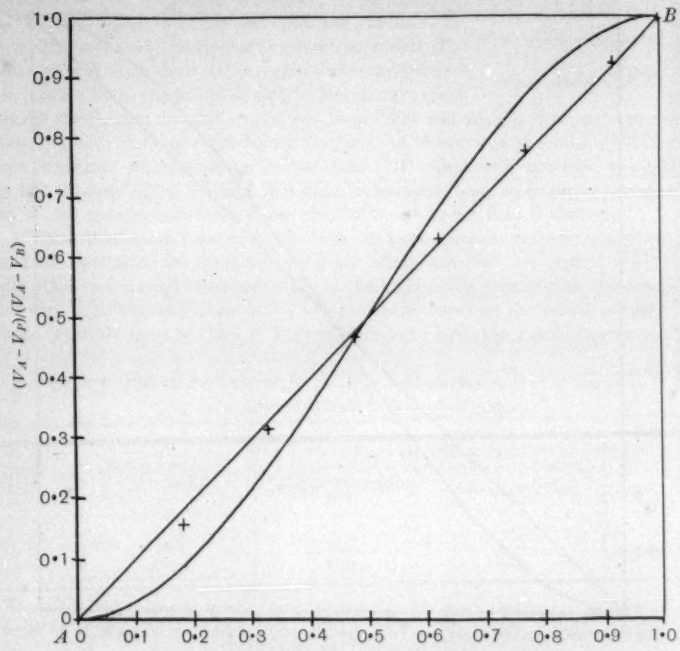

Fig. 5. Variation of voltage along a diameter for turbulent flow.
× × observed values. Curved line AB: theoretical values.
Straight line AB: values corresponding to a constant velocity across tube.

(15) is represented by the curved line AB in Fig. 4, and as it is based on the well established distribution of velocities in steady flow, the deviations of the observed points from this line show the accuracy with which the induced potential differences were measured. The root-mean-square deviation corresponds to about 0·02 millivolt. This is somewhat less than the differences between the observed and theoretical values in Table 1. This is to be expected, since in Table 1 the absolute observed and theoretical values are compared whilst in Fig. 4 it is the values of the ratio

$(V_A - V_P)/(V_A - V_B)$ that are dealt with, in which case errors in the measurement of the field and velocity do not come in. From the results in Table 1 and Fig. 4 we may conclude that potential differences induced in a moving liquid consisting of an aqueous solution of copper sulphate can be measured with the present arrangement with an accuracy of about one-thirtieth of a millivolt.

The results for the case of turbulent flow represented in Fig. 5 show that the velocity is nearly constant across the section of the tube. The straight line AB represents the values of $(V_A - V_P)/(V_A - V_B)$ for a constant velocity distribution, and it is seen that the observed points are much nearer this line than the curved line, AB, which corresponds to steady flow. It is of course to the gradient of $(V_A - V_P)$ that the velocity is proportional, and this cannot be measured with the same accuracy as $(V_A - V_P)$ itself. Still the results seem to indicate a bigger velocity near the walls than that found by F. E. Stanton in 1911 in experiments on the turbulent flow of air through straight pipes with smooth walls*.

A few observations were made on the flow through a spiral with a view to observing the circulation in curved pipes deduced by W. R. Dean† and subsequently observed by G. I. Taylor‡ in experiments using coloured bands. The coils of the spiral in the present experiments were nearly touching each other, the diameter of the coils being 4 cm. and that of the cross-section of the tube 0·7 cm. The spiral was placed in the magnetic field and measurements made on the potential along a diameter perpendicular to the field at a point on the spiral where the longitudinal flow was parallel to the field. In such a case the longitudinal flow produces no potential gradient so that any potential difference induced between the electrodes must be due to motion in the plane of cross-section of the tube§. The circulation in this plane, referred to above, arises from the effect of centrifugal force and its direction is therefore independent of the direction of the longitudinal flow. This circumstance was made use of to eliminate any effects due to the longitudinal flow which would be produced if, for instance, the latter were not adjusted exactly parallel to the field at the point investigated. The mean longitudinal velocity in the experiments was about 60 cm./sec., for which, according to an empirical relation due to C. M. White‖, the motion is steady. The observations gave no indication of motion in the plane of cross-section of the tube and the results show that under the conditions of these experiments the velocity, at any point, of the circulation described by Dean is probably less than $\frac{1}{20}$ of the longitudinal velocity at that point. In his experiments with coloured bands Taylor found that the ratio of the two velocities over a large range of longitudinal velocity was roughly equal to twice the ratio of the cross-sectional diameter to the diameter of the coils. In the present experiments this ratio is about $\frac{1}{8}$, and therefore from the results obtained for the velocities it appears that the relation mentioned by Taylor has no general applicability.

* *Proc. R.S.* **85**, 366 (1911). † *loc. cit.* ‡ *Proc. R.S.* **124**, 243 (1929).

§ Though induced currents are produced in other parts of the tube they vanish, by symmetry, in the section investigated.

‖ *Proc. R.S.* **123**, 645 (1929).

The above observations on the flow through a spiral were carried out chiefly to demonstrate the applicability of the method to the determination of the component of velocity in any direction, even though this component may be much less than the resultant velocity. In the above case, for instance, we are able to set to the velocity in a certain direction an upper limit which is about 20 times less than the resultant velocity.

§6. ACKNOWLEDGMENTS

I wish to thank Prof. W. L. Bragg, F.R.S. and Prof. D. R. Hartree, M.A. for their very kind interest in the work described in this paper.

DISCUSSION

For discussion see p. 485.

THE MOTION OF A LIQUID IN AN ENCLOSED SPACE

By E. J. WILLIAMS, Ph.D.

Communicated by Prof. W. L. Bragg, F.R.S., March 16, 1930.
Read and discussed, June 13, 1930.

ABSTRACT. The increase of resistance of a column of mercury in a magnetic field, found in experiments on this effect, is due to the internal motion of the liquid produced by the action of the Ampère forces between the magnetic field and the electric current traversing the mercury. The hydrodynamic significance of the results of such experiments is considered and the consistency of the results shows that e.m.fs. as small as 10^{-6} to 10^{-7} volts, induced by a magnetic field in moving mercury, can be accurately measured. This result shows the practicability of the method of investigating the flow of liquids proposed in the previous paper.

§ 1. INTRODUCTION

When a liquid conductor carrying an electric current is placed in a magnetic field forced convection currents are in general produced by the action of the Ampère forces between the field and the electric current. If, for instance, the liquid is contained in a straight glass tube in a transverse magnetic field, motion is produced in the regions where the field falls off at the edge MM', Fig. 1, of the pole pieces of the magnet. The energy required to move the liquid is supplied by the battery which drives the electric current, and a back e.m.f. is set up in essentially the same manner as in an electric motor. The back e.m.f. is induced by the action of the magnetic field on a moving liquid and it is proportional to the field strength and the velocity of the liquid. Information about the motion of the liquid may therefore be obtained by observations on this induced e.m.f., and such experiments may be regarded as a special application of the method of investigating the flow of liquids described in the preceding paper. In the experiments under consideration here not only may the velocity be found by electrical measurements but as the motion is itself produced by electric forces the "driving" forces also may be measured electrically.

The back e.m.f. gives rise to an apparent increase in the electrical resistance of the conductor caused by the magnetic field and the author showed some time ago that this effect accounts for the results of nearly all experiments on the magneto-resistance of liquid metals*. The purpose of the present paper is to consider the hydrodynamical significance of such experiments. The results obtained show the practicability of the measurement and interpretation of e.m.fs. induced in a moving liquid by a magnetic field and therefore of the method of investigating the flow of

* *Phil. Mag.* **50**, 27 (1925).

liquids described in the previous paper. The type of flow investigated is that of a liquid in an enclosed space produced by external volume forces. The stream lines in such a case are essentially curved, and it is interesting to consider the results of the present analysis in relation to observations on the flow of liquids through straight and curved pipes.

The nature of the flow of a liquid through a straight pipe is well known. For small velocities the motion is stream-lined and the resistance is proportional to the velocity. For higher velocities the motion is turbulent and the resistance varies approximately as the square of the velocity. These facts are shown graphically in Fig. 2 (*a*), where the values of (resistance/velocity) are plotted against the velocity. The transition from the first region to the second is marked by changes in the height as well as the slope of the curve. The recent experimental work of C. M. White*

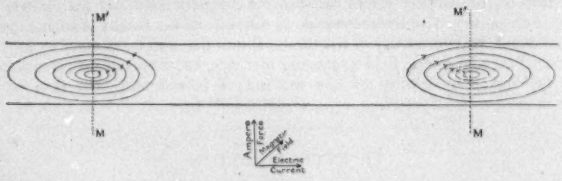

Fig. 1.

shows that for the case of flow through a curved pipe there exist three regions. His results are represented in Fig. 2 (*b*). For small velocities the motion is stream-lined and the resistance varies linearly with the velocity as in the case of straight pipes. This region is represented by the horizontal portion *AB* of the curve. As the velocity is increased above that corresponding to *B* the curve *gradually* leaves the straight line *ABB'*. The change is not marked by a discontinuity either in the height or slope of the curve and it is attributed to the gradual increase in the effect of centrifugal force which is essentially always present in a curved pipe. (C. P. Dean† has shown that this centrifugal force produces a stream-lined circulation in the plane of the cross-section of the tube.) Thus in the second region which begins at *B* the motion is still steady though the resistance is no longer proportional to the velocity. If the velocity is further increased a point *E* is reached where the stream-lined motion breaks down and turbulent motion sets in. This change corresponds to the transition which takes place in a straight tube but differs from it in that there is no discontinuity in the height of the curve. The interpretation of the three regions has been convincingly verified by G. I. Taylor by direct observation by means of coloured bands‡.

Since the motion of a liquid in an enclosed space, as in the case of a liquid conductor in a magnetic field, is essentially curvilinear, one would expect centrifugal force to produce the same general effect as in the case of flow through a curved pipe. The results of experiments on the apparent increase of electrical resistance of a

* *Proc. R.S.* **123**, 645 (1929).
† *Phil. Mag.* **5**, 673 (1928).
‡ *Proc. R.S.* **124**, 243 (1929).

liquid conductor due to a magnetic field show, however, that this is not the case. These experiments reveal only two regions as shown in Fig. 2 (*c*). As in all cases there is an initial region *AB* of small velocities for which the motion is stream-lined, the resistance being proportional to the velocity. In the second region *BG* the resistance varies approximately as (the velocity)$^{1\cdot75}$, and the motion is in all probability turbulent. The middle region for curved pipes, represented by *BE* in Fig. 3, is absent. We may suppose that turbulence sets in in these experiments before the effect of centrifugal forces becomes appreciable. However, in that case we should expect the transition to turbulence to resemble the transition observed in

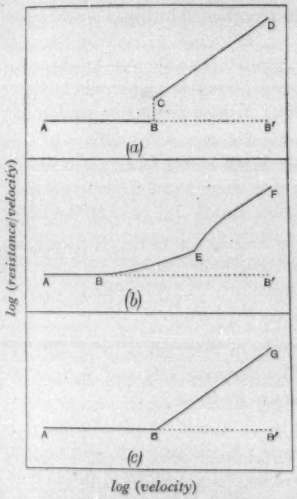

Fig. 2. Variation of resistance with velocity in (*a*) straight pipes, (*b*) curved pipes, (*c*) enclosed space.

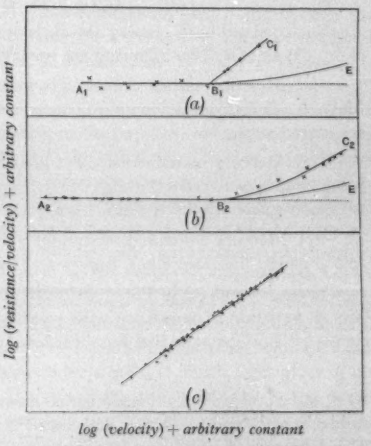

Fig. 3. Relation between resistance and velocity for mercury.

straight pipes and represented in Fig. 2 (*a*). This is not the case since the transition in the experiments concerned is not accompanied by a discontinuity in the height of the resistance/velocity curve, nor are there any velocities for which the motion is not stable.

§ 2. ANALYSIS OF THE EXPERIMENTS

In experiments on the effect of a magnetic field on the resistance of mercury the quantities measured are the dimensions of the liquid conductor, the strength H of the magnetic field, the electric current i traversing the conductor, and the increase δr in electrical resistance. In all the experiments that will be considered here the increase of the electrical resistance is wholly due to the motion of the mercury.

The Ampère forces which maintain the motion of the mercury are proportional to the product Hi of the magnetic field and the electric current. When the motion produced reaches an average steady state these driving forces are balanced by the
R hydrodynamic resistance R so that

$$R \propto Hi \qquad \text{.....(1)*.}$$

δe The induced back e.m.f. δe is proportional to the product of the magnetic field and
v the velocity v so that the latter is proportional to $\delta e/H$. If δr is the apparent increase in electrical resistance δe is equal to $i.\delta r$ so that

$$v \propto i\delta r/H \qquad \text{.....(2)*.}$$

The values of R/v and v which are the quantities represented in Fig. 2 are therefore proportional to $H^2/\delta r$ and $i\delta r/H$ respectively.

The values of $\log (H^2/\delta r)$ and $\log (i\delta r/H)$ calculated from the results of experiments carried out by T. Jones† and P. Jones‡ are represented graphically in Fig. 3 at (*a*), (*b*) and (*c*). The following are specifications of the experiments concerned:

(*a*) Mercury contained in straight glass tube of diameter d equal to 0·96 mm. Magnetic field from 4,800 to 10,000 gauss. Induced e.m.f. from $5{\cdot}8 \times 10^{-7}$ to $1{\cdot}1 \times 10^{-5}$ volts.

(*b*) Mercury contained in bent glass tube for which $d = 1{\cdot}1$ mm., the tube being bent so as to cross the edge of the magnetic field several times. H from 3,200 to 10,000 gauss. Induced e.m.f. from $3{\cdot}6 \times 10^{-7}$ to 4×10^{-5} volts.

(*c*) Mercury contained in straight glass tube, $d = 5{\cdot}0$ mm. H from 2,000 to 10,000 gauss. Induced e.m.f. from $1{\cdot}1 \times 10^{-5}$ to 5×10^{-4} volts.

Curves (*a*) and (*b*) in Fig. 3 show the existence of two regions with different laws of variation of resistance with velocity. The portions A_1B_1 and A_2B_2 of the curves are horizontal so that for these low velocities we have

$$\text{Resistance} \propto \text{velocity} \qquad \text{.....(3).}$$

At B_1 and B_2 respectively this law breaks down. In (*a*) the slope of the curve changes suddenly at B_1 and for higher velocities the resistance is proportional to $v^{1{\cdot}8(\pm 0{\cdot}1)}$. The departure from a linear law in the case of curved pipes is represented by the curve BE and it is seen to be much more gradual than that represented by BC. In the experiments represented at (*b*) the linear law breaks down at B_2, and though the change in slope is not as sudden as in the first case it is much more rapid than the change in the case of curved pipes. (The slope of the latter part of B_2C_2 corresponds to $R \propto v^{1{\cdot}6(\pm 0{\cdot}1)}$.) The less rapid change in slope in (*b*) is not unexpected. In the corresponding experiments the tube used was bent so that the column of mercury crossed the "edge" of the magnetic field several times. As was pointed out in § 1 with reference to Fig. 1, the motion produced by the Ampère forces takes place in

* Strictly speaking these expressions for R and v are valid only provided the stream lines of motion maintain a constant shape. This consideration affects very little the general nature of the results arrived at.

† T. Jones, *Phil. Mag.* **50**, 46 (1925).

‡ P. Jones and T. Jones, *Phil. Mag.* **2**, 176 (1926).

the region where the field falls off so that in these experiments the motion takes place at about ten different places. The conditions obtaining are not exactly the same for these different places and they may therefore have slightly different critical velocities. The transition for the resultant resistance/velocity curve is thus spread out. In the case of the experiments represented at (*a*) the motion is produced at two symmetrical points so that the abruptness of the transition is not affected.

In the experiments represented at (*c*), the resistance is nowhere proportional to the velocity. The points all lie within experimental error on a straight line, the slope of which corresponds to

$$\text{resistance} \propto \text{velocity}^{1\cdot75\pm0\cdot03} \qquad \text{.....(4).}$$

This law is within experimental error the same as that which corresponds to the slope of the parts of curves (*a*) and (*b*), after the transition point *B*. T. Jones carried out several experiments in addition to those represented at (*c*), and though the conditions in these experiments vary considerably, comprising different shapes of the boundary of the mercury and different distributions of field and current, the law of variation of resistance with velocity is in all cases in close agreement with equation (4). It therefore appears that there are only two régimes for the motion of a liquid in an enclosed space corresponding to the two laws of resistance given by equations (3) and (4). Rough calculations of the actual magnitude of the velocities in some of the experiments for which the resistance obeys equation (4) give values so high that it is inconceivable that the motion is stream-lined. (3) is only obeyed when the conditions are conducive to steady motion, i.e., when the velocity and diameter of the tube are small. The transition from the first régime, represented by the horizontal portions of curves (*a*) and (*b*), to the second régime, represented by the sloping portions of the curves, is therefore in all probability a transition from stream-line motion to turbulent motion, the stream-line motion being such that the effect of centrifugal forces is negligible. The transition takes place without a discontinuity in the resistance, in contrast with the transition which takes place in the case of flow through straight pipes. If this difference is attributed to the curvilinear nature of the motion we must suppose that this has considerable influence on the transition itself without appreciably affecting the stream-line motion for velocities immediately preceding the transition point. When the stream-lines are curved the centrifugal forces tend to make the resistance depend on a higher power of the velocity than the first, and it is difficult to explain the absence of the middle region *BE*, Fig. 2 (*b*), which exists in the case of flow through curved pipes.

The motion of mercury in an enclosed space of the type indicated in Fig. 1 is thus of a different character from that of motion through a straight tube or curved tube. In all cases there is an initial region of small velocities for which the resistance is proportional to the velocity. In the motion under consideration this régime is maintained until turbulence sets in, in contrast with the flow through curved pipes; and the transition to turbulence takes place without a discontinuity in the resistance/velocity curve in contrast with the flow through straight pipes.

§ 3. VARIATION OF THE RESISTANCE WITH THE VISCOSITY AND DENSITY OF THE MERCURY

In the case of steady motion the velocity produced by given driving forces is inversely proportional to the viscosity of the liquid and is independent of the density of the liquid. In turbulent motion, on the other hand, the density is a more important controlling factor than the viscosity. The exact relations may be found by applying the principle of dynamical similarity and it will be interesting to see if these agree with the observed dependence of the velocity on the viscosity and density. If we assume that the motion of the mercury depends only on the driving forces, the shape and size of the boundary of the liquid, its viscosity μ and its density ρ, then it follows from the above principle that the dimensionless quantities $(H^2/\delta r) \times (s/\mu)$ and $(i\delta r/H) \times (\rho/\mu)$ are functions of one another, where s denotes the linear dimensions of the system*. It can be shown from this result that in the region for which the resistance is proportional to the velocity, and in that for which it is proportional to the (velocity)$^{1\cdot75}$, the velocities produced by given driving forces are respectively proportional to $(\mu^{-1}\rho^{0})$ and $(\mu^{-1/7}\rho^{-3/7})$. T. Jones and P. Jones observed the values of δr at different temperatures, keeping the values of H and i constant. The corresponding changes in the velocity should therefore be given by the above expressions. The observed and calculated increases in the velocity due to increases in the temperature above room temperature are given in Tables 1 and 2. Table 1 refers to experiments carried out under conditions of steady motion, equation (3), and Table 2 to experiments carried out under conditions of turbulent motion, equation (4)†.

Table 1.

Temperature t (° C.)	$\mu_{17} \div \mu_t$	ρ_{17}/ρ_t	(Velocity at t°) ÷ (velocity at 17°)	
			observed	calculated
100	1·30	1·015	1·30	1·30
180	1·51	1·030	1·51	1·51

Table 2.

Temperature t (° C.)	$\mu_{17} \div \mu_t$	$\rho_{17} \div \rho_t$	(Velocity at t°) ÷ (velocity at 17°)	
			observed	calculated
100	1·30	1·015	1·055	1·045
280	1·58	1·048	1·119	1·090

* These results were used in a previous discussion, by the author, of the dependence of δr on h, i, etc., *Phil. Mag.* **50**, 27 (1925).

† In these experiments the amount of liquid used amounts to only a small fraction of a cc. and this circumstance makes it easy to carry out experiments at different temperatures.

It is seen that the observed variation of the velocity with the viscosity and density of the liquid agrees very satisfactorily with the calculated variation. This means that all the factors which affect the motion of the mercury were taken into account in the application of the principle of similitude, when it was assumed that no slip takes place between the mercury and the glass and that there is no surface property involved. The verification of the results therefore shows that in the motion of mercury over glass no slip takes place either in steady motion or turbulent motion.

§ 4. MAGNITUDE OF INDUCED E.M.F.

The magnitude of the induced e.m.fs. measured in the experiments which have been considered ranges from about 4×10^{-7} volts to 5×10^{-4} volts, and even for the smallest voltages the measurements give consistent results. This is especially shown by the horizontal portion A_2B_2 in Fig. 3 (*b*), which corresponds to a range of voltage from $3{\cdot}6 \times 10^{-7}$ to about 6×10^{-6} volts. The points all lie within 1 or 2 per cent. on a straight line, which represents stream-line motion at low velocities. The experiments therefore show that, in the case of mercury, contact potentials and other sources of spurious effects do not hinder the measurement of induced e.m.fs. of the order of 10^{-7} volts or more.

§ 5. ACKNOWLEDGMENT

In conclusion I should like to thank Professor W. L. Bragg, F.R.S., for his kind interest in this work.

DISCUSSION OF THE PRECEDING TWO PAPERS

Dr W. Jevons: The effect described in the author's first paper was observed in the course of some experiments at sea in which I was privileged to take part in 1918*. Two series of experiments were made. In one series, pairs of electrodes were moored at various distances apart in the entrance to the River Dart, and connected by insulated cables to a recording millivoltmeter in a shore observation hut. Continuous records extending over prolonged periods clearly showed the existence of periodic electromotive forces in the sea having a period identical with that of the tide, and amplitudes which varied from a maximum at the time of spring tides to a minimum at neap. In the other series of experiments, two electrodes 100 yards apart were towed in tandem at distances of 60 and 160 yards from a ship, and connected by insulated cables to a millivoltmeter aboard. The direction and magnitude of the tide were roughly estimated by observation at a buoy, and then a quadrilateral course was steered, with sides respectively down, across, up and across

* F. B. Young, H. Gerrard and W. Jevons, "On electrical disturbances due to tides and waves," *Phil. Mag.* **60**, 149 (1920).

the tide. The millivoltmeter readings were of the same order on the down and up courses, but greater and less respectively on the cross courses, the difference between these latter readings being almost invariably of the expected sign and magnitude (about 2 mv. per 100 yards per knot of tide velocity). In all but the smoothest seas the e.m.f./time graphs showed short-period variations, which were most marked when wind and tide were opposite to one another and at right angles to the line of the electrodes; these were clearly the result of wave action. The experimental details and results are fully discussed in a paper published in 1920*. [Some of the records obtained in the two series of observations were shown in lantern slides at the meeting.]

Dr E. G. Richardson: There are one or two points in these interesting papers on which I should like information. The electrodes are shown to give the integrated velocity gradient from the boundary to various points in the tube. This method, as in the ordinary capillary flow viscometer, requires one to assume a distribution of velocity across the tube and to test this against the observed flow. The hot wire possesses the advantage that it can be used to plot the velocity from point to point through the liquid. Is this possible with the present method?

The hot wire is liable to error when used in close proximity to the walls of the tube. Is there any correction required when the electrodes are close to the boundary?

The electromagnetic method certainly possesses the advantage of having no lag, and I suppose could be used to measure a fluctuating velocity with the aid of a string galvanometer. Has this been attempted? Dryden in Washington has used in connection with the hot wire an inductive circuit which compensates for the lag when rapidly fluctuating air currents are being measured.

Author's reply: I am very interested to hear of the experiments by Dr Jevons on the e.m.fs. produced in the sea by the action of the earth's field on tidal motion. They represent the application on a large scale of the principle underlying the present method of investigating the flow of liquids.

I am glad that Dr Richardson, who has taken a large part in developing the hot wire method, is present. In reply to his first question I would say that the velocity from point to point in a liquid can be determined directly by the present method provided the conditions are such that no induced currents are produced by the magnetic field. The limitation set when induced currents are produced is emphasized in my papers, and in such cases the distribution of velocity can be determined only indirectly—by calculating the distributions of potential corresponding to different distributions of velocity and comparing them with the observed distribution of potential. In the case of flow through a straight pipe this procedure is very satisfactory, and the velocity at a point can for all practical purposes be connected with the potential gradient at that point and hence directly determined. It might be pointed out that the arrangement of electrodes used in my experiments is only incidental. The potential difference between the fixed electrode at the wall and a

* F. B. Young, H. Gerrard and W. Jevons, "On electrical disturbances due to tides and waves," *Phil. Mag.* **60** 149 (1920).

given position of the movable electrode gives of course only the integrated velocity gradient, but by taking different positions of the latter the velocity at a point can be determined from the slope of the curve connecting $(V_A - V_P)$ and ϵ. This could be obtained more directly by employing two movable electrodes kept at a small constant distance apart, but for various experimental reasons this procedure was not adopted.

In the present method there is no reason to expect any special trouble in investigating the velocity close to the walls and no correction of the kind mentioned by Dr Richardson is necessary. In the present experiments, for instance, one electrode was kept flush with the walls but there was no indication of trouble due to this.

I have not used the method for investigating fluctuating flow. With the help of sufficient voltage amplification and a string galvanometer it could no doubt be used for that purpose.

A SIMPLE METHOD OF SHOWING THE MODES OF VIBRATION OF A WIRE

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ABSTRACT. A method is described of showing the modes of vibration of a wire by heating it with an alternating electric current and varying its tension, the various modes being easily observable owing to the luminosity of the wire.

The methods usually employed for showing the various ways in which a string or wire may be made to vibrate, such as Melde's experiment and the use of the monochord, are troublesome and tedious. An electrically driven tuning fork to one prong of which a thread is attached, provision being made for alteration of the tension by means of a tightening-peg, is satisfactory except when the thread is divided into only a few segments, in which case the applied tension is apt to overpower the fork. The method to be described is simpler to carry out than any of the foregoing and enables any number of vibrating segments between 20 and 1 to be obtained and readily seen.

In the method under notice regular impulses are imparted to a wire by means of an alternating electric current, which produces periodic changes in the length of the wire as its strength rises and falls. A thin wire, heated to a high temperature, is necessary in order that marked differences in the length of the wire may be produced during each cycle and a good amplitude obtained. A suitable wire is one made of an alloy of platinum and iridium, which may be obtained from dealers in various diameters of which the best for the experiment is 0·0014 in., or 0·0036 cm. This wire fuses with a current of 0·8 amp. at a temperature of about 1800° C. It has an average temperature-coefficient of 0·0009, and at the working current of 0·5 to 0·6 amp. has a resistance of 300–350 ohms per metre. A piece of this wire about 70 cm. long is fixed to terminals supported by two wooden stands or blocks and connected to a.c. mains through a rheostat of 400 ohms resistance capable of carrying 1 amp. This arrangement is suited to any supply voltage from 220 downwards. The rheostat is adjusted until the wire attains a white heat, when one of the stands is moved away gently until a slight tension is produced on the wire, which will then be seen to form into a large number of nodes and loops. The number of loops depends upon the temperature of the wire; with the procedure described it is usually about 18. The nodes, which are at rest, appear brighter than the loops, which are cooled by their movement through the air. If the stand be drawn slightly

farther away the number of loops may be reduced by one at a time until the wire breaks under the tension. To obtain the lesser number of segments the wire is shortened and the above procedure repeated. The fundamental form can be obtained on a length of 25 cm. with care, and readily on 15 cm. with an amplitude of 0·5 cm. or more, but there is a danger of the wire breaking if slightly overstretched. In all cases fine adjustment can be made by movement of the slider of the rheostat. The experiment may be carried out with greater certainty if the wire be mounted between two sliding pieces on a bench, one of the sliders being movable by hand and provided with a clamping device, whilst the other is moved by a screw so as to give a fine adjustment of the tension.

A nichrom wire of the same diameter gives quite good results, but cannot be made so hot as the iridio-platinum wire. At a safe working temperature the vibrating part of the nichrom wire is not so clearly seen as in the case of the wire recommended.

The method described has the advantages of simplicity in the apparatus and manipulation, and easy observation owing to the wire being self-luminous.

THE EFFECT OF PHOTOSENSITIZED MERCURY VAPOUR ON THE WALLS OF SILICA VACUUM TUBES

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ABSTRACT. An investigation is made into some phenomena occurring at the gas-solid interface in the well-known experiments in which mercury atoms absorb energy from the radiation λ 2537 and then transfer that energy to other gas molecules at collision. It is shown that a combination of both condensation and liberation of gas at the surface of the apparatus must be taken into account, if the effects of irradiation of a gas mixture are to be traced by any of the usual methods of observing pressure changes. In a severely outgassed tube of mercury and hydrogen, the initial pressure-fall due to adsorption of the products of dissociation reaches a limit when a monomolecular layer having a quarter of the surface density previously obtained by electrodeless discharges in glass tubes is completed. After the completion of this saturation, a liberation of gas from the silica begins. This liberation is shown to follow a law independent of the partial pressure of H_2 but dependent on that of Hg, and on the absorption of resonance radiation by the latter. This action on the solid surface is contrasted with the more usual experiments in which the final recipient of the energy of photosensitization is solely gaseous. Comparison may be made with other recent experiments in which atoms in high quantum states communicate their energy to solid surfaces, and also with the better-known phenomena in which gas is liberated from solids by the impact of multiply charged ions.

§ 1. TRANSFORMATIONS OF THE ENERGY OF ABSORBED RESONANCE RADIATION

In 1922 Franck and Cario[1] showed that when a mixture of Hg vapour and some foreign gas is irradiated with the Hg resonance line λ 2537, the energy absorbed by the Hg atoms may afterwards be transferred to the foreign gas by molecular encounters of the type called "collisions of the second kind." In terms of the quantum theory, an Hg atom is excited to the $2\,^3P_1$ level; if before the time for its spontaneous return to the original $1\,^1S$ level it happens to collide with a gas molecule, the energy difference between the levels is liberated and appears either as kinetic energy or as causing some disturbance, such as dissociation, of the gas molecule.

In the first experiments of Franck and Cario the foreign gas was hydrogen: the dissociation potential of this is about 4·4 volts, and the energy available in the excited Hg atom is, in the same units, about 4·9 volts. Accordingly the effect provided a convenient though indirect method of producing atomic hydrogen without the ionization which is unavoidable in an electric discharge. In contrast

to the very complex sequence of events in any discharge, we have here instead two stages: (i) the primary transformation of energy is the absorption of λ 2537, not by the H_2 which is transparent to it, but by the Hg which is extremely opaque to it; (ii) in a secondary transformation the activated or excited Hg, which we shall denote by Hg′, dissociates the H_2.

This type of process, said to be "photosensitized" by the presence of the Hg vapour, has since been extended by many physicists and chemists[2]; a large number of such reactions has been catalogued, though even in the simplest case of hydrogen it is doubtful whether the formation of an unstable hydride may not accompany the simple splitting of H_2 into atoms.

Now in many of these researches on mercury photosensitization, the progress and results of the energy transformations are inferred from the observed change in total gas pressure. Thus, various workers detected the formation of the atomic hydrogen in the irradiated mixture by the pressure-fall due to its well-known tendency to become adsorbed on the wall of the vessel; Franck and Cario first detected it by its reduction of metallic oxides, again measuring a pressure change. The interpretation of any such results of a photosensitized process introduces more than one complication, as seen for instance in the fact that Senftleben and Meyer both employed hot-wire gauges to trace the sequence of events, but each in an opposite way. The former observed the cooling of the wire due to the conductivity of H_1 formed in its vicinity, the latter observed the heating of the wire due to loss of pressure as the H_1 was adsorbed or entered into combination and was condensed. Meyer[3] pointed out that the consistency of either method would depend on the state of the walls, and that any heating of the wire by recombination of H_1 at its surface might also introduce a third factor in the measurement.

In view of these complications, and following an investigation I have made[4] on the laws governing loss of hydrogen pressure by adsorption, the work described in the present paper is designed to isolate and measure such of the pressure changes in a photosensitized mixture as can be shown to depend only on gas liberation and condensation at the surface of the silica vessel itself.

Accordingly it is shown here that the adsorption of products of the energy transformations exhibits a saturation maximum and a fatigue, in close agreement with that exhibited by the adsorption of hydrogen in electrodeless discharge tubes of glass. The surface density (number of particles per square centimetre) of the adsorbed layer on the silica is, however, only about a quarter of that on the glass; this fact suggests that a larger molecule than H_1 is included in the layer formed after photosensitization. When the saturation is completed, a reverse process is shown to set in, liberating far more gas than had been initially adsorbed. This process is not thermal, and it occurs at the same rate whatever the partial pressure of the hydrogen in the mixture, but it requires the presence of the Hg vapour and also its absorption of the resonance radiation. Further, the liberation ceases when a limit is set to the available supply of Hg vapour, which thus appears itself to be used up in attacking the silica vessel.

These experiments are mainly concerned with pressure changes on a small

scale, and are not likely to intrude as more than slight corrections into the usual photosensitization of gas mixtures at considerable pressures. In addition to such possible corrections, they serve to exhibit a new example of the transfer of energy of excitation of a neutral atom to a solid surface instead of to another gas molecule, the disruption of solid surfaces having previously been associated with the impact of charged ions.

§ 2. APPARATUS

A bulb of 135 cc., made of fused silica by the Thermal Syndicate, Ltd., is connected with the following apparatus: (*a*) a palladium valve for admission of pure H_2; (*b*) a vacuum system including pumps, drying materials, liquid air trap, and discharge tube; (*c*) a hot-wire pressure gauge of the Pirani type, with platinum filament connected to bridge and potentiometer, and suitably balanced and heat-insulated; (*d*) a mercury micromanometer of a modified optical-lever type, of which I have given details in the paper quoted[4]. This is capable of indicating pressure changes from 0·5 mm. to less than 10^{-3} mm. as rapidly as they occur.

The Pirani gauge is calibrated against the micromanometer for various potentials applied to its bridge, to give suitable sensitivity over various ranges between 10^{-4} mm. and 10^{-1} mm. total pressure. The micromanometer, in which a liquid Hg surface is necessarily exposed, will only measure the pressure of gas constituents additional to the Hg vapour. Accordingly, when the micromanometer is in the gas system the whole of the latter is filled with Hg vapour at the saturation pressure corresponding to room temperature. This pressure, of the order of 10^{-3} mm., is sufficient to enable the Franck and Cario process to be carried out if the measuring instruments are of a high sensitivity, though in many researches on the subject a reservoir of Hg has been warmed to provide a higher vapour pressure. In those parts of the present work where Hg vapour has to be excluded, the micromanometer has, of course, to be shut off, and the Pirani gauge alone to be used in conjunction with a liquid air trap. In calibration and check experiments when the two instruments are used together it is necessary to take into account the considerable time lag in the response of the hot wire to pressure variations.

The silica bulb can be baked in an electric furnace adapted to slide over it, and in certain cases it was made red-hot in a blow-pipe. For the principal experiments the bulb is exposed to a quartz mercury lamp, of whose spectrum the resonance line λ 2537 is the highest frequency that emerges with little absorption. In order that pressure-broadening of this line radiation may be avoided, the lamp is kept cool by means of a compressed-air blast. Heating of the reaction vessel itself is negligible when this precaution is taken.

§ 3. SATURATION MAXIMA OF THE ADSORPTION

Fig. 1 shows the fall of hydrogen pressure on irradiation of the SiO_2 tube of hydrogen and mercury with the cooled mercury arc. The pressure, being measured on the micromanometer, does not include the 10^{-3} mm. of Hg vapour which is also present. Before each determination the bulb was baked at about 400° C. for two

hours, the whole vacuum system having been tested to stand a cathode-ray vacuum for days. Pure dry H_2 was admitted, and the system was cut off and tested for the constant pressure shown in the initial horizontal portions of the graphs. Irradiation begins at the same point *O* on the time scale of each graph.

The curves may be compared with similar ones obtained with a glass tube and the same micromanometer, a high-frequency electrodeless ring discharge being used instead of the mercury photosensitization[4] as the source of activation of the hydrogen. Curves 1, 2 and 3 of Fig. 1 exhibit the character described in that previous investigation; namely, in each graph the initial rate of pressure-fall decreases until the curve becomes approximately parallel with the time axis, while in passing from one graph to the next the total amount of pressure-fall became less each time the experiment was repeated, although the tube was rebaked, rewashed with H_2, re-exhausted, and refilled before each repetition. Hence we have two fatigue phenomena, firstly the arrival at a steady state of pressure after each short period of excitation, and secondly the decrease in the difference between initial and final pressures each time the whole cycle of operations is carried out.

The first of these fatigue phenomena I refer to as saturation, implying that the surface has taken up as much gas as it can on that occasion, but not necessarily implying that there is no more space on the surface available for closer packing of an adsorbed layer. The numerical values on the graphs show, as in the previous work on glass, that this saturation is a property of the surface and not of shortage in further gas supply.

The capacity for maximum value of the pressure-fall is not easily regained after the second type of fatigue has reduced the adsorptive power to zero. It seems to be more completely restored by long atmospheric exposure than by heating *in vacuo* even to red heat. Thus, curve 4 was taken after the whole apparatus had been dismantled and rebuilt with the addition of the Pirani gauge chamber. An effect of the added volume of the latter is seen in the slower rate of pressure-fall. This curve was followed by another sequence of decreases in the saturation at successive adsorptions, as in the series of curves 1, 2, 3.

Langmuir was the first to interpret this loss of pressure which occurs in hydrogen when subjected to certain kinds of disturbance. He gave strong evidence for its being due to the dissociation into H_1, the atoms clinging to the walls of the apparatus and thus removing half their number of H_2 molecules from the gaseous phase. Langmuir's atomic hydrogen was derived from very hot tungsten filaments, and the adsorbable hydrogen of my previous paper[4] was derived from electron collisions in the ring discharge, in the total absence of metals. Although the ring discharge gives rise to other particles, charged and neutral, besides H_1, it was assumed there that the neutral atom is again the adsorbable product. On this assumption, and making use of the measured maximum pressure-fall and the dimensions of the apparatus, I have calculated the surface density of the adsorbed layer of monomolecular thickness in three glass tubes of very different sizes and shapes, obtaining respectively $5{\cdot}9 \times 10^{15}$, $3{\cdot}2 \times 10^{15}$ and $5{\cdot}3 \times 10^{15}$ atoms per cm.^{2}.

It has been assumed by all writers that the loss of pressure in a photosensitized

mixture of H_2 and Hg represents a similar adsorption of atoms, together with any pressure change due to reduction of oxides, etc. specially placed in the tube for observation. The structure of the layer formed has not hitherto been investigated. But in this determination of the density of the layer whose formation is inferred

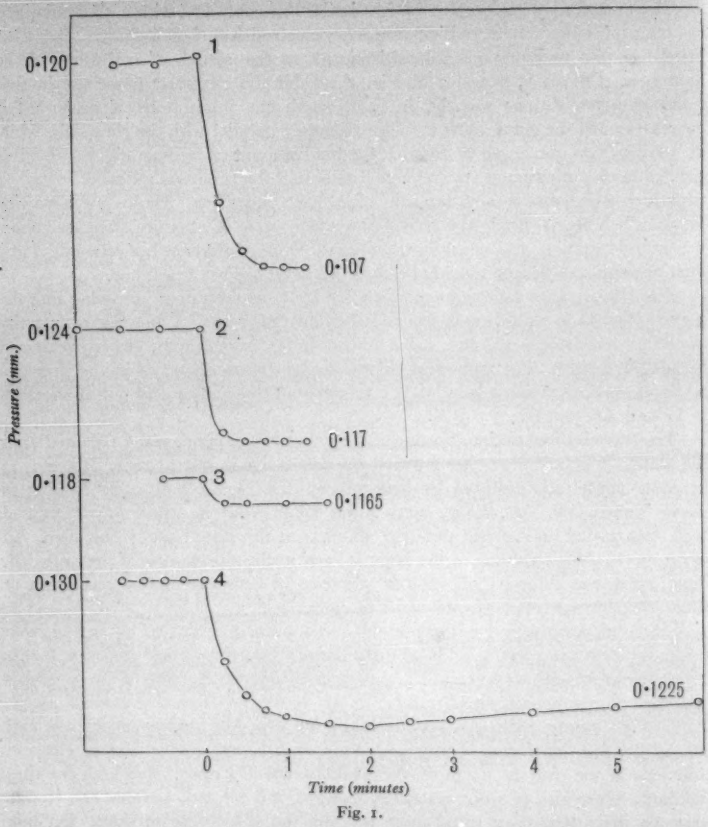

Fig. 1.

from Fig. 1, for comparison with the previous experiments, account must be taken of the ambiguity which is still not completely cleared up, as to the possible by-products of a photosensitized dissociation. Franck and Cario concluded that the principal reaction is

$$Hg' + H_2 \rightarrow Hg + H + H,$$

while not neglecting the further possibility that a diatomic molecule may be produced. On the basis of certain band spectra seen in mixtures of H_2 and Hg, Compton and Turner adopted the latter possibility, in the form

$$Hg' + H_2 \rightarrow HgH + H,$$

and it is not inconceivable, according to Mitchell and others, that H_2' plays some part in the process.

From their great instability it is highly probable that both HgH and H_1 would be strongly adsorbable in comparison with H_2, and either of the above two interpretations of the reaction would mean the disappearance from the gaseous phase of two particles for every H_2 molecule lost to the gas pressure. On this assumption I have calculated from the dimensions of the apparatus, and from curves 1 and 4 of Fig. 1, the density of the layer when saturated, obtaining respectively $1{\cdot}0 \times 10^{15}$ and $1{\cdot}2 \times 10^{15}$ particles per cm.^{2}. These figures are seen to be of the order of $\frac{1}{4}$ the surface density of the layer on glass in all the other experiments.

My experience shows that, to obtain the maximum possible saturation density, a surface must be rigorously outgassed to start with; hence, since the above values were not increased even by red-heating the silica, they suggest that the average adsorbed particle in the photosensitized experiments is of considerably larger diameter than the particle adsorbed in the discharge tube. As to the latter, Langmuir's hypothesis of a moderately closely packed monomolecular layer of atoms was well confirmed, since our maximum density, $5{\cdot}9 \times 10^{15}$ per cm.^{2}, is related to the density to which Bohr's H_1 orbits could theoretically be packed, $8{\cdot}9 \times 10^{15}$ per cm.^{2}, by just the degree of difference to be expected between a "structural" diameter and a "collision" diameter, or distance of nearest practicable approach of two non-combining atoms. If the present case of adsorption is also an example of the Langmuir structure, the collision diameter of any particles which share the monomolecular layer with these H_1 atoms must be of the order of at least 3 A.U. to give the density I have found. The two possible alternatives are H_2' and HgH. But since H_2' was present in the discharge-tube experiments, and since nevertheless the adsorbed layer there showed a packing so close as to be attributable to H_1 only, it is more probable that H_2' is not so strongly adsorbable, and that Compton's HgH, a necessarily large molecule, is here responsible for the lesser saturation maxima observed. For in the photosensitized experiments H_1 can only be formed at the expense of Hg', and hence the concentration of any activation products involving Hg is here comparable with that of H_1, whereas in the discharge tube such products could only contribute according to the initial partial pressure of Hg, which is small compared with that of H_2. The suggestion that some particle which includes an Hg atom can be adsorbed is also required by the measurements we proceed to describe, dealing with the dependence of the gas exchange at the surface on the partial pressure of Hg.

§ 4. THE LIBERATION OF GAS FROM THE SOLID SURFACE

In the previous experiments on adsorption in the electrodeless discharge the saturation was stable, i.e. the pressure graph having once become parallel with the time axis remained so, whether the discharge was maintained or not. But in the present photosensitized experiments a slight rise of pressure was observed to set in if the illumination was maintained. This is seen incipient in curve 4 of Fig. 1, and becomes more definite after the surface has been fatigued by successive adsorptions, Figs. 2 and 3.

Freedom from leak being guaranteed to a high degree of perfection, a rise in pressure can only be ascribed to one or more of the following causes: (i) heating by the warmth of the lamp; (ii) any gas reaction involving a dissociation (this would show dependence on the initial partial pressure of H_2); (iii) diffusion through the walls due to some effect of irradiation on the SiO_2 (this would be independent of the partial pressure of Hg); (iv) surface reactions: desorption of layer on the silica, decomposition of the silica, or liberation of gas from the interstices of the silica.

I base my conclusions on (iv), for the following reasons. Heating was eliminated as a cause by check experiments. (*a*) The bulb was replaced by a comparison tube of similar dimensions containing a thermometer. This was found to rise less than 2° in ten minutes of irradiation. (*b*) The bulb was heated in a flame until too hot to be touched, with only $\frac{1}{10}$ of the result seen in the pressure increases of Fig. 2, whereas during those experiments the air blast had kept the bulb quite cold to the touch. (*c*) Any tendency of the micromanometer to function as a thermometer would result in an effect proportional to the initial pressure, whereas the pressure-increases are found to be independent of this quantity. (*d*) The null effect in the absence of Hg, recorded below, supplies the upper limit of any thermal consequences.

To test whether any gas reaction of the very highly unstable H_1, for instance a dissociation of tap-grease vapour, could be responsible, the rise in pressure on irradiation was obtained at varying initial partial pressures of H_2. Fig. 2 shows that the effect of intermittent switching on and off of the radiation produces a pressure-rise which is approximately independent of the H_2 and consequently cannot represent a gas reaction. It even takes place when the H_2 is initially pumped out to below 10^{-3} mm. (curve 5), and in that instance the tube is filled to over 0·01 mm. as a result. That is to say, ten times the original content is liberated on irradiation. In this last curve, the impossibility of complete outgassing of the Hg micromanometer is shown in the slight steady increase of pressure which takes place at the highest evacuation, but superposed on this is a liberation of gas coinciding with irradiation as at the higher pressures. It may be remarked that curves 1 and 3 were the first and second, respectively, to be obtained, and exhibit the disappearing remnant of the initial adsorption, which becomes lost in the completed stage of fatigue.

The gas liberation being, then, independent of the partial pressure of H_2, its dependence on the partial pressure of Hg is investigated in the following manner, which also adds plausibility to the previous suggestion that Hg, in some form, is removed from the gas phase at the first irradiation.

So long as the micromanometer is in connection, Hg vapour is present to its saturation pressure of 10^{-3} mm., and any of it which may be removed is at once

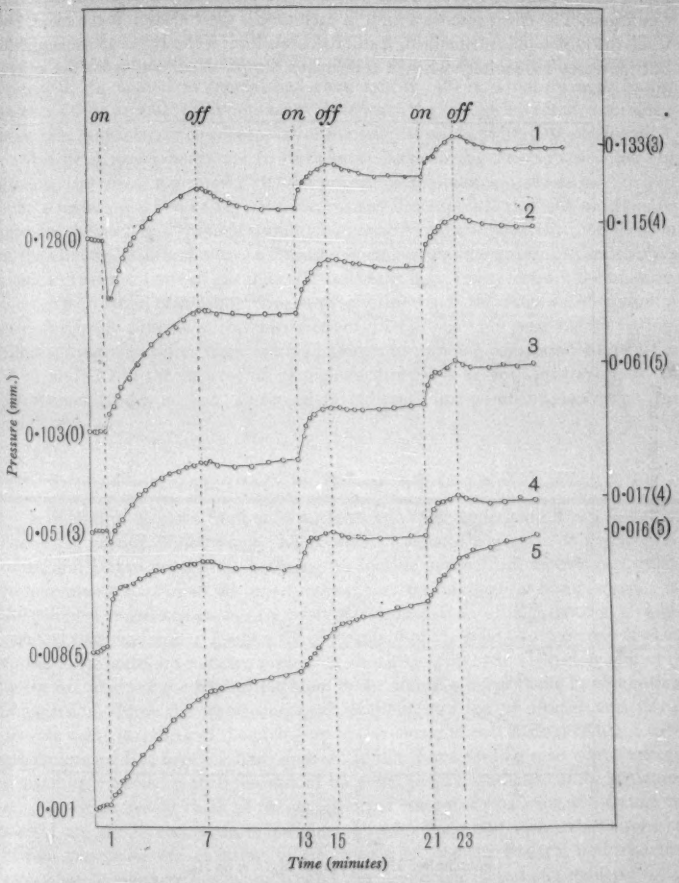

Fig. 2.

replenished from the liquid surface. Accordingly the Pirani gauge was constructed and added at this stage, and comparison was made between the liberation of gas under irradiation, (*a*) with the micromanometer tap open, i.e. with an unlimited

availability of fresh Hg vapour, and (*b*) with the tap closed, i.e. with an initial partial pressure of Hg vapour of 10^{-8} mm., but no further supply to be drawn upon. Fig. 3 shows pairs of curves of the pressure-rise on illumination under these two conditions. The effect reaches a limit in each case if the radiation is removed, but, when the radiation is maintained, it still reaches a limit if the Hg is not replenished, but increases indefinitely when a continuous supply is drawn from the micromanometer.

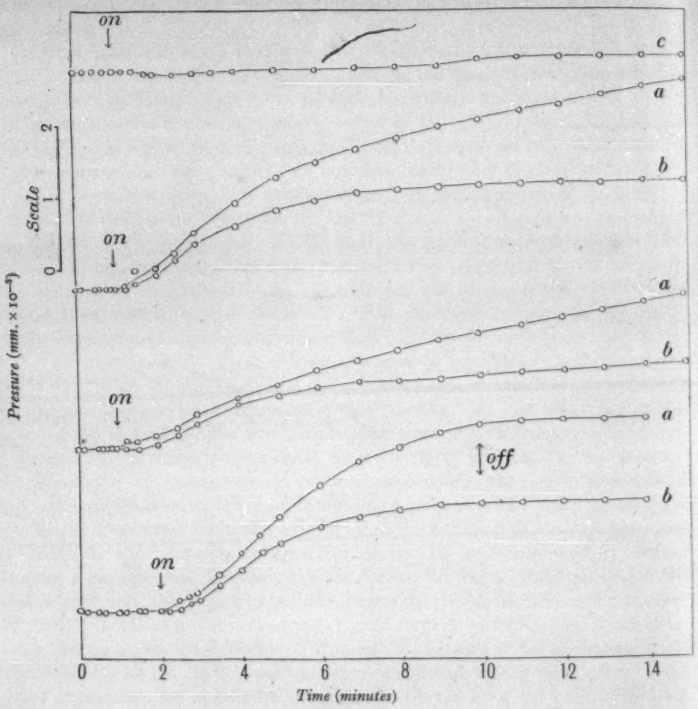

Fig. 3.

a, unlimited Hg. *b*, limited Hg. *c*, no Hg.

To carry this observation to its extreme, the micromanometer was kept shut off and Hg vapour was as far as possible removed from the whole system by six successive washings with H_2 over a liquid air trap. Repetition of the experiment then gave curve (*c*), showing a complete disappearance of the pressure-rise on

irradiation, and exhibiting only the slight changes which represent the sum of thermal and other corrections and observational error.

Hence we conclude the conditions governing the liberation of gas to be the following: (1) it occurs only when the bulb is irradiated; (2) it is not a thermal effect; (3) it is independent of the partial pressure of H_2; (4) it decreases and finally ceases when a limited quantity of Hg vapour is available; (5) it increases indefinitely if Hg vapour is replenished continuously; (6) it is non-existent in the absence of Hg vapour; (7) it only takes place when the surface has reached complete saturation with respect to adsorption of the original products of photosensitization.

It now becomes necessary to distinguish between the alternatives under the heading (iv) of p. 496 above, i.e. the possible interpretations which may be put upon a surface reaction of Hg′. We dismiss any decomposition of SiO_2 owing to its extreme chemical and thermal stability. In considering the possibility of liberation of the already adsorbed layer, we must remember that the pressure-rise involves a much greater volume of gas than had originally been removed from the mixture, and shows no sign of ceasing. Hence we conclude that Hg′ causes a liberation of gases originally present in the interstices of the SiO_2 and not removed by baking. Silica is known to differ from ordinary glasses in trapping considerable quantities of air and CO in the process of its manufacture: it is to this that the opacity of the coarser grades is due. I proceed to indicate some points of interest in this conclusion.

§ 5. DISCUSSION OF RESULTS

A liberation of gases from solid surfaces and their underlying layers *in vacuo*, due to Hg vapour, is not a new phenomenon; it is known to occur in positive ray tubes, and I have previously discussed, in another paper[5], the mechanism which is there involved. But in all those cases it was the mechanical bombardment of the solid—a metal electrode—by multiply charged ions of Hg moving at high velocity, that was able to liberate the gas content of the solid. In the present case, on the other hand, no ions are present at all, and the liberation is consequent only on the excitation of the neutral Hg atom to the higher quantum state by absorption of radiation. Now the photosensitized reactions initiated by Franck and Cario provide many instances of the giving up of the energy of that state in collision with gas molecules, but here we have apparently the corresponding case where energy is given up in collision with the wall of the tube, and a disturbance of the structure results. Other recent cases of the giving up of energy of excitation of an atom to a solid are the experiments of Oliphant[6], who finds that metastable atoms of helium can cause the emission of electrons from a metal surface, and also the somewhat similar conclusions of Webb[7], of Messenger[8], and of Coulliette[9].

The most obscure part of the present experiments is the way in which the process of liberation depends upon saturation's having been reached in the adsorption of the primary products: there seems, from the graphs, to be something more than a masking of the former by the latter. § 3 of the present paper, in

comparison with the previous paper[4] on the adsorption of H_1, makes it appear probable that some at least of the constituents of the monomolecular layer are the unstable HgH molecules of Compton, and the disappearance of Hg vapour supports this view. Hg′ impinging on this adsorbed layer evidently causes not merely desorption of gas to an amount comparable with that already deposited, but also a mechanical disturbance of the underlying structure such as to allow an indefinitely continued liberation of the imprisoned gases which silica is known to contain. In noticing that the energy exchange between Hg′ and the solid is able to effect what the flame of a blow-pipe cannot, we must remember that the energy available in the Hg′ (112,000 calories per gram-molecule) is very great if it can only be localized when transferred. Since this energy is able to dissociate H_2, whose heat of dissociation is 10^5 cal., it should be equivalent to more than the heat of an oxyhydrogen flame, when it is applied to aggregates of not much more than molecular dimensions in, and immediately below, the vitreous surface.

§ 6. ACKNOWLEDGMENTS

I am very grateful to Prof. S. W. J. Smith, F.R.S., for generous provision of research facilities, and to Mr G. O. Harrison for skill in instrument construction.

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DISCUSSION

Mr B. S. Gossling (communicated): As regards the preliminary outgassing of the silica vessel it is generally understood that the temperature at which the most copious evolution of gas occurs from vitreous substances is higher the more refractory the substance. For the softer gases 400° C. is ample, and for the harder 500° C., but for silica something like 1000° C. would be necessary. This supports the possibility which Mr Johnson infers that the evolved gas comes from the silica itself.

Author's reply: I am grateful for Mr Gossling's hint, which will be valuable to me. While it makes more plausible the gain of gas from the solid, it also makes more remarkable the fact that this appears to be accomplished by mere contact of Hg′, to an extent far greater than even by the severe outgassing which Mr Gossling shows to be necessary. Actually I made the silica glow in more than one of the attempts to increase the saturation maxima, with no appreciable further liberation of gas.

A SENSITIVE ROTATING-COIL MAGNETOMETER

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ABSTRACT. This paper describes a sensitive rotating-coil magnetometer in which the flux due to the earth's field or to a magnet is neutralized by that due to a current passing through a fixed concentric compensating coil which forms with the rotating coil a variable mutual inductance. It shows how the first-order correcting term due to the length of the magnet may be made to vanish if the angle of contact be suitably chosen, and that the second-order correction may be eliminated by a correct choice of the dimensions of the coil. Many experiments bearing on the theory and uses of the magnetometer are described. In particular, the ohm may be measured very simply, in terms of a mutual inductance and a period, by Weber's method of damping.

§ 1. INTRODUCTION

It is well known that in the absolute determination of the ohm by the British Association method (suggested by Weber and put forward independently by Lord Kelvin) a small correction has to be made for the mutual energy between the magnet and the rotating coil, the correction involving the moment of the magnet and the galvanometer-constant of the rotating coil.

It is here shown that such a rotating coil, forming a variable mutual inductance with an outer fixed coil and fitted with suitable commutating contacts joined to a low resistance galvanometer, may be used as a magnetometer for the rapid and accurate comparison of magnetic moments of small magnets, for their absolute measurement, and for the determination of hysteresis curves, of the magnetic inclination, and of local variations of the earth's horizontal field-component. Furthermore, it is pointed out that the magnetometer is specially suitable for the complete elimination of the quantities which give trouble when the ohm is determined absolutely by Weber's method of damping, and it is suggested that the method will ultimately be available for the study of susceptibilities of feebly magnetic substances.

The present preliminary investigation has been carried out with a small lecture-demonstration rotating coil not specially designed for our purpose. The results obtained, however, have been so encouraging and the sensibility so good that they have led us to look more closely into the theory of the method and to deal with the problem of the length of the magnet.

§ 2. PRINCIPLE OF THE METHOD

m If a small magnet of moment m has its centre coincident with that of a rotating
G, θ coil of galvanometer-constant G, and its axis inclined at an angle θ to the plane of
F the coil, the flux F through the coil due to the magnet is given, to a first order, by

$$F = mG \sin \theta \qquad \text{.....(1).}$$

Thus, if contact is made with a galvanometer connected to strips which are touched by the terminals of the rotating coil over an angle θ on each side of the position of zero flux, a total flux change of $4F$ takes place in the galvanometer circuit during every complete revolution.

This flux may be neutralized by that due to a larger concentric compensating
C, M coil carrying a current C and possessing a mutual inductance M with the rotating coil at the instant of make and break. If the plane of the fixed compensating coil, always normal to the magnet, is adjusted to lie in the magnetic meridian, balance may be tested by rotation of the movable coil, the sensibility being adequate even at low speeds. When the current C is so adjusted that, on rotation, no current traverses the galvanometer, we have for equilibrium

$$mG \sin \theta = CM \qquad \text{.....(2),}$$

and thus $$m = KC,$$

K where the constant $$K = M/G \sin \theta \qquad \text{.....(3),}$$

and is known as the moment-constant. It may be determined by the following methods, all of which have been used in this research:

q, γ (*a*) A small search-coil of total area q and carrying a current γ may be substituted for the magnet and its flux through the rotating coil may be neutralized by a current
C' C' in the compensating coil, balance being tested by rotation: whence

$$K = q\gamma/C' \qquad \text{.....(4).}$$

(*b*) Such a search coil may be substituted for the magnet, and its mean mutual
M' inductance M' with the rotating coil at the contact edges may be compared with M, the corresponding mean mutual inductance between the rotating coil and the compensating coil: whence

$$K = M/G \sin \theta = qM/M' \qquad \text{.....(5).}$$

If a standard variable mutual inductometer is available this method is excellent, for the ratio M/M' changes slowly with θ and moreover q may be determined with a standard solenoid.

(*c*) G may be found directly and M measured absolutely, this method being best for large contacts.

Three different sizes of contacts are fitted to the apparatus. With the largest, extending over some 87°, the mutual inductance M is accurately

$$M_{\max} \sin \theta, \text{ and } K = M_{\max}/G \qquad \text{.....(6).}$$

For the smallest, used in the absolute determination of the ohm, $\theta = 9\frac{1}{2}°$. Over the range 0–40°,

$$M = \mu\theta,$$

where μ is a constant and $K = (\mu/G)\,(\theta/\sin\theta)$ .....(7). μ

The usual contact, however, for the measurement of magnetic moments is given by $\theta = 50°\,46'$ and is chosen for the following reason. If the length $2l$ of the magnet is appreciable in comparison with $2a$, the diameter of the rotating coil, the expression for the flux, viz. l a

$$F = mG\sin\theta,$$

is subject to correction by terms involving l^2/a^2 and higher even powers, the correction varying with the inclination θ. It is shown below that if $\sin^2\theta = \frac{3}{5}$, this correction vanishes to a first order and commences with a term not greater than

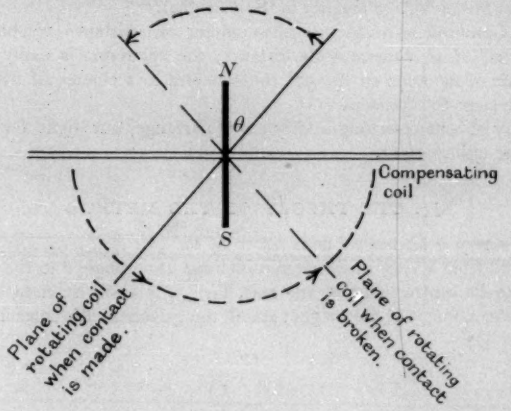

Fig. 1.

$\frac{1}{5}\,l^4/a^4$ which may also be made to vanish if the coil is suitably constructed, the correction being reduced to $\frac{1}{40}\,l^6/a^6$. With the apparatus used in this research

$$F = mG\sin\theta\,(1 - \tfrac{1}{7}\,l^4/a^4),$$

where $a = 10$ cm. when $\sin^2\theta = \frac{3}{5}$. The great advantage of using this angle of make and break, for which the sine law holds best, is strikingly brought out by the curves in Fig. 6 below, for we show that the magnet then lies on a line of isomutual inductance—a line along which the axis of a search-coil may be moved without changing its mutual inductance with the main coil. The very long and very short contacts are, nevertheless, useful, not so much in setting a lower and a higher limit to the moment of the magnet as in enabling the effective length of the magnet to be estimated and the correcting constants to be determined.

If the apparatus fitted with the largest contacts is rotated through 90°, so that

the plane of the compensating coil is now at right angles to the meridian and the contact brushes lie on the middle point of their contacts when the rotating coil is in the meridian, the effect of the earth's horizontal field-component H may be balanced by a current C_H in the compensating coil. We have then for equilibrium

$$AH \sin\theta = C_H M = C_H M_{\max} \sin\theta \qquad \text{.....(8),}$$

where A is the total effective area of the rotating coil.

The vertical component of the earth's field may likewise easily be received and balanced, and hence the angle of dip may be determined with sensibility, from the ratio of the two balancing currents.

Moments of magnets may also be measured easily with the apparatus in the position for receiving H, either by allowance for the small current needed for field neutralization, or by the taking of the mean of the two balancing currents for the one and the other lie of the magnet in the meridian.

With a Gambrell moving-coil galvanometer of resistance 10 ohms and sensibility 20 cm./μA at a metre scale distance, the apparatus is easily sensitive to 0·2 c.g.s. unit of moment on the 51° contacts and to a change of field of 0·0003 gauss on the large 87° contacts.

Symmetry of contact-setting is very easy to arrange, but slight deviations from symmetry are unimportant.

§ 3. THE THEORY OF THE METHOD

Flux of a very small magnet with centre on the axis of a coil. The flux passing through a coil from a very small magnet inclined at an angle θ to the plane of the coil and with its centre lying on the axis, Fig. 3, is easily seen to be $mG \sin\theta$, where m is the moment of the magnet and G the galvanometer-constant of the coil

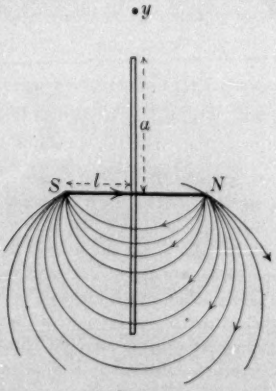

Fig. 2.

at the centre of the magnet, for if a current C traverses the coil, the potential energy of the magnet is numerically $mCG \sin\theta$ and this is equal to the current multiplied by the flux. C

Since fluxes and galvanometer-constants are additive this is equally true for a coil having a number of turns in parallel planes.

Flux of a magnet of length 2l lying centrally on the axis of a coil. Of the lines of force leaving the N pole, Fig. 2, some thread through the coil in the negative direction and some escape the coil; all return through the magnet. The resultant flux is thus equal to the escape flux. l

Since the field at a point on the axis of the magnet at a distance y from the centre is equal to $m(y^2 + l^2)^{-\frac{1}{2}}$ and is normal to the plane of the coil, y

$$\text{Total escape flux} = \int_a^\infty m(y^2 + l^2)^{-\frac{1}{2}} 2\pi y\, dy \qquad \text{.....(9).}$$

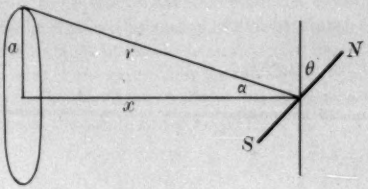

Fig. 3.

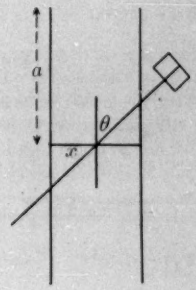

Fig. 4.

Thus
$$F = 2\pi m(a^2 + l^2)^{-\frac{1}{2}} = mG(1 + l^2/a^2)^{-\frac{1}{2}} \qquad \text{.....(10)}$$
$$= mG(1 - \tfrac{1}{2} l^2/a^2) \text{ approx.},$$

the true correcting factor lying between the limits

$$(1 - \tfrac{1}{2} l^2/a^2) \text{ and } (1 - \tfrac{1}{2} l^2/a^2 + \tfrac{1}{2} l^4/a^4).$$

This method of treatment may easily be applied if the magnet is shifted a distance x along the axis, and complications due to multiple layers only affect the correcting term.

Flux of an inclined magnet of appreciable length 2l ($l < a$) with centre on the axis of a coil. Consider a magnet of length $2l$ with its centre on the axis of a circular coil of radius a and at a distance x from the coil centre. Let σ be the pole strength of the magnet. a, x, σ

Let Ω_N, Ω_S be the magnetic potentials at the positions occupied by the north and south poles N and S respectively when a current C passes round the coil in the direction making a positive flux. Ω_N, Ω_S

Then the potential energy of the magnet is $\sigma(\Omega_N - \Omega_S)$, and the flux through
F the coil from the magnet is F, where

$$F = -(\Omega_N - \Omega_S)\,\sigma/C \qquad \text{.....(11).}$$

Now the potential at N is given by

$$\Omega_N = 2\pi C\left[1 - \cos\alpha + \sin^2\alpha \sum_{n=1}^{\infty} \frac{1}{n}\frac{dP_n(\cos\alpha)}{d(\cos\alpha)} P_n(-\sin\theta)\left(\frac{l}{r}\right)^n\right] \quad (12),$$

r where $r = (a^2 + x^2)^{\frac{1}{2}}$;

α $\sin\alpha = a/r$;

P_n P_n is the Legendre function of first kind of order n;

n and n is a positive integer*.

Since a similar expression holds for Ω_S in which $-l$ replaces l, we have for the flux through the coil due to the magnet

$$F = 4\pi\sigma\sin^2\alpha \sum_{n=1}^{\infty} \frac{1}{n}\frac{dP_n(\cos\alpha)}{d(\cos\alpha)} P_n(\sin\theta)\left(\frac{l}{r}\right)^n \qquad \text{.....(13),}$$

where n is a positive odd integer, whence putting n successively equal to 1, 3, 5, 7
m and writing $2l\sigma = m$, we have for the required flux

$$F = m \,.\, 2\pi\frac{a^2}{r^3}\sin\theta\left[1 + \left(1 - \frac{5}{3}\sin^2\theta\right)(a^2 - 4x^2)\frac{3}{4}\frac{l^2}{r^4} + \left(1 - \frac{14}{3}\sin^2\theta + \frac{21}{5}\sin^4\theta\right)(a^4 - 12a^2x^2 + 8x^4)\frac{45}{64}\frac{l^4}{r^8} + \left(1 - 9\sin^2\theta + \frac{99}{5}\sin^4\theta - \frac{429}{35}\sin^6\theta\right) \times (5a^6 - 120a^4x^2 + 240a^2x^4 - 64x^6)\frac{35}{256}\frac{l^6}{r^{12}} + \ldots\right] \qquad \text{.....(14),}$$

where $2\pi a^2/r^3 = G$, the galvanometer-constant at the centre of the magnet.

We first obtained this expression without the use of spherical harmonics by
Ω finding values of Ω_N, Ω_S by Taylor's theorem from Ω, the potential at the centre of the magnet, but the expansion is more laborious when that procedure is adopted.

Elimination of the effect of the length of the magnet. A rotating coil with provision for an axle and supports for the magnet and calibrating search-coil must necessarily be wound in two sections, and the Helmholtz-Gaugain pattern at first suggests itself. Our fundamental equation (1),

$$F = mG\sin\theta,$$

is essentially additive for different layers of winding in parallel planes, and we measure experimentally the resultant flux and the resultant value of $G\sin\theta$ at the centre of the magnet. The problem before us is thus somewhat different from the problem of simplifying the expression for the couple on a needle where, in galvanometry, a calculation from coil dimensions is required. This contrast is very

* Cf. Andrew Gray, *Absolute Measurements in Electricity and Magnetism*, p. 210 (1921).

noticeable in connection with the effect of multiple layers, which in the galvanometer problem have a more fundamental effect, but in this case only give rise to small correcting terms.

The first correcting term in l^2 changes sign with θ and vanishes when $\sin^2\theta = \frac{3}{8}$. Since this vanishing is independent of the value of x, the effect of multiple layers is thrown down to the second-order correcting term. In practice the selection of 50° 46′ as the value of θ means only the cutting of a copper contact arc of twice this angle, an operation which not only can be done with accuracy but can be verified, when the contact is *in situ*, by the aid of a variable mutual inductometer.

The second-order correcting term in l^4, which would vanish if θ were 32° 35′ or 64° 59′, has the value $-\frac{5\cdot1}{100}(a^4 - 12a^2x^2 + 8x^4)\,l^4/r^8$ when $\sin^2\theta = \frac{3}{8}$. This vanishes when $x^2 = a^2(3 \pm \sqrt{7})/4$, the useful solution being $x = 0{\cdot}2976a$. This value of roughly $\frac{3}{10}$ for the ratio x/a is ample for the axle and support table, and the loss of sensibility due to axial displacement is only 12 per cent. as against a loss of 28·4 per cent. in the Helmholtz arrangement, while there is a marked gain in compactness when rotation is considered.

The elimination of the second-order correcting term by the choice $x/a = 0{\cdot}298$ is, unlike the elimination of the first correcting term by the choice $\theta = 50°\,46'$, subject to correction for multiplicity of layers. This complication, however, may easily be removed by means of a design based on a principle due to Maxwell. If a coil of rectangular cross-section has axial breadth $2b$ and radial depth $2d$ and if P_0 is any term in the expression for the action of any kind between the magnet and the central circular filament of the coil, then to a first order the average term for the action of the whole coil is given by

b, d, P₀

$$\overline{P} = P_0 + \frac{b^2}{6}\frac{\partial^2 P_0}{\partial x^2} + \frac{d^2}{6}\frac{\partial^2 P_0}{\partial a^2} \qquad \text{.....(15).}$$

If
$$P_0 = \frac{a^2}{r^3}\left(\frac{a^4 - 12a^2x^2 + 8x^4}{r^8}\right) \qquad \text{.....(16),}$$

$$\partial^2 P_0/\partial x^2 = -7a^2 r^{-15}(5a^6 - 120a^4x^2 + 240a^2x^4 - 64x^6) \quad \text{.....(17),}$$

$$\partial^2 P_0/\partial a^2 = r^{-15}(30a^8 - 755a^6x^2 + 1650a^4x^4 - 552a^2x^6 + 16x^8) \quad \text{...(18).}$$

Thus if
$$\frac{b^2}{d^2} = -\frac{\partial^2 P_0}{\partial a^2}\Big/\frac{\partial^2 P_0}{\partial x^2} \qquad \text{.....(19),}$$

and P_0 vanishes owing to an arrangement that $x^2 = 0{\cdot}08856a^2$ for the central filament, the entire effect of the multiple layers will be zero for the term we are considering. On substituting for x we find that $b/d = 0{\cdot}957$.

Whence if the ratio of the axial breadth to the radial depth of the coil channel be chosen as 0·957, and $x/a = 0{\cdot}298$ for the central winding, the second-order correcting term will disappear. Moreover, slight deviations from this prescription will not have a serious effect. It is interesting to note that our ratio for b/d is different from that, viz. $b/d = 0{\cdot}733$, which we find would be required to make all the turns have the same galvanometer-constant at $x = 0{\cdot}298a$ as if they were coincident with the centre turn.

Having made the second-order correction vanish we may alter the contact angle θ, whence our flux-correcting factor will be very approximately

k $$[1 + k(l^2/a^2)(1 - \tfrac{5}{8}\sin^2\theta)] \quad \text{.....(20),}$$

where the constant k can be found experimentally with the aid of a solenoid, a search-coil and mutual inductometer and should differ little from 0·409 if the coil is built as suggested. Knowing k and using the other angular contact pieces on the apparatus we may estimate the effective length of a magnet if this is at all appreciable.

If both the first-order and the second-order correcting terms are made to vanish by the selections $\sin^2\theta = \frac{3}{8}$, $b/d = 0{\cdot}957$ and $x/a = 0{\cdot}298$, the value of the flux F will be theoretically

$$mG\sin\theta\,(1 - 0{\cdot}025\,l^6/a^6) \quad \text{.....(21),}$$

while the third-order correction is normally less than the error due to imperfect construction.

Significance of the line of lie of the magnet as a line of iso-mutual inductance. It is easy to show, either from expressions for axial and radial components of field or from the potential in the neighbourhood of a current-bearing circle, that the com-

N ponent N of field along a line inclined at an angle θ to the plane of the circle and passing through the centre is given at a distance l away from the centre by

$$N = -\frac{d\Omega}{dl} = C\frac{2\pi}{a}\sin\theta\left[1 + \frac{9}{4}\frac{l^2}{a^2}\left(1 - \frac{5}{3}\sin^2\theta\right) + \frac{225}{64}\frac{l^4}{a^4}\left(1 - \frac{14}{3}\sin^2\theta + \frac{21}{5}\sin^4\theta\right) + \ldots\right] \quad \text{.....(22).}$$

Hence if $\sin^2\theta = \frac{3}{8}$ a small search-coil with its axis on the line will have a constant mutual inductance with the circle until it is moved so far that l^4/a^4 is no longer negligible compared with unity.

If the line inclined at an angle θ to the plane of the circle does not pass through the centre but strikes the axis at a distance x therefrom, additional odd powers of l/r, which vanish when $x = 0$, appear in the corresponding expression for N, and we have no longer a line of equal values of N when $\sin^2\theta = \frac{3}{8}$. If, however, the line considered is symmetrically situated between twin coils, the odd-power terms will be neutralized and we have for the field in this direction

$$N = 2\pi t\frac{a^2}{r^3}C\sin\theta\left[1 + \left(1 - \frac{5}{3}\sin^2\theta\right)(a^2 - 4x^2)\frac{9}{4}\frac{l^2}{r^4} + \left(1 - \frac{14}{3}\sin^2\theta + \frac{21}{5}\sin^4\theta\right)(a^4 - 12a^2x^2 + 8x^4)\frac{225}{64}\frac{l^4}{r^8} + \ldots\right] \quad \text{...(23),}$$

t where t is the total number of turns wound on the twin coil.

If then the coil be designed as above with $b/d = 0{\cdot}957$ and $x/a = 0{\cdot}298$ we shall, when $\sin^2\theta = \frac{3}{8}$, have a line along which $N = CG\sin\theta$, giving a uniform mutual inductance of $qG\sin\theta$ with a small search-coil of area q. The same simple expression will serve fundamentally for the mutual inductance between the coil and a thin

solenoid lying along this line and having its centre at the origin, but the correcting terms will have the smaller coefficients of the expression (14) for the flux of a magnet.

The curves of mutual inductance shown in Fig. 6 below, and obtained with a travelling search coil, illustrate the force of this theory, though our rotating coil has only $x/a = 0{\cdot}15$. Such tests performed on a designed coil may well afford a practical estimate of its value, for the method is sensitive to deviations, the first-order term in the correction of the length of the magnet being increased threefold for a search coil, and the second-order term fivefold. By simple area measurements the percentage error for any length of magnet for any contact angle might be established.

The determination of the ohm absolutely in terms of a mutual inductance and a period. The apparatus is specially suitable for the determination of the ohm by the method of damping, in that it enables the quantities m^2G^2/I to be eliminated, and also the effect of the length of the magnet, which does not appear to have been considered previously. In addition, the small effect of self-inductance may yet further be reduced by virtue of this elimination of moment of inertia.

When a magnet is making small oscillations at the centre of a coil such as that just described and its position of rest lies in the plane of the coil, we may write for the flux

$$F = mG \sin\theta\,(1 + A_1) = Q \sin\theta \qquad \text{.....(24),}$$

A_1, Q

where the correcting factor A_1 is a constant for small angles. If self-inductance, which has a very small effect, is considered to a first order, the equation of motion for small oscillations is

$$(I - LQ^2/R^2)\,\ddot{\theta} + (p + Q^2/R)\,\dot{\theta} + \mu\theta = 0 \qquad \text{.....(25),}$$

p, I L, R μ c

where p is the air damping coefficient, I the moment of inertia of the oscillating system, L the self-inductance of the coil, R the total resistance of the coil and closing connections, and μ the restoring couple per unit angular displacement, μ being equivalent to $(mH + c)$, where c is the very small torsion effect. This equation leads without approximation to

$$\frac{Q^2}{R} = 4I\left(\frac{\lambda}{T} - \frac{\lambda_0}{T_0}\right) - \frac{4\lambda}{T}\cdot\frac{LQ^2}{R^2} \qquad \text{.....(26),}$$

λ_0, T_0 λ, T

where λ_0, T_0 are the logarithmic decrement and period respectively on open circuit, and λ, T the corresponding quantities when the coil is closed through the total resistance R. Hence since the effect of λ is very small,

$$R = m^2G^2\,(1 + A_1)^2/4I\,(\lambda/T - \lambda_0/T_0) + 4\lambda L/T \qquad \text{.....(27).}$$

The elimination of the undesirable quantities which have subjected the method to criticism is effected thus: First we have the open-circuit equation of oscillations

$$\mu/4I = (\pi^2 + \lambda_0^2)/T_0^2 \qquad \text{.....(28).}$$

Again, using the smallest contacts so that the length-correcting factor is still

C essentially $(1 + A_1)$, we have, on balancing the flux of the magnet by a current C in the compensating coil,

$$mG \sin\theta\,(1 + A_1) = CM \qquad \text{.....(29),}$$

M where M is the mean mutual inductance between the rotating and the compensating coils at the contact edges.

Lastly, using the rotating coil as a sine galvanometer so that the length correction is still the same, setting it in turn on the contact edges, and finding (by means of mirrors on the magnet and a Gauss eyepiece with object lens) the currents of mean $\bar{c}$ value $\bar{c}$ which restore the magnet to the plane of the rotating coil, we have

$$\bar{c}mG\,(1 + A_1) = \mu \sin\theta \qquad \text{.....(30),}$$

whence, all three equations (28), (29), (30) being multiplied together,

$$\frac{m^2G^2\,(1 + A_1)^2}{4I} = \frac{\pi^2 + \lambda_0^2}{T_0^2} \cdot M \cdot \frac{C}{\bar{c}} \qquad \text{.....(31),}$$

so that

$$R = M\frac{C}{\bar{c}} \cdot \frac{\pi^2 + \lambda_0^2}{T_0^2(\lambda/T - \lambda_0/T_0)} + \frac{4\lambda L}{T} \qquad \text{.....(32),}$$

or with an accuracy of 1 part in 1000,

$$R = M\frac{C}{\bar{c}}\,\frac{\pi^2}{T_0(\lambda - \lambda_0)} \qquad \text{.....(33).}$$

The current ratio $C/\bar{c}$, as taken on a Cambridge thermo-electric potentiometer, depends essentially on the ratio of two resistances.

It will be noticed that the minute effect of self-inductance is virtually to reduce the moment of inertia to a value

$$I' = I - L\,(1 + A_1)^2\, m^2G^2/R^2 \qquad \text{.....(34).}$$

Since I' is eliminated, we may write

$$R = m^2G^2\,(1 + A_1)^2/4I'\,(\lambda/T - \lambda_0/T_0) + 4\lambda_0\,L/T_0 \qquad \text{.....(35),}$$

and hence, using the alternative expression

$$\mu/4I' = (\pi^2 + \lambda^2)/T^2$$

for equation (28) and again multiplying, we have

$$R = M\frac{C}{\bar{c}} \cdot \frac{\pi^2 + \lambda^2}{T^2(\lambda/T - \lambda_0/T_0)} + 4\,\frac{\lambda_0}{T_0}\,L \qquad \text{.....(36),}$$

which, though reducing the effect of self-inductance, introduces a period more difficult to measure.

It is here pointed out that, though by using the small contacts we eliminate the effect of the length of the magnet almost completely, this would not necessarily be the best procedure if the method were pushed to its limit of accuracy.

The sensibility in relation to the dimensions and winding of the coil. Taking the deflection of a galvanometer as proportional to the current and to the square root R_g, C of the galvanometer resistance R_g, we have for the mean current C when the flux cut is due to a magnet,

$$C = 4mG \sin\theta\, .n_r/(R_g + R_c) \qquad \text{.....(37),}$$

where n_r is the number of revolutions per second and R_e the resistance of the coil. If a total length l_1 of wire of radius y fills the coil channel of volume V and mean radius a and if ρ is the specific resistance of the material of the wire and g the galvanometer-constant per unit length, then, insulation being neglected, n_r, R_e l_1, y, V a, ρ, g

$$V = l_1 y^2, \quad R_e = \rho l_1^2/V, \quad G = g l_1,$$

whence for the resulting deflection D we have D

$$D = k g l_1 R_g^{\frac{1}{2}}/(R_g + \rho l_1^2/V) \qquad \text{.....(38)},$$

where k is constant for a given speed of revolution. k

This deflection is easily seen to be a maximum when $R_g = R_e$, and when this condition is fulfilled we have

$$D \propto g V^{\frac{1}{2}} \qquad \text{.....(39)},$$

so that the gauge of wire is of consequence only as regards the choice of a galvanometer of resistance equal to the coil, and essentially it is the weight of wire that is important. Though $g = 1/a^2$, the reduction of sensibility on increase of a will not be great, for on theoretical grounds we have for the breadth and depth of the channel $2b = k_1 a$, $2d = k_2 a$ and the volume of the channel will be proportional to the cube of the radius. We may thus take the moment-sensibility to be inversely proportional to the square root of the coil-radius, if the coil is constructed according to the best theoretical conditions.

It is easy to show that if such a coil is exposed to H, then

$$D \propto a V^{\frac{1}{2}} \qquad \text{.....(40)},$$

and is again independent of the gauge of wire. The field-sensibility is thus greatly increased with the radius, being proportional to $a^{\frac{5}{2}}$.

§ 4. THE APPARATUS

The general form of the main apparatus will be gathered from Fig. 5. The rotating coil A, which had been made solely to illustrate the B.A. determination of the ohm, is of mean diameter 20 cm. and of resistance 2·8 ohms. It is wound with a total of 289 turns of enamel-covered wire of s.w.g. 18 (diameter 1·2 mm.), filling two channels each of radial depth 1 cm. and of axial breadth 2 cm., the central circular filament of each section being about 1·5 cm. from the axis of rotation FY. The coil rotates about two spindles EF and XY which are fixed to the framework of the instrument and is provided at its base with a pulley which enables it to be belt-driven from a hand wheel.

The fixed circular concentric compensating coil B is nearly 40 cm. in diameter and of narrow channel; it is wound in two portions with double silk-covered wire of s.w.g. 22 (diameter 0·71 mm.). The inner winding consists of 17 turns, having a calculated galvanometer-constant of 5·50 cm.$^{-1}$; it enables the galvanometer-constant of the rotating coil to be found experimentally by the method of field

neutralization. The outer winding consists of 38 additional turns. Normally all 55 turns are used in the compensation, the maximum mutual inductance with the rotating coil being 1814 microhenries.

Within the rotating coil and on the spindle XY can be fixed either a table D, graduated in degrees, capable of rotation when necessary, and adapted to support centrally any magnet or solenoid under test; or an ebonite carrier holding any of a series of search-coils C (not shown), of known dimensions, arranged with their central planes coincident with that of the compensator B. Leads from such search-coils pass outwards to terminals through the hollow spindle EF.

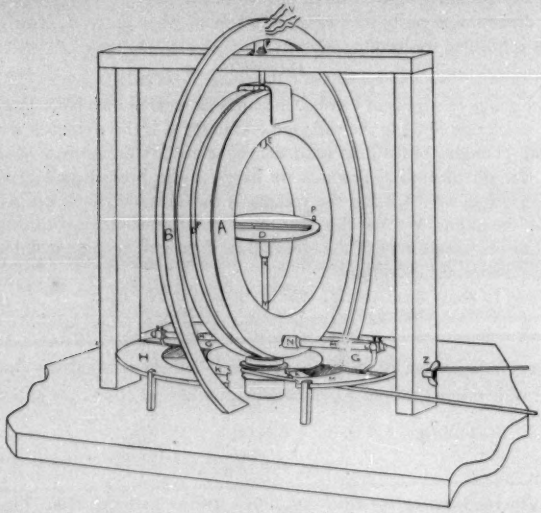

Fig. 5.

The ends of the windings of the rotating coil are brought to special pillar terminals NL projecting outwards as shown and carrying adjustable spring brushes of manganin or phosphorbronze. These brushes touch on copper sectors G and H or K which are fixed on a circular horizontal ebonite table calibrated in degrees and capable of being rotated when necessary and then fixed in position by means of the clamping screws Z, Z. The wide sector G subtends nearly 180° and almost covers half the table, while the narrower outer and inner sectors H and K subtend respectively 175° and 101° 30′ and fix these angles of contact. It will be seen that when the brushes are in the position L they make contact alternately with G and H, while in the position M they make contact with G and K. To ensure smoothness of running the gaps between the sectors are filled with ebonite of the same thickness as the copper.

The sector G and the sectors H and K are normally connected through a tapping-key to a low resistance Gambrell galvanometer provided also with a shorting-key and shunts. Alternatively by means of a mercury rockover switch the rotating coil may be thrown into series with the primary of a standard variable mutual inductometer and a valve oscillator. Likewise the compensating coil B or the search coil *in situ* may be joined to suitable battery circuits or connected so as to be in series in either direction with the secondary of the inductometer and a telephone.

Symmetry of contact is attained as follows. First the rotating coil is turned so that the mutual inductance between it and the compensator is zero—a very sharp setting. The sector turntable is next rotated so that the centre line of the contact H or K, as the case may be, is under one of the brushes. The coil is then rotated through 180° until the mutual inductance is again zero, when the other brush is adjusted to lie on the same centre line. The settings are checked as follows: the four mutual inductances are measured in the positions of the coming on and going off of each brush, so that it is possible to see whether their magnitudes are sufficiently close. Any further necessary improvement is then effected by means of a slight rotation of the sector table.

The mutual inductometer is also used to set a search-coil C coplanar with the compensator B. The rotating coil and compensator are set for zero inductance. The search-coil C is then turned until its mutual inductance with the rotating coil is zero also.

A third sector, not shown in the diagram, and subtending an angle of 19°, occasionally replaced the sector K, as in the tests on the determination of the ohm. For this purpose the magnet, fitted at its ends with plane mirrors, was supported in a stirrup hung at the coil centre on a fibre attached to a torsion rod passing through the hollow spindle EF. A Gauss eyepiece attached to a tube passing through the rotating coil at P enabled the arrangement to be used as a sine galvanometer.

When the plane of the compensating coil is in the meridian the resultant effect of the earth's field is zero if the contacts have been set symmetrically as described above. When this position has been found by means of rotation tests, the framework of the instrument is securely fixed to the bench. Though no resultant deflection ensues there are nevertheless, especially with large contacts, alternating jerks of the galvanometer spot, particularly at low speeds. These jerks are of little consequence except when weak magnets are being dealt with, but in any case they are easily suppressed by the use of a large square frame (not shown in the diagram) wound with some 40 turns of insulated wire of s.w.g. 28 (diameter 0·38 mm.), set symmetrically over the compensator at right angles to the meridian, and carrying a small current sufficient to neutralize the earth's flux approximately. When, on the other hand, as in the experiments on the ohm, the plane of the compensator is set at right angles to the meridian, there are no such jerks and a small current through the compensator neutralizes the earth's flux.

All currents used either in the compensator or in the search-coil circuits can

be switched through a series of calibrated standard resistances which range in value from 0·01 to 10·0 ohms and can be connected at will by potential leads to a Cambridge thermo-electric potentiometer reading from 0 to 90 millivolts. Such currents can thus be measured or compared with accuracy and auxiliary ammeters can at any time be tested.

The areas of search-coils in all later work have been found with the aid of the mutual inductometer and a standard solenoid 1 m. long.

§ 5. EXPERIMENTAL TESTS

Determination of G. The calculated value of the galvanometer-constant G' of the 17 turns of the large compensating coil is 5·50 cm.$^{-1}$. With the planes of both the compensator and rotating coil in the meridian, currents are sent through both in opposite directions so as to produce no deflection of a very small magnet fitted with a mirror and hung by means of a fibre at the common coil-centre. Correcting for the length of the magnet we find

$$G/G' = 31{\cdot}60,$$

whence $G = 173{\cdot}8$ cm.$^{-1}$.

t, l / d_1, d_2 Alternatively various search-coils of t turns, axial length l, and inner and outer diameters of winding d_1 and d_2, were set at the centre of the rotating coil and the maximum mutual inductances were compared with that between the search-coil and a standard solenoid of field-constant 310·9 gauss per c.g.s. unit of current. Table 1 shows the results obtained.

Table 1. Determination of G with different search-coils.

Search-coil	t	l	d_1	d_2	Mutual inductance with standard solenoid	Mutual inductance with rotating coil	G
S	1400	1·5	1·0	2·6	1233·5	690·0	173·9
T	1700	1·8	1·0	2·7	1483·0	828·5	173·7
U	1100	0·8	1·0	2·24	712·2	398·2	173·8
V	1513	$\frac{l}{d} = \frac{\sqrt{3}}{2}$			498·0	278·0	173·6

Determination of H/G. The plane of the compensator coil is set at right angles to the meridian and the period T of a small oscillating needle at its centre is taken with various steady currents C passing through the 17 turns of the compensator in such a direction as to oppose H. A plot of C as ordinate against T^{-2} as abscissa yields a straight line whose intercept on the y axis gives H/G' with precision. When the needle is reversed the torsion head is twisted in the same direction through 180°. Incidentally the current C' which reverses the needle and gives the same period as that given by H alone is easily found, and G' for the compensator is 5·50 cm.$^{-1}$; so that, since the torsion head has been rotated, $H/G' = C'/2$. Since $C' = 0{\cdot}07084$ c.g.s. we have $H/G' = 0{\cdot}03542$, $H = 0{\cdot}1948$ gauss and $H/G = 0{\cdot}001121$.

Determination of A, the area of the rotating coil, a, the mean radius and t, the number of effective turns. When the coil was set with the plane of the compensator at right angles to the meridian, a balancing current C of 0·00985 c.g.s. was needed through the full 55 turns which were of maximum mutual inductance M with the rotating coil. Whence, since $AH = CM$ and $M = 1814\mu H$ (or alternatively without an inductometer $H/G = 0{\cdot}001121$, and $M/G = 10435$ as explained below), $A = 91720$ cm.^{2}, so that for 289 turns $a = 10{\cdot}05$ cm. *A, a, t*

If, however, we use the equations $A = \pi a^2 t$, $G = 2\pi t\,(a^2/r^3)$ we have $r^3 = 2A/G$, so that $r = 10{\cdot}18$, whence, since $x = 1{\cdot}5$, we have $a = 10{\cdot}07$ and hence $t = 287{\cdot}8$.

Thus the first flux correcting term $(1 - \frac{3}{8}\sin^2\theta)\,(a^2 - 4x^2)\,\frac{3}{4}l^2/r^4$ is for our experimental coil $(1 - \frac{3}{8}\sin^2\theta) \times 0{\cdot}00648l^2$.

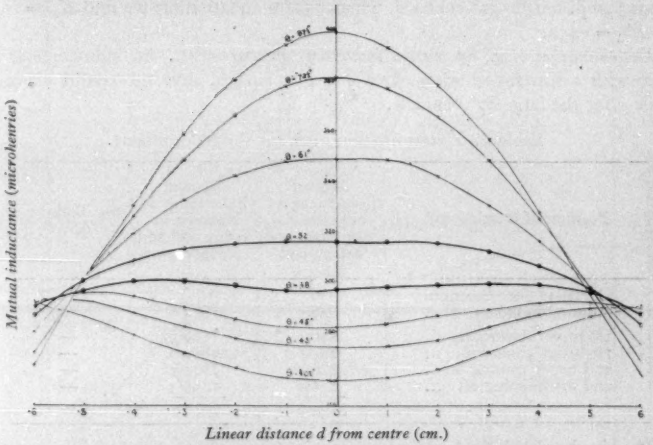

Fig. 6. Mutual inductance between main coil and search coil.

Experimental illustration of the advantage accruing from the use of the contact angle 50° 46′. The search-coil used was wound on an ebonite former, 1 cm. in diameter, drilled with a hole so that it could be made to slide along a straight glass tube. The coil consisted of 1100 turns of double-silk-covered copper wire of s.w.g. 38 (diameter 0·15 mm.) and the diameter of the coil rose to a maximum of 2·24 cm., its axial length being 0·8 cm. and its total area as found by the standard solenoid 2290 cm.^{2}. The glass tube carrying the search-coil had a linear scale inside it and was fixed horizontally so as to pass through the centre of the rotating coil. The mutual inductance between the search-coil and the rotating coil was then measured at various axial distances from the coil centre ranging from -6 to $+6$ cm. The rotating coil was then turned and fixed with its plane inclined at some other angle θ to the glass tube, an angle easily determined since, when the search-coil is at the centre, its mutual inductance with the rotating coil obeys a simple sine law. The mutual inductance is again measured at various distances along the guiding tube.

It suffices to show the data graphically in Fig. 6 which very clearly supports the general theory of the method and in particular equation (23). When it is realized that with the standard magnet of actual length 7·65 cm. and moment 1385 c.g.s. units, the error with 87½° contacts is less than 4 per cent., it will be seen how insignificant the length-error must be when the 51° contacts are used, and the reason why solenoidal bobbins yield good values of the moment constant K for this particular angle will readily be understood.

Determination of K, the moment constant, by current-ratio method. The current-ratio method is illustrated with a search-coil whose area $q = 4770$ cm.^{2} while it carries a current of 0·2165 amp. This was balanced, as tested by rotation of the coil, with the current C' equal to 0·09845 amp. on the 87½° contacts and equal to 0·10925 amp. on the 51° contacts, whence from equation (4) we find $K_{87} = 10490$ and $K_{51} = 9450$.

Determination of K by mutual inductance measurements. An inductometer test made with a search-coil whose area $q = 4770$ sq. cm. gave the results shown in Table 2 for the large 87° contacts.

Table 2. Determination of K for the 87° contacts.

Position of rotating coil	Mutual inductance M' between rotating coil and search-coil	Mutual inductance M between rotating coil and compensator	Ratio M/M'
Coplanar (for maximum mutual inductance) ...	828·5	1814·0	$2{\cdot}189_5$
1st brush coming on ...	826·4	1809·5	—
1st brush going off ...	825·5	1810·0	—
2nd brush coming on ...	826·5	1809·5	—
2nd brush going off ...	826·5	1809·5	—
	3304·9	7238·5	$2{\cdot}190_2$

The sine law holds for both inductances in this neighbourhood and the constant

$$K_{87} = (M/G \sin\theta)(q/q) = qM_{\max}/M'_{\max} = 10440.$$

The data of a typical experiment with the same search-coil determining K for the 51° contacts is given in Table 3.

Table 3. Determination of K for the 51° contacts.

Position of rotating coil	Mutual inductance M' between rotating coil and search-coil	Mutual inductance M between rotating coil and compensator
1st brush going off	645	1278
2nd brush coming on	642	1269
2nd brush going off	644·5	1277
1st brush coming on	642·5	1268

Hence $K_{51} = qM/M' = 9435$.

As a result of experiments with four different search-coils we find $K_{87} = 10435$, $K_{51} = 9430$.

Effect of using solenoidal search-coils in the finding of K_{51}. That the search-coils whose particulars are given in Table 1 might have been considerably larger for the determination of K_{51} is shown by the following extreme cases. For a solenoidal bobbin of length 6·1 cm. $K_{51} = 9480$ and for a solenoid of length 10 cm. $K_{51} = 9530$.

Variation of moment-constant K over the whole possible range of contacts. The search-coil whose area $q = 4770$ cm.^{2} was mounted so as to be coplanar with the compensator and the mutual inductances M and M' were measured at many

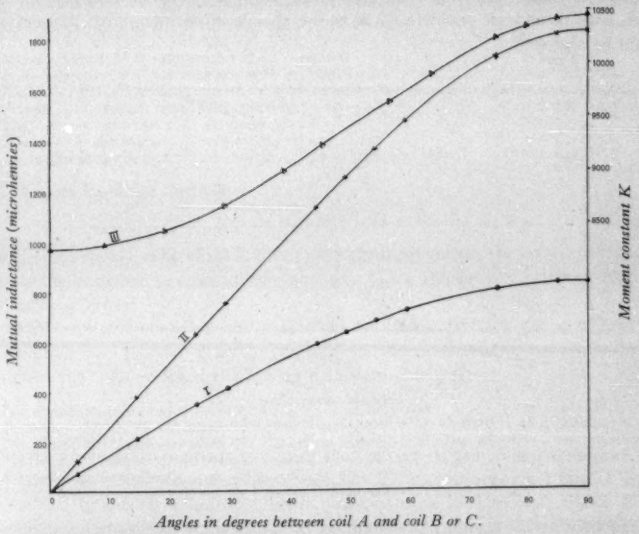

Fig. 7. I, Mutual inductance between coils A and C. II, Mutual inductance between coils A and B. III, Moment-constant K.

angles over a complete revolution of the rotating coil. Fig. 7 shows a plot of the average experimental values over a quadrant. Curve I for the rotating coil and the search-coil is a sine curve within the limits of experimental error. Curve II for the rotating coil and the compensator is practically straight up to 45°, the mutual inductance increasing at the rate of about 25·1 μH per degree. Curve III is the ratio of the ordinates M/M' multiplied by the area 4770 cm.^{2} so as to show the actual variation of K, the moment-constant, over all possible angles of contact. It will be seen that K changes from the lower limit 8290 for $\theta = 0$ to the higher limit 10440 for $\theta = 90°$. As the rate of change is always slow it matters little in the determination of K for any actual contact if the brushes are not exactly set at the coming-on and going-off positions.

Determination of the moment of a magnet. A flat cobaltcrom steel magnet of actual length 7·65 cm. was found by ordinary magnetometer methods to have a moment of 1370 ± 10 c.g.s. units.

On the 87° contacts it required a balancing current of 1·280 amp. yielding a moment of 10435 × 0·1280 = 1336 c.g.s. units uncorrected for length.

On the 51° contacts of constant $K = 9430$ it required a balancing current of 1·469 amp. yielding $m = 1385$ c.g.s. units. These data yield for the approximate effective length of the magnet $2l = 5{\cdot}7$ cm.

Variation of the moment of a magnet with small changes of temperature. Small contacts having θ equal to $9\frac{1}{2}°$ had been prepared for the determination of the ohm. Even with these least sensitive contacts the change of moment with temperature could be observed.

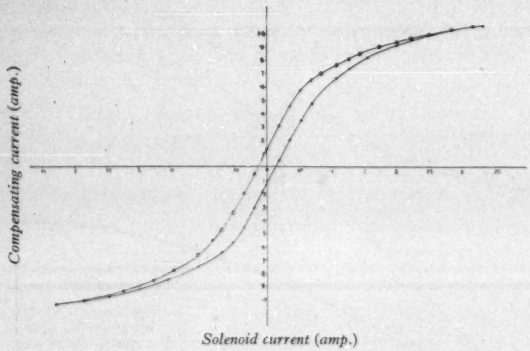

Fig. 8. Hysteresis curve for nickel obtained with apparatus described.

An ordinary steel magnet 7·9 cm. long gave a balancing current of 1·0728 amp. at 18° C. and 1·0518 amp. at 50° C., yielding a temperature coefficient of − 0·00061.

A cobalt steel magnet about 7·2 amp. long required a balancing current of 1·7814 amp. at 18° C. and 1·7718 amp. at 38° C., giving a much smaller temperature coefficient of − 0·00027.

Hysteresis shown by nickel. A solenoid of 2992 total turns of s.w.g. 28 insulated copper wire (diameter 0·38 mm.), spread over a length of 10·1 cm., was mounted on the magnet table shown in Fig. 5, normal to the plane of the compensator. The solenoid current passed also, but in the contrary direction, through the 38 turns of the compensator which was so shunted that on rotation of the coil no deflection of the galvanometer spot took place even when the maximum current used in the experiment was flowing. A few pieces of nickel wire of s.w.g. 18 (diameter 1·2 mm.) and of length 7·5 cm. were placed in a glass tube at the centre of the solenoid. These were subjected to various magnetizing fields and the resulting magnetic moments were neutralized by current flowing through the 17 turns of the compensator. A full cycle involving 70 points of observation was worked through and

the hysteresis curve shown in Fig. 8 was obtained. In view of the length of the wire and shortness of the solenoid there is little point in calculating values of I and H with the present apparatus. Excellent curves were also obtained for pianoforte steel wire, but the field available was not sufficient for saturation.

Determination of the magnetic inclination. The neutralizing currents for the earth's horizontal and vertical field-components were 0·0985 amp. and 0·1853 amp. respectively, whence $\tan\theta = 1853/985$ and the angle of dip was given as 62° 0′.

The laboratory dip circle at the same place gave a mean value of 61° 33′.

Absolute determination of resistance with large standard magnet. An experiment was first made with the standard magnet of moment 1385 c.g.s. units and actual length 7·65 cm. and the following data were obtained:

Mean value of M at extremities of $9\frac{1}{2}°$ contacts	$= 244{\cdot}5\ \mu$H
Balancing current C for magnet, with $9\frac{1}{2}°$ contacts	$= 1{\cdot}738$ amp.
Mean sine galvanometer current $\bar{c}$ deflecting magnet to contact edges	$= 0{\cdot}001761$ amp.
Period T_0 of magnet fitted with mirrors	$= 6{\cdot}013$ sec.
Logarithmic decrement λ_0, on open circuit	$= 0{\cdot}00115$
Logarithmic decrement λ, on shorting of coil alone	$= 0{\cdot}1416$
Logarithmic decrement λ, on shorting of coil through 7 ohms	$= 0{\cdot}04245$

Whence from the formula:

$$R = M\,(C/\bar{c})\,\pi^2/T_0\,(\lambda - \lambda_0) \qquad \text{.....(33),}$$

we obtain 2·82 and 9·59 ohms for the two resistances tested, the actual resistances across the shorting key being 2·85 and 9·84 ohms respectively, as measured by Wheatstone bridge.

Resistance determination with a short strong magnet. With a strong cylindrical cobaltcrom steel magnet of actual length 5 cm., diameter 1·6 cm. and approximate moment 700 c.g.s. units, the following data were obtained:

Mean mutual inductance M at extremities of $9\frac{1}{2}°$ contacts	$= 244{\cdot}5\ \mu$H
Balancing current C for magnet, with $9\frac{1}{2}°$ contacts	$= 0{\cdot}8609$ amp.
Mean sine galvanometer current $\bar{c}$ deflecting magnet to contact edges	$= 0{\cdot}001794$ amp.
Period T_0 of magnet fitted with mirrors	$= 4{\cdot}578$ sec.
Logarithmic decrement λ_0, on open circuit	$= 0{\cdot}00132$
Logarithmic decrement λ, on shorting of coil alone	$= 0{\cdot}08965$

Whence, from (33), $R = 2{\cdot}86$ ohms, the resistance, as measured independently being again 2·85 ohms.

The current ratio $C/\bar{c}$ was taken on a thermo-electric potentiometer, the electromotive force being drawn off 0·01 and 10·00 ohms respectively. Self-inductance was negligible in these tests, having an effect of only 0·07 per cent. at most.

§ 6. PROPOSED FURTHER DEVELOPMENTS

A weakness in the present type of apparatus is that any flux through the rotating coil due to a magnet or a solenoid must, of necessity, be neutralized by a flux from the compensator through the same coil and in the contrary direction. This introduces a demagnetizing effect which, though not serious in the experiments described, may well be avoided.

It is proposed, therefore, to construct a new rotating coil of larger dimensions and conforming to theoretical conditions and at the same time to remedy the defect alluded to by making the coil in two portions well separated and probably at right angles to one another, the smaller part only receiving the compensating flux. It is proposed also to consider later the very small corrections due to the width and depth of the magnet.

§ 7. ACKNOWLEDGMENT

In conclusion we would express our deep gratitude to Prof. A. Griffiths of Birkbeck College, who has encouraged us throughout this research and has obtained for us the latest Cambridge pattern of variable mutual inductometer which has proved so useful throughout this investigation.

THE FREQUENCY ERRORS OF RECTIFIER INSTRUMENTS OF THE COPPER-OXIDE TYPE FOR ALTERNATING CURRENT MEASUREMENT

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ABSTRACT. Alternating current milliammeters containing copper oxide rectifiers possess frequency errors of an unusual type. The errors are almost independent of the instrument reading, and thus the percentage errors are inversely proportional to the current to be measured, and may be very large for small currents. It is shown that the errors are due to the capacities of the rectifiers, which, in milliammeters, are of the order $0{\cdot}09\,\mu$F. The peculiar nature of the errors is due to the fact that the resistance of the instrument is approximately inversely proportional to the square root of the current passing through it. It is found to be possible to compensate the frequency errors by providing the instrument with an inductive shunt, the constants of which satisfy a given relation. The perfection of the compensation depends on the resistance of the instrument, and, therefore, on its range. The compensation was practically perfect for a 1·5 mA instrument up to 4,000 ~, and for a 7·5 mA instrument up to 10,000 ~.

§1. INTRODUCTION

The development of the copper oxide rectifier has made it possible for instrument makers to produce portable instruments for the measurement of alternating current, reading directly in milliampères or microampères. Such instruments can now be purchased in this country, and they have been found to be very useful for many purposes in the electrical laboratory. Their handy form,

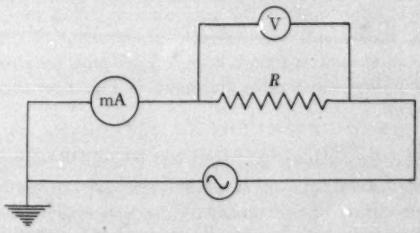


Fig. 1.

almost instantaneous action, and general robustness, are very valuable features, but experiment has shown that they are liable to suffer from a defect which has not yet received sufficient attention; their frequency errors, even at audio frequencies, may be very large.

Three instruments of this type, with ranges of 1·2, 6 and 12 milliampères respectively, were calibrated at various frequencies in the range 50–10,000 ~. The calibrating circuit is shown in Fig. 1, where R is a non-reactive resistance which was given values varying from 500 to 10,000 ohms, and V is a reflecting electrostatic voltmeter giving a full scale reading for 7·5 volts. The earth connection shown eliminated the error due to the earth-capacity of the voltmeter. The self-capacity of the voltmeter and connecting leads was measured, and a correction for this was applied where necessary.

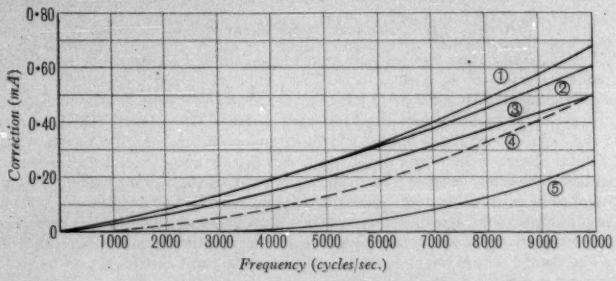


Fig. 2. Frequency errors of rectifier milliammeters.

(1) Instrument of range 2 to 12 mA. (2) Instrument of range 1 to 6 mA. (3) Instrument of range 0·2 to 1·2 mA. (4) Calculated curve. (5) 1·2 mA instrument after compensation.

The results showed the surprising fact that the error of an instrument at any given frequency is approximately independent of the actual reading. Thus the error of the first instrument at 4000 ~, when reading 0·3 mA, was 0·15 mA (an error of 50 per cent.), while the error at the reading 1·2 mA was 0·18 mA (an error of 15 per cent.). The other two instruments, which included rectifiers of the same size, possessed errors of approximately the same amount. Thus the percentage error with these three instruments was inversely proportional to the current to be measured, the smallest value at 4000 ~ being 1·6 per cent. for a reading of 12 mA. The errors for the three instruments are shown in Fig. 2 for the complete range of audio frequencies.

§ 2. THE INSTRUMENT NETWORK

In seeking for an explanation of these errors, one naturally considers the capacities of the rectifiers. Each rectifier consists essentially of a film of cuprous oxide between two metal plate electrodes, and, as the film is very thin, it is obvious that the capacity must be large. A complete rectifier instrument consists of four rectifiers and a moving-coil d.c. instrument connected as shown in Fig. 3 (*a*). Suppose alternating current is passed between the terminals A and B. For each half-wave of current, two of the rectifiers will pass a conduction current, and these may be represented by a resistance R_1 shunted by a capacity C_1 as in Fig. 3 (*b*). The other

R_1, C_1

two rectifiers pass practically no conduction current, but they will still pass capacity current and thus may be represented by capacities C_2. As a first approximation, the moving-coil d.c. instrument may be represented by an inductance L_1. When the current changes its direction, the equivalent network, Fig. 3 (*b*), is turned round, but otherwise remains unaltered, since the four rectifiers have the same properties. Since the network is symmetrical, we may, as a further simple

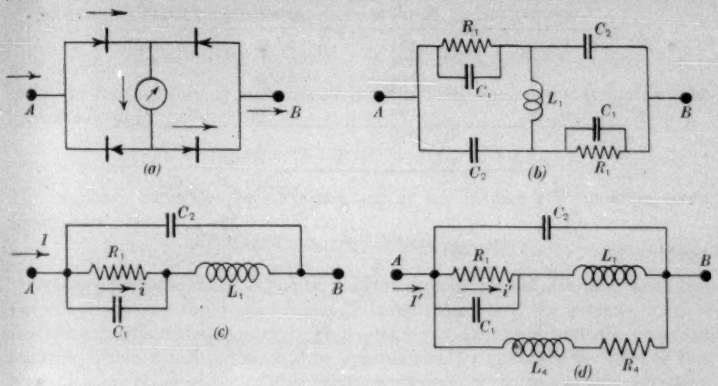


Fig. 3.

approximation, reduce it to Fig. 3 (*c*) (the values of R_1, C_1, C_2, L_1 will of course be different in the two cases). Suppose a current I passes through the rectifier instrument by way of the terminals A and B. A part i of this will flow through the resistance R_1, and it is this part that the instrument indicates. Applying Kirchhoff's laws to the network, we find for the ratio I/i at the frequency $\omega/2\pi$ the vector equation

$$I/i = 1 - L_1C_2\omega^2 + j\omega\,[C_1R_1 + C_2R_1\,(1 - L_1C_1\omega^2)] \quad \ldots\ldots(1),$$

$$\fallingdotseq 1 - L_1C_2\omega^2 + j\omega R_1\,(C_1 + C_2) \quad \ldots\ldots(2).$$

§ 3. IMPEDANCE OF THE INSTRUMENT

In order to get some idea of the quantities involved, measurements of the overall impedance of a rectifier instrument were made by means of the bridge shown in Fig. 4, which is self-explanatory. The ratio arms R_3, R_4, were equal non-reactive resistances. The quantities noted were R_e, the effective shunt resistance of the instrument, and C_e, the effective shunt capacity.

Owing to the rectifying properties of the instrument, perfect silence of the telephone T could never be obtained; the harmonics of the fundamental frequency of the source of current were always strongly audible, but the bridge could be balanced fairly conveniently for the fundamental frequency. The results obtained

represent the constants of the instrument with respect to sine-wave voltage, whereas in using it as an ammeter, we are concerned with its properties with respect to sine-wave current. In considering the results, this limitation must always be borne in mind. The results are given in Table 1.

Table 1. Effective shunt resistance and capacity of rectifier milliammeter.

Current (i) mA	R_e ohms	C_e microfarad	$R_e^2 C_e^2 i$
1	320	0·051	266
2	200	0·057	260
3	155	0·060	260
4	128	0·060	240
5	109	0·060	215

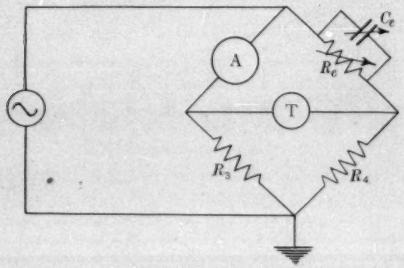


Fig. 4.

Measurements were made at frequencies ranging from 1000 to 4000 ~. The resistance was found to be independent of frequency to the accuracy given. The capacity values seemed to be subject to variations of about 10 per cent. They could not, therefore, be determined with great accuracy, but the observations were sufficient to show that the change with frequency, if any, was not very great.

It is easy to show that the effective shunt resistance R_e and capacity C_e of the network shown in Fig. 3 (*c*) are given by

$$R_e = R_1 [1 + (L_1'/R_1 - C_1 R_1)^2 \omega^2]/(1 + R_1^2 C_1^2 \omega^2) \qquad \ldots\ldots(3),$$

$$C_e = C_2 + (C_1 - L_1'/R_1^2)(1 + R_1^2 C_1^2 \omega^2)/[1 + (L_1'/R_1 - C_1 R_1)^2 \omega^2] \qquad \ldots\ldots(4),$$

where $$L_1' = L_1 (1 + R_1^2 C_1^2 \omega^2).$$

When the variation with frequency is small, these expressions must take the approximate forms

$$R_e = R_1 \qquad \ldots\ldots(5),$$

$$C_e = C_2 + C_1 - L_1/R_1^2 \qquad \ldots\ldots(6).$$

§ 4. THE FREQUENCY ERRORS

We may obtain from equation (2) the ratio of the magnitudes of I and i. Thus

$$I_m/i_m = [(1 - L_1C_2\omega^2)^2 + \omega^2R_1^2(C_1 + C_2)^2]^{\frac{1}{2}} \quad \text{.....(7),}$$

where I_m, i_m are the magnitudes of the vectors I and i respectively. I_m, i_m

Substituting the approximate expressions for R_e and C_e, we get

$$I_m/i_m = [(1 - L_1C_2\omega^2)^2 + \omega^2(R_e)^2(C_e + L_1/R_e^2)^2]^{\frac{1}{2}} \quad \text{.....(8).}$$

When the terms involving ω are small compared with unity, this reduces to the approximate form

$$I_m/i_m \fallingdotseq 1 + \tfrac{1}{2}\omega^2R_e^2C_e^2 + L_1(C_e - C_2)\omega^2 + \tfrac{1}{2}L_1^2\omega^2/R_e^2 \quad \text{.....(9).}$$

The frequency correction of an instrument at any reading i is therefore given approximately by

$$\omega^2[\tfrac{1}{2}R_e^2C_e^2 + L_1(C_e - C_2) + \tfrac{1}{2}L_1^2/R_e^2]\,i \quad \text{.....(10).}$$

It seems probable that the first term is the most important one, and, since this correction is found to be approximately independent of i, the product $R_e^2C_e^2i$ should be approximately constant. This quantity is calculated from the observed results in Table 1 and is found to be approximately constant. The value of this constant being taken as 260 (Table 1), the corresponding values of the correction $\frac{1}{2}R_e^2C_e^2\omega^2$ were worked out and plotted in Fig. 2, where they are represented by the dotted line. It is evident that this supplies an explanation of the greater part of the frequency correction, although the agreement between calculated and observed values is far from complete. No doubt the other terms in (10) account for part of the difference, but the discrepancy must also be due in part to the limited validity of the networks, Fig. 3 (*b*) and (*c*), and of the results given by the impedance bridge, since the observed values of the correction are not proportional to the square of the frequency whereas the calculated ones are so, if R_e and C_e are independent of the frequency as indicated by the bridge results. It is possible that the resistance R_1 might vary with the frequency, if it were measured under the condition of sine-wave current instead of sine-wave voltage.

§ 5. THE COMPENSATION OF THE FREQUENCY ERRORS

A device for the reduction of the frequency errors immediately suggests itself, viz. an inductive shunt across the instrument terminals. This will obviously cause the complete instrument to pass relatively more current at the lower frequencies, and thus will in some measure compensate for the increase of current at the higher frequencies represented by the errors under discussion. Let the network of Fig. 3 (*c*) be provided with a shunt across the terminals AB, of resistance R_4, and self inductance L_4, as in Fig. 3 (*d*). The complete expression for the ratio of the total R_4 L_4

I', i' current I' to the current i' through R_1 is now somewhat complicated, but if $L_1\omega$ is small compared with R_1 it may be shown that the ratio is given approximately by

$$I'/i' = 1 - L_1C_2\omega^2 + j\omega R_1(C_1 + C_2) + R_1R_4/(R_4^2 + L_4^2\omega^2) - j\omega R_1L_4/(R_4^2 + L_4^2\omega^2) \quad \ldots\ldots(11).$$

If we neglect the term $L_1C_2\omega^2$, due to the inductance of the moving coil, which is not likely to be large, we may write the equation for magnitudes

I'_m, i'_m

$$\frac{I'_m}{i'_m} = \left[\left(1 + \frac{R_1R_4}{R_4^2 + L_4^2\omega^2}\right)^2 + \omega^2R_1^2\left\{(C_1 + C_2) - \frac{L_4}{R_4^2 + L_4^2\omega^2}\right\}^2\right]^{\frac{1}{2}} \quad \ldots(12).$$

For frequencies such that $L_4\omega$ is small compared with R_4, we may make a further approximation and obtain

$$\frac{I'_m}{i'_m} = \frac{R_1 + R_4}{R_4}\left[\left\{1 - \frac{L_4^2\omega^2}{R_4^2}\frac{R_1}{R_1 + R_4}\right\}^2 + \left(\frac{R_4}{R_1 + R_4}\right)^2\omega^2R_1^2\left\{(C_1 + C_2) - \frac{L_4}{R_4^2}\right\}^2\right]^{\frac{1}{2}} \quad \ldots\ldots(13).$$

By expanding this in ascending powers of ω^2, and equating the coefficient of ω^2 to zero, we obtain, as the condition under which the frequency correction is zero, to the first order of small quantities:

$$\frac{2R_1}{R_1 + R_4}\frac{L_4^2}{R_4^2} - \frac{R_4^2}{(R_1 + R_4)^2}R_1^2\left[(C_1 + C_2) - \frac{L_4}{R_4^2}\right]^2 = 0,$$

which reduces to

$$C_2 + C_1 = \frac{L_4}{R_4^2}\left[1 + \left\{\frac{2(R_1 + R_4)}{R_1}\right\}^{\frac{1}{2}}\right] \quad \ldots\ldots(14).$$

It must be remembered that R_1 varies with the current, so that, strictly speaking, compensation can only be effected for one scale reading; but the smaller the ratio R_4/R_1 the less will be the variation with R_1 of the value of L_4/R_4^2 required to compensate a given capacity, and thus the more perfect will be the compensation over the whole instrument scale. R_4 cannot be made very small without undue reduction of the sensitivity of the instrument. The instruments referred to above were provided with experimental shunts, in which the mean ratio of R_4/R_1 at the most important part of the scale was about 2. The value of the resistance having been adjusted, the instrument was re-calibrated at 50 ~. The frequency was then raised to, say, 4000 ~, and inductance was added to the shunt until the calibration was the same as at 50 ~. The constants of the shunts required for the three instruments were as shown in Table 2.

Table 2. Constants of the shunts required for frequency compensation.

Instrument range mA	R_1 ohms	R_4 ohms	L_4 mH	L_4/R_4^2	$C_2 + C_1$
7·5	130	300	2·3	$\cdot025 \times 10^{-6}$	$\cdot090 \times 10^{-6}$
15	130	370	3·2	$\cdot024 \times 10^{-6}$	$\cdot091 \times 10^{-6}$
1·5	500	1350	38	$\cdot021 \times 10^{-6}$	$\cdot078 \times 10^{-6}$

The corresponding values for $(C_1 + C_2)$ calculated from (14) are given in the last column. It will be seen that these capacities and the values of L_4/R_4^2 are approximately constant, the capacities being about 50 per cent. larger than those measured by the bridge. This is to be expected from equation (6).

The final performance of the instruments when fitted with these shunts was very satisfactory. The frequency errors of the 7·5 and 15 mA instruments (6 mA and 12 mA without the shunts) were so small, even up to 10,000 ~, that they could not be detected with certainty. The error was certainly less than 1 per cent. of the maximum reading up to 10,000 ~, and this accuracy held for all scale readings. Mr F. M. Colebrook, of the Wireless Division of the National Physical Laboratory, kindly calibrated the 7·5 mA instrument at frequencies of 20 and 40 kilocycles. The errors were approximately the same for all scale readings and were found to be 0·19 and 1·8 mA respectively for these two frequencies. Thus the useful working range of these two instruments may now be taken as 25 to 10,000 ~. The 1·5 mA instrument was free from frequency errors up to 4000 ~. At 5000 ~ the error was 1 per cent. of the maximum scale reading, and it increased rapidly beyond this point, as may be seen from the curve in Fig. 2. The frequency range of this instrument may therefore be considered to be 25 to 5000 ~.

§ 6. FREQUENCY ERRORS AND TEMPERATURE COMPENSATION

It may be worth while to warn users of commercial models of rectifier instruments that they sometimes possess frequency errors much larger than those discussed in the early part of this paper. The reason is, that it is the practice to compensate the temperature errors of the instruments by providing them with a copper shunt of a suitable resistance. This shunt is unfortunately wound inductively and possesses a self-inductance which may be more than ten times as large as that required to compensate the natural frequency errors of the instrument. The consequence is that the instrument has large errors, of the opposite sign to those found for an unshunted instrument. In a particular case an instrument possessed a correction of − 15 per cent. at 2000 ~, whereas its correction with the temperature compensation removed was only + 0·5 per cent. at the same frequency. The temperature coefficient of the three instruments discussed in this paper was measured, but no constant value was obtained. For one instrument, the coefficient was 0·3 per cent. per 1° C. at a reading of 3 mA, and about half this amount at a reading of 6 mA. For another instrument it was too small to be measured over the range 15° to 20° C. As, moreover, under fixed conditions, the instrument readings were apt to show erratic changes, of the order of 1 per cent. in some cases, it is doubtful whether temperature compensation is worth while. If, however, rectifiers of sufficient stability can be produced, it seems probable that both temperature and frequency errors could be compensated by the shunting of the instrument with a coil of copper wire chosen so that its resistance R_4 is of the value required for temperature compensation, while its inductance L_4 is such that the value of L_4/R_4^2 would compensate for the frequency correction.

The actual performance of a commercial instrument is shown in Fig. 5, which gives in detail the calibration curves for the instrument (1) as originally supplied, (2) with the shunt provided for temperature compensation removed, (3) with the shunt required for frequency compensation fitted.

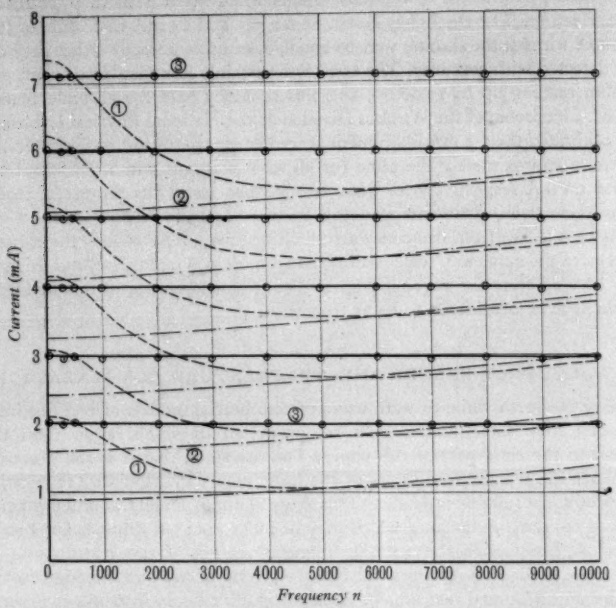


Fig. 5. Frequency errors of rectifier milliammeters. Current/frequency curves for constant instrument readings; (1) as originally supplied; (2) with original temperature-compensating shunt removed; (3) with frequency-compensating shunt added.

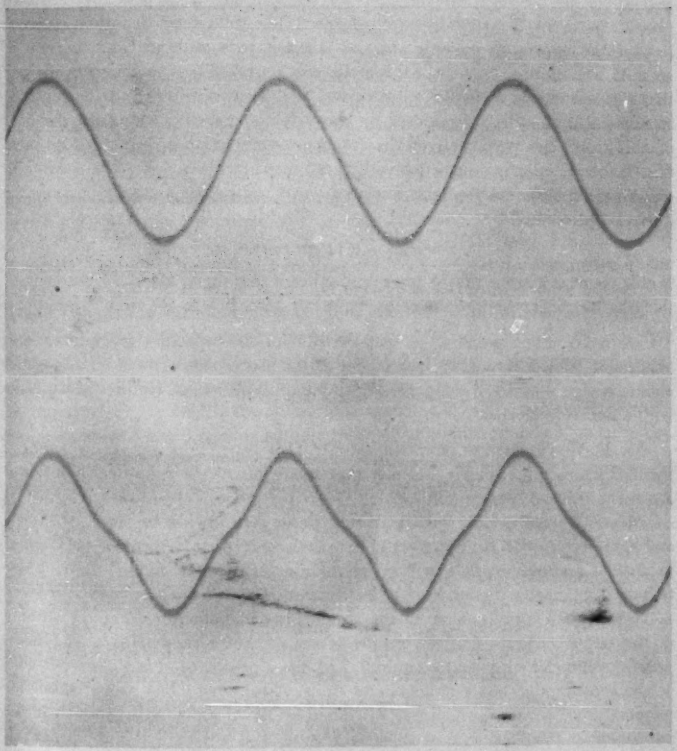
Each curve represents the variation with frequency of the actual current flowing through the complete instrument for a constant scale reading. Thus, parallelism of the curves indicates that the error is the same for different scale readings, and for a perfect performance the curves become horizontal straight lines.

§ 7. ACKNOWLEDGMENT

I wish to acknowledge my indebtedness to Mr F. Cheeseman for assistance in making the measurements recorded in this paper.

DISCUSSION

Mr D. A. Oliver: Thousands of small a.c. rectifier instruments are now in use and comparatively little attention has been paid to their errors. Dr Hartshorn's paper therefore appears at an opportune time and is, I believe, the first to deal



Wave-form distortion produced by rectifier instrument.

thoroughly with the more serious errors involved. My first introduction to the large frequency-errors of commercial instruments of this type was in taking out an approximate frequency-characteristic of a resistance-capacity-coupled amplifier of which the amplification was found to fall off much more rapidly than was to be expected. Subsequent calibration of the rectifier meter against a thermojunction

ammeter explained the results completely. The simple and effective method of frequency-compensation described in the paper will doubtless find favour in practice. There is, in addition, the question of wave-form distortion. The two oscillograms here shown were taken at the Research Laboratories of the General Electric Company by Mr J. S. Thompson and myself; first a resistance and secondly a bridge-type rectifier ammeter were connected in the low resistance secondary circuit of a step-down transformer. The wave-form distortion recorded is thus the maximum possible and was taken at 50 ~ with an r.m.s. current of 20 mA. The maximum range of this meter was 50 mA. These oscillograms indicate that the resistances R_1 in the approximate equivalent network must be regarded as non-linear as well as dependent on the value of current flowing. In the light of this result the perfection of the frequency-compensation becomes the more remarkable. Resistance in series with these instruments reduces the wave-form distortion. I have analysed the distorted wave and to the accuracy possible the curve is represented by

$$i = 20\sqrt{2}\,(\sin \omega t - 0{\cdot}14 \sin 3\omega t) \times 10^{-3} \text{ amp.}$$

If this result is assumed to be typical, it appears that up to about 15 per cent. o third harmonic can be introduced by the meter according to the circuit conditions.

Mr R. W. Paul pointed out that the sum of the capacities given in the last column of Table 2 is roughly the same for all the instruments. Would it be practicable to design a standard compensating shunt applicable to all rectifier instruments with a fair degree of accuracy?

Mr E. W. H. Banner (communicated): This paper is of interest to users of rectifier instruments, but it is a pity that some statements and conclusions in it are apt to mislead users to some extent.

Messrs Ferranti, Ltd. have marketed these instruments for over two years and have performed a large number of tests both on rectifier elements of the copper-oxide type and on complete instruments. Unfortunately the author's tests appear to have been made with one make of instrument only, and the generalizations given do certainly not apply to instruments of the above manufacture, although nothing in the paper suggests that the results given are not representative. The usual rectifier voltmeter supplied by Ferranti, Ltd. has a resistance of 667 ohms per volt, or the consumption is 1·5 milliampères r.m.s. at full scale. The copper shunt for temperature correction, which was advocated by the above-named firm in a paper published in 1929*, is a most valuable feature, and although the compensation varies with different ranges it does in general reduce the error from a high one of the order of 0·5 per cent. per ° C. to the order of 0·1 per cent. per ° C. or less. At the same time it was realized that inductance was desirable for frequency-compensation but that it could be overdone, and so such copper shunts are wound "commercially" non-inductive. Actually the shunts used on a voltmeter consuming 1·5 milliampères have an inductance of about 30–40 millihenries, which is about

* *Electrical Review*, **104**, 823 (1929).

the figure of 38 given in the paper and not ten times this as stated to be found in commercial instruments. As a result the frequency error is quite small over the range 20–4000 cycles per second, generally within ± 2 per cent., and up to 6000 cycles per second the error is still small although somewhat greater. Numerous tests have been made on Ferranti rectifier instruments by various government departments and testing laboratories and they confirm Ferranti's tests, showing that the copper shunt does not render the frequency error prohibitively high. It has been found that the frequency curve of a rectifier instrument over a wide range of frequency is not a simple curve but one varying alternately about the zero line, and that for correct compensation for high frequencies various networks, each designed to compensate for a given portion of the curve, become necessary; but this is certainly not a commercial proposition. The reference to "erratic changes" is of interest, and although instability of the rectifier can easily be produced by overload such erratic readings when an instrument has been turned out correct are known but inexplicable, so far as I am aware.

AUTHOR's reply: Mr Oliver's oscillogram is a very useful contribution to the subject. The amount of third harmonic present is surprisingly large in the case chosen (admittedly the worst). It suggests that the wave-form errors might be considerable. This point was investigated, and although I have no quantitative information, I have satisfied myself that in ordinary practical cases the wave-form errors are not large, not more than 1 per cent., say.

In answer to Mr Paul, I am of the opinion that it would be quite practicable to design a standard compensating shunt applicable to all instruments of given range. The only doubtful factor is the degree of reproducibility of the rectifiers, and this could very simply be ascertained by making measurements of the effective resistances and capacities of a batch of rectifiers by the bridge method I have described.

Mr Banner appears to be under the impression that I have not tested the instruments made by Messrs Ferranti, Ltd., and that these are free from the errors that I have discussed. Such is not the case, and I cannot agree that users of such instruments are likely to be misled by any of my statements and conclusions, provided they are read with reasonable care. The statements made in the technical press as to the accuracy of copper-oxide rectifier instruments have shown that manufacturers were quite unaware of the existence of large frequency errors in their instruments, and especially of the large variation of percentage error with the range of the instrument. My object has been to explain these facts and to show how the errors may be corrected, and I shall be surprised and disappointed if the instruments manufactured in the next two years are not very much better than their predecessors. My measurements have shown that the frequency curve of the rectifier itself is a simple curve, and that, whenever the more complicated curve referred to by Mr Banner is obtained, it is due to the presence of too large an inductance.

A METHOD OF ELIMINATING THE EFFECTS OF MAGNETIC DISTURBANCE IN HIGHLY SENSITIVE GALVANOMETERS

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ABSTRACT. The paper describes a method of making correction for the chaotic fluctuations, of magnetic origin, to which the zero of a highly sensitive moving-magnet galvanometer is susceptible. The method consists in the employment of a second galvanometer with properties adjusted as nearly as possible to identity with, and placed as close as possible to, the first. No current is passed through the second galvanometer, which acts in this respect as a dummy whose sole function is to record the zero changes. As a result of the proximity of the galvanometers, and the interrelation of their dynamical and geometrical conditions, the recording spots of light have an approximately constant separation, in spite of the fluctuations in the position of each. The current to be measured activates one of the galvanometers, and is measured by the accompanying change in the separation of the spots. The paper also describes a compensating method of observing, in which a single beam of light is reflected in succession from the mirrors of the active and dummy galvanometers so that the emergent beam tends to remain stationary.

§ 1. INTRODUCTION

Many branches of experimental work, in which a galvanometer of very high effective sensitivity is an essential part of the equipment, have called urgently for an improvement in existing sensitivity. Thus the investigations of Prof. A. V. Hill on the production of heat by nerves have necessitated and led to much successful work on the Paschen type of galvanometer by himself and Mr A. C. Downing of University College, London. Infra-red spectroscopy, and indeed radiometry in general, is another important field of research in which it is well recognized that future progress depends to a great extent upon the possibility of securing an increase in effective sensitivity. While in recent years great progress has been made, especially in America, towards greater spectroscopic resolution by the development of the échellette grating with a consequent increase in the amount of radiation available, progress has also been considerable in the improvement of detecting systems. The thermopile has been rendered very stable and rapid in action, while the figure of merit of galvanometers has been considerably increased, particularly by the employment of new types of magnet steel. The demands of the experimenter have however by no means yet been satisfied. Indeed, the progress made with galvanometers is to some extent counterbalanced by the great increase in recent years of the number of mechanical and magnetic disturbances which are produced in the neighbourhood of most laboratories, and have made it necessary,

in order even to maintain the position already reached, to give attention to the improvement of galvanometers.

In order to avoid the loss of effective sensitivity caused by these disturbances many workers have found it desirable to experiment in the small hours of the morning when the disturbances reach a minimum, but the inconvenience and fatigue involved in such a choice of time are undoubtedly an impediment to any protracted research requiring it.

The method here described has been found to give in this laboratory the same order of effective sensitivity during the day-time as is obtained without it under the most favourable conditions at night.

In the earlier stages of a research which required the use of a galvanometer having very high sensitivity, we were fortunate in obtaining an instrument embodying the result of recent work by Mr A. C. Downing and made by him. This galvanometer had the following special features: it removed all difficulties connected with mechanical vibrations, as it was found to be quite immune from such disturbances even when supported on a wooden table without any precautions for the elimination of shocks; it also removed the necessity for magnetic shielding, as the system had been adjusted to be so nearly astatic that it was disturbed only by the introduction of a magnetic field whose non-uniformity was considerable. The presence of a shield, by distorting the field within it, results, not in a gain, but in a loss of stability.

However, this galvanometer, excellent as it is, was still affected to an undesirable extent by the parasitic magnetic fields present in this laboratory. At a sensitivity of the order of 10^{-11} amp./mm. at 1 m. the chaotic movements due to this cause were sometimes as large as 5 mm. The elimination of the effects of these is an important preliminary to our utilizing the highest sensitivity, and forms the subject of the present paper.

§ 2. DESCRIPTION OF THE METHOD AND APPARATUS

If a galvanometer is set up in a certain position in an approximately uniform magnetic field the disturbing forces to which it is subject will be only slightly different at a neighbouring point in space. Consequently the behaviour of the galvanometer would be nearly the same in either of two adjacent positions. If then two absolutely identical galvanometers could be obtained and placed as near together as possible, one would expect them to perform the same series of movements as a result of fluctuations in the surrounding magnetic field. One galvanometer could then be used to measure the electric currents while the other acted as a dummy whose sole function is to record changes of zero, the position of the spot of light from the current-activated galvanometer being measured not from a fixed point on the "scale" (nor from that point of the scale occupied by the spot before the current was switched on) but from the spot of the dummy galvanometer. The success of such a method clearly depends on the nearness of the approach to identity of the two galvanometers and the closeness to each other with which they can be placed.

Fortunately, a few weeks after the method had been devised a second galvanometer became available for sufficient time to give the method a trial. The results obtained do not necessarily represent the best which the method is capable of giving. Firstly no attempt had been made to render the properties of the galvanometers identical, and secondly they were placed only so near together as the size of their bases would allow: it was not desirable at the time to construct bases specially adapted to allow a closer approach of the systems. The actual separation used was 25 cm. Experiments made on the mutual action between the two galvanometers indicate that a much closer approach would be possible without this action becoming prejudicial. Although the two galvanometers were not exactly matched,

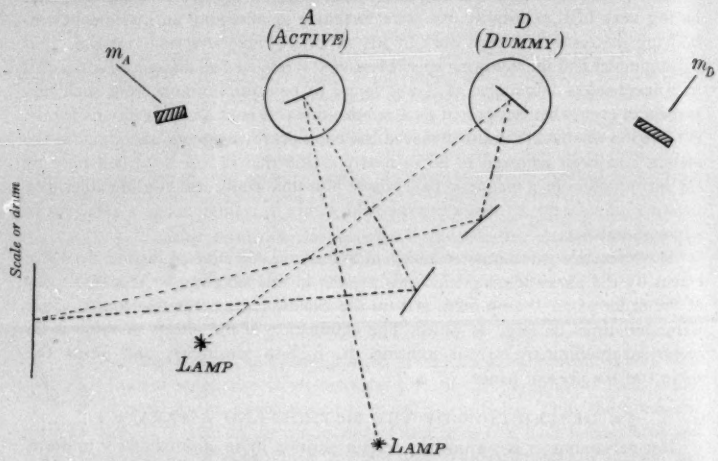

Fig. 1. Arrangement for adjusting twin system of galvanometers.

it was possible by using mechanical control of the orientation, magnetic control of the period, and optical control of the sensitivity, to bring the main relevant factors within the scope of adjustment. Thus, in order to make the two spots behave in the same way in spite of the lack of identity in the galvanometers themselves, the following method was used:

Firstly the galvanometers were adjusted in azimuth so that when all control magnets were removed each system of suspended magnets was in equilibrium parallel to the planes of windings of its own set of coils, or, what is the same thing, so that each window was parallel to the suspended mirror.

In the second place, when the sensitivity of the measuring galvanometer A, Fig. 1, had been adjusted to the desired value by means of a controlling magnet m_A placed at the side of A remote from D, the period of D was adjusted to equality

with that of A by means of a controlling magnet m_D remote from A. This disposition of the control magnets gives a reasonably independent control of each galvanometer. The equality of periods can be judged with fair nicety by watching the two spots swing together. Both galvanometers were almost critically damped. In order to make these adjustments it is almost essential to mount m_A and m_D so that they are capable of fine adjustment. The arrangement used in practice is shown in Fig. 2. The magnet m is mounted on a vertical rod which can be raised or lowered, adjusted to and kept at any desired height by the collar c, and clamped to the vertical pillar by the screw s. Coarse adjustment of m in rotation

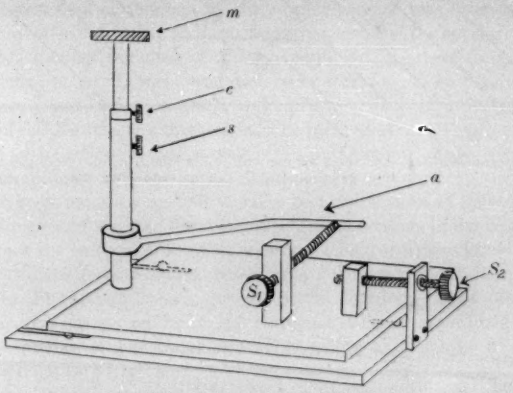

Fig. 2. Mounting for adjusting control magnets.

about a vertical axis is given by unclamping at s and rotating by hand: fine adjustment is given by operation of the screw S_1, which bears on a radial arm a. Coarse adjustment of the distance of m from the galvanometer is given by the sliding of the whole mounting along the table, and fine adjustment by the turning of the screw S_2 which moves a slide on which the pillar carrying m is mounted.

Finally, the periods having been adjusted to equality, we can make the effective sensitivities equal by adjusting the relative lengths of the optical paths between the galvanometers and the scale. To effect this and also to direct each beam on to the scale it may be necessary to employ two reflections of each beam. This was in fact necessary in the first trial given to the method, but fortunately in a subsequent set-up the galvanometers happened to be so orientated that a single reflection (given by a 90° prism) was sufficient to fulfil the necessary conditions for each beam.

§ 3. EXPERIMENTAL DETAILS

Before proceeding to describe the results obtained it is of interest to examine the actual methods by which observations may be made. It is clear that it would be unprofitable to attempt to make visual determinations of the simultaneous

positions on the scale of two fluctuating spots. It is possible, however, to judge with accuracy when the two spots are superimposed or when their separation changes, if this is small, by using such a type of spot as is shown in Fig. 3. A null method is therefore very easily applicable. The deflections of the spot due to passing a current through the activated galvanometer may be compensated to a measured

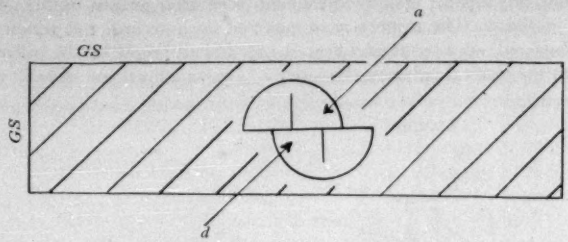

Fig. 3. Appearance of scale and spots for use with null method. *a*, spot from galvanometer *A*; *d*, spot from galvanometer *D*; *GS*, galvanometer scale.

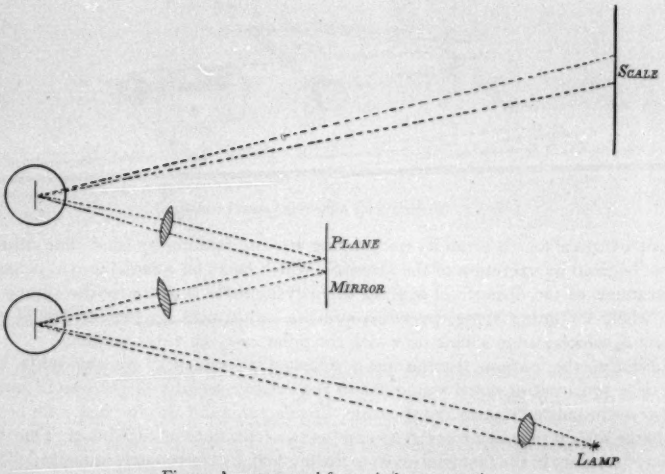

Fig. 4. A system used for optical compensation.

extent either electrically by means of a potentiometer or optically by the use of a rotatable mirror after the manner of a sextant. The electrical method is preferable for the reason that its use eliminates any non-linearity of the current scale of the galvanometer and avoids the need for its calibration.

Another method of optical compensation suggested by my colleague, Mr J. Guild, was developed and tried, in which only one beam of light was used and

passed from one galvanometer mirror to the other on its way to the scale, with the result that no displacement of the spot occurs if the two galvanometer mirrors are rotated through equal angles. This procedure was subsequently suggested in principle by several other physicists to whom the outlines of the general method had been shown. In particular a similar arrangement is suggested by Mr A. B. D. Cassie* to whom the system of galvanometers had been shown by Mr Downing, but who was unaware that the method of optical compensation had already been applied. An arrangement employed in this laboratory is illustrated in Fig. 4, which is self-explanatory. The method proved quite satisfactory in use but does not give greater precision than is obtained by the employment of two independent spots; moreover it involves the use of additional optical parts, and the aperture tends to be more limited. Further the absence of movement disguises the disturbances which are being compensated. It was found preferable, therefore, at least during the stage of preliminary investigation, to revert to the previous method, which shows clearly the details of the disturbances themselves. The method of compensation is of course applicable to a deflection method as well as to a null method, and for some purposes this is advantageous.

Use has been made by several workers of amplification of galvanometer deflections by means of a relay. The advantage gained consists in the relative ease of working, since the galvanometer gives the same effective sensitivity when in more stable adjustment. There seems no reason why a similar method should not prove applicable to the method of twin-galvanometers described in the present paper. The investigation has not reached a stage at which it has been profitable to develop this refinement, but it may be of interest to give a brief outline of one type of scheme which appears feasible.

A rectangular opaque screen XY, Fig. 5, is placed in the path of the beams of light reflected from the galvanometers so that the spots are focussed on it. The spots might be circular or rectangular, but whatever their shape the separation of their vertical axes is made approximately equal to the width of the screen XY. This result may be accomplished by controlling either the separation of the spots or the width of XY, whichever is more convenient in practice. The portions of the beams unobstructed by XY are directed by prisms on to a photo-electric cell C, the current through which is measured by any suitable means. If we assume that the energy is uniformly distributed over the cross-sections of the beams, it is clear that with such an arrangement a common displacement of the two beams will produce no change in the photo-electric current, while a relative displacement will do so. The method of optical compensation giving a single spot would be immediately adaptable to types of relays already designed for such purposes.

The method finally adopted to investigate the behaviour of the two galvanometers was to take photographic records on a revolving drum. This method has indeed advantages which render it in some respects preferable to the visual null method not only for such preliminary investigations as that in which it was here

* *Journal of Scientific Instruments*, 7, 21 (1930).

employed but for actual measurements. It creates a permanent record which can be re-measured at leisure, eliminates the personal judgment which enters into the visual settings, and gives a continuous record of the behaviour of the galvanometers during the time occupied in the measurements, instead of a discrete series of observations selected more or less arbitrarily by the observer. In the final stages of measurement these advantages outweigh the disadvantages due to the indirectness of photographic methods and to the inapplicability of the null method.

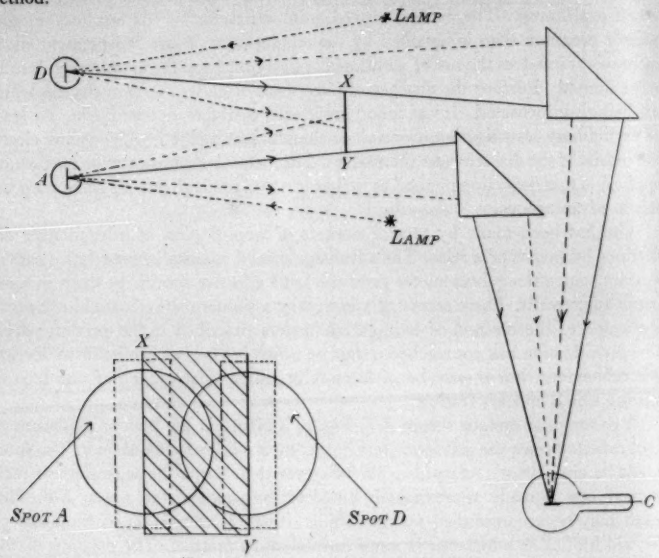

Fig. 5. System suggested for use with relay.

§ 4. TYPICAL RESULTS

Typical records are reproduced in Fig. 6 (*a*), (*b*), (*c*). Records (*a*) and (*c*) were taken during laboratory working hours, whereas (*b*) was taken at 1.0 a.m. The vertical lines are time-traces obtained by flashing on a pea lamp for about 1 second. These traces are marked with numbers representing the time in minutes from the first trace. The time scale is the same in (*a*) and (*b*): in (*c*) it is much reduced, so that a longer time is included but at the expense of the finer structure in the records.

It will be noticed that in (*a*) and (*b*) one trace is repeatedly displaced in successively opposite directions. These displacements are due to the switching on and off of a current of 10^{-9} amperes which is made to pass through one galvanometer. In (*a*) the track of the current-activated galvanometer will be found to cross the track

of the dummy galvanometer on several of the occasions of switching on and off. In other regions there is nearly exact superposition of the tracks. The relative smallness of the disturbances at 1.0 a.m. is clearly brought out in (*b*). In this record, where the irregularities are themselves much smaller, the relative irregularities are more apparent.

No current was passed through the galvanometer during the making of record (*c*), which thus represents the "natural" behaviour of the galvanometers for about 1 hour. At various points of the record will be seen discontinuities in the traces, on

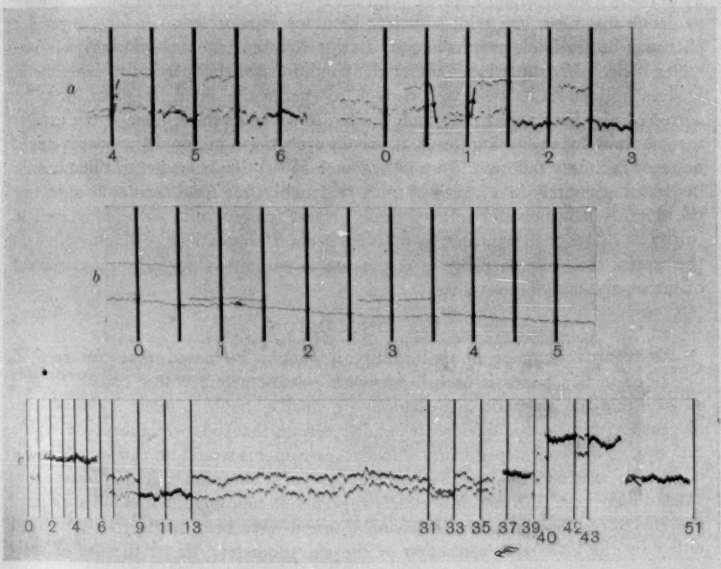

Fig. 6. Typical records obtained with the twin system.

either side of which the separation of the traces differs. These discontinuities were produced by the action of a heavy luggage lift situated about 10 yards from the galvanometers, and, as it happened, in the most favourable position for producing a relative disturbance; that is, nearly in line with them. The changes in the relative and absolute positions of the traces after a discontinuity are due to the changes in height of the lift and its counterpoise.

In each record slow changes occur in the separation of the two traces. This is particularly striking in (*b*). These changes may be accounted for in several ways, and probably are removable. They do not however appreciably affect the accuracy of measurement of current. The function of the dummy galvanometer is,

so to speak, to bridge the time-gap required by the activated galvanometer for settling down after activation, and to indicate the change of "zero" which has occurred after this gap. If the activated galvanometer had infinitesimal period the correction for change of zero would be infinitesimal and the dummy galvanometer would be superfluous.

In practice the time-gap is of the order of 10 seconds and in this time the secular change is negligible. When the disturbances reach so small a magnitude as in (*b*) we can obtain with sufficient accuracy the natural or steady scale reading by drawing a mean straight line through the trace, and the length of trace necessary to define the mean line with sufficient accuracy is short enough for the secular change to be negligible over its length. In these circumstances the dummy galvanometer is probably superfluous under the conditions of this particular experiment. It does not follow its twin well enough to enable the small irregularities to be corrected any better than they could be by drawing the mean line. If the experimental conditions were improved, however, even these minor disturbances might be more faithfully followed. In a record such as (*a*) the mean straight line cannot be drawn accurately in a length of track reasonably free from secular change, but the dummy galvanometer follows these grosser irregularities sufficiently well to justify its presence in securing, by measurements of separations of single points on the tracks, the same accuracy as is obtainable without it in (*b*) by measurement of the separations of mean lines.

§ 5. CONCLUSION

As regards its general applicability it should be noted that the method described in this paper is useful only with galvanometers whose construction is so good that the magnetic disturbances are small enough to make it desirable to dispense with magnetic shields. It is fairly certain that the correlation between the movements of two independently shielded instruments would be very small, and it is unlikely that success would attend the use of a common shield of reasonably small dimensions. Further, it is clearly superfluous to apply the method at all until the effects of disturbances of mechanical origin have been made relatively small either by the internal construction of the galvanometers, or by the use of anti-vibration supports.

In conclusion it may be stated that Mr Downing has meanwhile been carrying out promising experiments with the object of producing two systems very nearly identical. The results already obtained and here described make it appear reasonable to hope that the use of such improved systems, and a decrease in their working separation, will still further increase the utilizable sensitivity of this type of galvanometer.

ADDENDUM

Since the above paper was written my attention has been called to a paper by Dr G. A. Shakespear*, in which some similar arrangements are described.

* G. A. Shakespear, "Experiments on the temperature coefficient of a Kew collimator magnet," *Proc. R. S.* A, **39**, 220 (1913).

CLASSICAL QUANTUM THEORY AND X-RAY EXCITATION BY CANAL RAYS AND ALPHA-PARTICLES

By WILLIAM BAND, M.Sc.

Received March 14, 1930. *Read in title June* 26, 1930.

ABSTRACT. This paper shows, by application of the classical quantum theory and simple equations of energy, that it is not possible for canal rays of normal experimental energy to remove K-electrons from the atoms of a metal target either by capture or by simple removal into free space. No particular mechanism is assumed, but a safe maximum for the possible transfer of momentum to the ejected electron is taken to be the transfer which takes place in a head-on collision, electric attraction being neglected. The fact that alpha-particles are observed to excite X-rays shows that the mechanism must be almost as efficient as this. The process of capture is shown to be no more probable than that of simple removal into free space. The minimum energy for canal rays and the minimum velocity for alpha-particles, for K-radiation from copper, are calculated as at least 72 million volts and $1{\cdot}7 \times 10^8$ cm./sec. respectively. The theory claims to give a satisfactory explanation of all the experimental data so far available.

§ 1. THE PROBLEM: EXPERIMENTAL DATA

In an early investigation, J. J. Thomson appeared to have detected X-radiation excited by canal rays impinging on a metal target, but his results were explained away by an observation of C. Gerthsen* that canal rays when reflected from the target surface often have their charges neutralized; they were thus able to pass through the electric-field-filter which was used by Thomson to shield the plate from the reflected rays. Gerthsen, using a more refined method, detected an exceedingly weak effect due possibly but not certainly to soft radiation excited by the canal rays.

J. J. Thomson, using a specially assumed model for the collision process, showed theoretically that a proton possessed of the energy actually available could not remove an electron from any X-ray level of the target atoms; but his model also required that the alpha-particle should be unable to effect the removal. His model is therefore unsatisfactory, for alpha-particles have been observed to excite X-rays†.

In a recent piece of work Prof. Barton‡, using a highly sensitive instrument, shows that there is negligible radiation due to even heavy currents of protons up to 25 kV, the efficiency relative to the excitation by electrons being at the most of the order 10^{-5}.

* C. Gerthsen, *Ann. der Phys.* **85**, 881 (1928).
† W. Bothe and H. Franz, *Zeit. für Phys.* **52**, 446 (1929).
‡ H. A. Barton, *Journ. Frank. Inst.* p. 1 (1930).

We therefore require a theory which while it allows the alpha-particle to excite radiation, must render it practically impossible for protons of the above energy to do so. This paper shows that such a theory may be erected from the quantum theory without any assumption as regards the mechanism whereby the momentum is transferred to electrons.

§ 2. QUALITATIVE SUGGESTIONS

Barton apparently supposes that there is no essential difference between the processes of excitation of radiation by positive and by negative particles. But excitation by electrons approximates to the collision between two elastic spheres, on account of the mutual repulsion between the incident and the ejected electrons, whereas excitation by protons or alpha-particles must be essentially due to the *K*-electron being pulled out from the atom by the attractive force exerted by the close approach of the incident particle.

Accepting Thomson's results provisionally, that the mere transfer of momentum during close approach is not sufficient to effect the complete removal from the atom, we are left with the one alternative that the proton or alpha-particle captures the electron and retains it in an appropriate quantum orbit after emerging from the atom. This process may quite possibly be more efficient than complete removal of the electron into free regions, for in the quantum orbit round the capturing nucleus the electron will still have a negative energy, whereas in the free regions it cannot have less than zero energy. The kinetic energy of the capturing nucleus will modify this result to an ascertainable degree.

We are thus led to the following physical picture. An electron revolves in the inner quantum orbit of a metal atom. An incident nucleus, H or He, enters the field of the metal nucleus, and with diminished energy due to the repulsion of the latter approaches very closely to the electron. The electron is deflected from its normal course and sweeps in an open curve round the intruder. Work has been done on the electron by the field of the incident particle, and this is sufficient to carry the electron out into one of the inner orbits of the new nucleus, by which time the latter has carried its prize well out of the field of the original atom. Since the intruding nucleus is of less atomic number than the metal atoms, the energy of the electron has been increased, and by an amount that can be calculated by the rules of the quantum theory.

The possibility of capture thus depends primarily on the amount of energy that must be given to the electron in order to get it into the innermost orbit round the capturing nucleus, and this will be greater for the proton than for the He nucleus. Hence we can already see that the latter will have a much greater chance of capturing the electron than will the proton, quite apart from its greater energy and momentum.

It will thus be seen that, if we assume the quantum theory, simple equations of energy will suffice to solve the problem, without any other assumptions as to the mechanism of the transfer of the momentum. We shall proceed accordingly,

taking for simplicity the simple circular quantum orbits of elementary quantum theory.

§ 3. PROTON COLLISION

Let M	be the mass of the incident proton;	M
Let V	be its initial velocity on entering the target;	V
Let V'	be its velocity after escape from the target atom;	V'
Let e, m	be the electronic charge and mass;	e, m
Let a	be the radius of the K-orbit in the target atom;	a
Let b	be the radius of the orbit about the proton after capture;	b
Let Z	be the atomic number of the target atom; and	Z
Let E	be the kinetic energy transferred to the electron during the close encounter, all units being in the c.g.s. system.	E

We assume that the proton has negligible effect on the target nucleus. Then the equation of energy of the electron alone is

$$E - Ze^2/2a = \tfrac{1}{2}mV'^2 - e^2/2b \qquad \text{.....(1)},$$

and that of the complete system is

$$\tfrac{1}{2}(M+m)\,V'^2 - e^2/2b = \tfrac{1}{2}MV^2 - Ze^2/2a \qquad \text{.....(2)}.$$

If b is the radius of the innermost orbit round the proton, then

$$b = Za,$$

and by (2) therefore

$$\begin{aligned} V'^2 &= e^2\,(1/b - Z/a)/(M+m) + MV^2/(M+m) \\ &= MV^2/(M+m) - e^2\,(Z-1/Z)/a\,(M+m) \end{aligned} \qquad \text{.....(3)}.$$

Adding together (1) and (2) we get

$$E = \tfrac{1}{2}MV^2 - \tfrac{1}{2}MV'^2 \qquad \text{.....(4)},$$

which by (3) gives

$$E = [\tfrac{1}{2}MV^2 + \tfrac{1}{2}(M/m)\,(e^2/a)\,(Z - 1/Z)].\,m/(M+m) \qquad \text{.....(5)}.$$

Taking Z as 29, and canal rays of 25 kV, or $\frac{1}{2}MV^2$ as 4.10^{-8} ergs, we calculate that

$$E = 1{\cdot}7.10^{-8} \text{ ergs} \qquad \text{.....(6)}.$$

§ 4. ALPHA-PARTICLE COLLISION

In terms of the same symbols as in the last section, the mass of the alpha-particle is $4M$ and its charge $2e$; the equations of energy become for the ejected electron

$$E - Ze^2/2a = \tfrac{1}{2}mV'^2 - 2e^2/2c \qquad \text{.....(1')},$$

and for the complete system

$$\tfrac{1}{2}(4M+m)\,V'^2 - 2e^2/2c = 2MV^2 - Ze^2/2a \qquad \text{.....(2')}.$$

c Here c is the radius of the orbit round the He nucleus, so that

$$2c = Za.$$

From (1′) and (2′) we get

$$V'^2 = 4MV^2/(4M + m) - e^2 (Z - 4/Z)/a (4M + m) \quad \ldots\ldots(3')$$

and

$$E = 2MV^2 - 2MV'^2 \quad \ldots\ldots(4'),$$

so that finally

$$E = [2MV^2 + 2 (M/m) (e^2/a) (Z - 4/Z)] . m/(4M + m) \quad \ldots\ldots(5').$$

Assuming for the velocity V of the alpha-particle that

$$V = 2 . 10^9 \text{ cm./sec.},$$

we calculate

$$E = 1{\cdot}6 . 10^{-8} \text{ ergs} \quad \ldots\ldots(6').$$

n To remove an electron from the $n = 2$ orbit, we merely substitute for a the
a′ value $a' = n^2 a$ with $n = 2$. This gives

$$E_2 = 0{\cdot}54 . 10^{-8} \text{ ergs} \quad \ldots\ldots(6'').$$

It is worth noting that the negative energies of the electron in the two states a and a' are $1{\cdot}54 . 10^{-8}$ and $0{\cdot}36 . 10^{-8}$ ergs respectively, so that the high kinetic energy of the alpha-particle renders the process of capture no more efficient than that of simple removal.

§ 5. MINIMUM VELOCITIES AND VOLTAGES

The energy of the alpha-particle with the above velocity is $1{\cdot}3 . 10^{-5}$ ergs, so that the above results show very definitely how much more probable is the capture, or removal, by an alpha-particle than by a proton of 25 kV. In fact the latter will have to give up nearly half its initial kinetic energy to the electron.

The above work tells us what values of E are necessary for the excitation process. Let us now suppose that we can obtain a safe maximum for the possible values of E by calculating them for head-on collision, neglecting the effect of electrical attraction, and assuming perfect elasticity. This method gives for the alpha-particle the value $E_{max} = 1{\cdot}4 . 10^{-8}$ ergs and for the proton the value $E_{max} = 9 . 10^{-11}$ ergs. Since the latter is of a much smaller order than the required value for the proton, we conclude that no mechanism could possibly be constructed which would enable protons of 25 kV to eject a K-electron from a Cu atom. For alpha-particles we may profitably reverse the argument: since X-rays of very slight intensity, due to alpha-particles, are actually observed, the efficiency of the mechanism in this case must be practically as great as that of head-on collision.

To estimate the minimum voltages required to give the proton sufficient energy to excite X-rays we must put our formulae in a more general form. Thus, for the general expression for the possible maximum value of E as a function of the velocity V of the incident particle we have the following equations:

$$MV = MV' + mV'',$$

$$MV^2 = MV'^2 + mV''^2,$$

V″ where $\frac{1}{2}mV''^2 = E_{max}$ defines V''.

These give both E_{max} and V', thus

$$V' = V(M - m)/(M + m),$$

$$V'' = 2V(M + m)^2/M^2,$$

or
$$E_{max} = 2mV^2(M + m)^2/M^2 \qquad \ldots\ldots(7).$$

We may now write (5) in the form

$$\tfrac{1}{2}MV^2 = 2mV^2[(M + m)/m](M + m)^2/M^2 - \tfrac{1}{2}(M/m)(e^2/a)(Z/n^2 - 1/Z)$$

or
$$\tfrac{1}{2}MV^2 = \tfrac{3}{2}(M/m)(e^2/a)(Z/n^2 - 1/Z) \text{ ergs} \qquad \ldots\ldots(8).$$

This gives the voltage as $7{\cdot}2 . 10^4$ kV., with $n = 1$.

For the alpha-particle we shall have

$$E_{max} = 2mV^2(4M + m)^2/(4M)^2 \qquad \ldots\ldots(7'),$$

and by (5)

$$2MV^2 = 2mV^2[(4M + m)/m](4M + m)^2/(4M)^2 - 2(M/m)(e^2/a)(Z/n^2 - 1/Z)$$

or
$$V = [(1/12m)(e^2/a)(Z/n^2 - 1/Z)]^{\frac{1}{2}} \qquad \ldots\ldots(8').$$

With $Z = 29$, for Cu targets, this gives the following results:

$$n = 1;\ V = 1{\cdot}65 . 10^9 \text{ cm./sec.},$$

$$n = 2;\ V = 0{\cdot}65 . 10^9 \text{ cm./sec., etc.}$$

which are normal values of the velocity of alpha-particles.

As another example, put $Z = 78$ for Pt; then we get

$$n = 1;\ V = 7 . 10^9 \text{ cm./sec.},$$

$$n = 2;\ V = 2 . 10^9 \text{ cm./sec., etc.},$$

so that the alpha-particles will hardly be able to excite K-radiation from a platinum target.

With regard to the head-on collisions it should be remarked that if we assume that superposed on the inverse-square attraction between positive and negative particles is an inverse-fourth-power repulsion, in accordance with Bieler's* deductions from experiments on the wide-angle scattering of alpha-particles by light nuclei, then it follows that such head-on collisions will be exactly equivalent to head-on collisions between perfectly elastic spheres, and the actual energy-transfer will in this case be accurately given by the above maximum values. It is thus quite permissible to suppose that the required energy-transfers are within the range of possibility.

Obviously the distance within which the proton and the electron must approach each other, in order to cause that repelling force to become effective, will be very much less than that required when two electrons collide. Consequently the approximately head-on collision between the proton and a K-electron will be less probable than that between the electron and either an incident electron, or a presumably larger alpha-particle.

* E. S. Bieler, *Proc. R. S.* A, **105**, 434 (1924).

§6. CONCLUSION

From further experimental data and by the help of the above theory we should be able to elucidate the mechanism of the process whereby the momentum is transferred to the ejected particle. Thus in the theory we have taken a safe maximum value for E, the kinetic energy transferred, and in practice we expect to find that the actual possible value is somewhat less than this; and the required velocities of the exciting particles will thus have to be greater than the calculated ones. The actual velocities as compared with the theoretical ones will then give a measure of the efficiency of the process of transfer.

Further, if the minimum velocities are found in practice to be sharp, or critical, then the momentum at transference must be quantized; if on the other hand the minima are gradual, or non-critical, as is suggested by the results of Bothe and Franz*, then the momentum is not quantized at transfer.

This paper does not of course pretend to be an exhaustive study, which would be premature until more experimental data are at hand; but it may perhaps suggest possibilities of treatment of the problem by simple methods that do not appear to have been employed before in this connection. We may also fairly claim to have shown that we do not need to wait for the further development of wave mechanics in order to obtain a satisfactory explanation of the phenomena so far observed.

* W. Bothe and H. Franz, *Zeit. für Phys.* **52**, 446 (1929).

DIFFUSION INTO AN INFINITE PLANE SHEET SUBJECT TO A SURFACE CONDITION, WITH A METHOD OF APPLICATION TO EXPERIMENTAL DATA

By A. T. McKAY, M.Sc.

Communicated by H. Bradley, Director of Research, The British Boot, Shoe and Allied Trades Research Association. Received February 21, 1930 *and in revised form May* 7, 1930. *Read in title June* 26, 1930.

ABSTRACT. The solution of the partial-differential diffusion equation for the infinite plane sheet is found by operational methods, subject to a surface condition analogous to Newton's law of cooling. By use of the fact that the area between the theoretical quantity/time curve, its asymptote and the quantity-axis is finite, a method of curve-fitting suitable to the case is suggested. To facilitate the application of the method to experimental data two numerical tables are appended.

§ 1. INTRODUCTION

The investigation with which this paper deals originated in the course of experimental research relating to the interchange of moisture between leather and the atmosphere, consequent upon changes in humidity. It has already been shown* that when a piece of leather is immersed in water this entry of moisture is very closely represented by a simple diffusion theory. Other workers† have found similar results for rubber sheets immersed in water.

When leather specimens which were in equilibrium with an atmosphere of fixed relative humidity were transferred into atmospheres of different humidity, the experimental curves obtained, showing a gain or loss of moisture against time, closely resembled those of the previous case and quite naturally were tested against the simple theory. Discrepancies between experimental results and the simple theory were manifested, however, and were of such a magnitude as could not be attributed to errors of observation. It was necessary to try to formulate a new theory to be tested against experiment. Retaining the hypothesis that the mechanism of moisture movement in the leather is that of diffusion, we were led to contemplate the influence of the surface in retarding the flow.

Analogy with heat problems‡ suggested the examination of the solution of the partial-differential diffusion equation for the infinite plane sheet subject to surface conditions expressed by the analogue of Newton's law of cooling. The problem

* H. Bradley, A. T. McKay and B. Worswick, *Journ. International Society of Leather Trades' Chemists*, **13**, 87–105 (1929).

† D. H. Andrews and I. Johnstone, *Journ. American Chem. Soc.* **46**, 640–650 (1924).

‡ Preston, *Theory of Heat*, ch. 7 (1924).

is not new in the mathematical sense*† but for the present purpose it has to be considered in a rather unusual way, owing to the fact that experiment merely provides a measure of quantity, whereas in the case of heat investigations temperature is recorded. It is hoped that the study of this problem by means of the operational methods of Heaviside‡, extended by Bromwich§, Jeffreys‖ and others, will be of interest to physicists. To enhance the utility of the work a method is put forward whereby numerical values of the constants are determined from the experimental curve, so that a theoretical curve may be plotted therefrom to test the fit of the theory. This procedure is perhaps worthy of note for it suggests the employment of a similar device when it is necessary to fit data to such a complicated equation as the one in question. To facilitate the practical analysis of data on the lines of the present theory two numerical tables are appended.

§ 2. STATEMENT OF PROBLEM AND DEFINITION OF SYMBOLS

An infinite plane sheet of hygroscopic material, initially in equilibrium with an atmosphere of given relative humidity, is placed in an atmosphere of different relative humidity. Supposing that the absorption is expressed by the usual partial-differential diffusion equation together with the surface condition given by the analogue of Newton's law of cooling, we have to find the manner in which the sheet attains equilibrium with the different humidity.

The term "concentration" is to be taken as the excess of moisture present, per unit volume, over that contained initially. Most of the subsequent work will not be affected by the supposition that the quantity of moisture in a lamina of thickness τ is equal to τ multiplied by the concentration in the lamina.

The following symbols are used:

t — t, time measured from the instant when the sheet is placed in the atmosphere of higher humidity.

x — x, distance from the central section, measured perpendicularly to the plane of the sheet.

Q — Q, quantity of moisture in the sheet, per unit area of face, in excess of that contained initially.

Q_m — Q_m, maximum value of Q, i.e. the equilibrium moisture content of the specimen at the higher humidity, less the equilibrium moisture content at the lower humidity.

θ_a — θ_a, concentration at the surface at time t.

θ — θ, concentration at plane x at time t.

θ_m — θ_m, equilibrium value of θ corresponding to Q_m.

* W. E. Byerly, *An Elementary Treatise on Fourier Series and Spherical Harmonics*, pp. 117–121 (1893).

† H. S. Carslaw, *Phil. Mag.* **39**, 603–611 (1920).

‡ Oliver Heaviside, *Electromagnetic Theory*.

§ T. J. I'A. Bromwich, *Proc. Lond. Math. Soc.* (2), **15**, 401–448 (1916); *Phil. Mag.* (6), **37**, 407–419 (1919); *Proc. Camb. Phil. Soc.* **20**, 411–427 (1921).

‖ H. Jeffreys, *Cambridge Tracts in Mathematics*, No. 23.

$2a$, thickness of sheet.	$\gamma \equiv 2k/ca.$	a, γ
k, diffusivity constant.	$p \equiv \frac{d}{dt}.$	k, p
c, surface constant.	$\sigma^2 \equiv p/k.$	c, σ

§ 3. SOLUTION OF THE DIFFERENTIAL EQUATION

We require to solve the differential equation

$$\frac{\partial^2\theta}{\partial x^2} = \frac{1}{k}\frac{\partial\theta}{\partial t} \qquad \text{.....(1),}$$

subject to the conditions

$$\left.\begin{aligned} &(a)\ \text{Symmetry with respect to } x \\ &(b)\ \frac{dQ}{dt} = c\,(\theta_m - \theta_a) \end{aligned}\right\} \qquad \text{.....(2).}$$

The requisite solution could, of course, be built up by the usual device of writing $\theta = \theta_m e^{Ax+Bt}$ and expanding unity as a generalized Fourier series*. This method does not commend itself for the present purposes, so we shall make use of the operative methods of Heaviside and Bromwich†. The equation (1) may be written

$$\frac{\partial^2\theta}{dx^2} = \sigma^2\theta \qquad \text{.....(3).}$$

Solving this as an ordinary differential equation and choosing in accordance with condition ($2a$) the solution symmetrical in x, we have

$$\theta = A\cosh\sigma x \qquad \text{.....(4),}$$

where A is independent of x but involves σ. Further, *A*

$$Q = 2\int_0^a \theta\,dx = 2A\sinh\sigma a/\sigma.$$

Hence, using condition ($2b$) and writing γ for $2k/ca$, we find

$$A = \theta_m/(\gamma\sigma a\sinh\sigma a + \cosh\sigma a).$$

Substituting this in equation (4) we arrive at the operational solution

$$\theta = \theta_m\cosh\sigma x/(\gamma\sigma a\sinh\sigma a + \cosh\sigma a) \qquad \text{.....(5).}$$

The operator on the right-hand side of equation (5) now requires interpretation, and we could effect this by expanding the function of σ in partial fractions, combining pairs of terms and making use of the fact that $1/(p+h) = (1-e^{-ht})/h$. We shall not, however, do this directly, but shall employ the rule due to Bromwich‡, which states that provided $f(z)$ is an analytic function of z, then

$$f(p) = \frac{1}{2\pi i}\int_L e^{tz} f(z)\,\frac{dz}{z},$$

where L is a curve in the z-plane, and extends from $\xi - i\infty$ to $\xi + i\infty$ (where ξ is positive and finite) and is such that all the singularities of the integrand are on the

* W. E. Byerly, *loc. cit.* † *Loc. cit.* ‡ T. J. I'A. Bromwich, *loc. cit.*

$-\infty$ side of the path (see Fig. 1). Recalling that $\sigma = p^{\frac{1}{2}}/k^{\frac{1}{2}}$ and applying the above rule, we find

$$\theta = \frac{\theta_m}{2\pi i}\int_L \frac{e^{kzt/a^2}\cosh(xz^{\frac{1}{2}}/a)}{\{(\gamma z^{\frac{1}{2}})\sinh z^{\frac{1}{2}} + \cosh z^{\frac{1}{2}}\}}\frac{dz}{z} \qquad \text{.....(6),}$$

whence, by integration with respect to x, and noting that $Q_m = 2a\theta_m$, we find

$$Q = \frac{Q_m}{2\pi i}\int_L \frac{e^{kzt/a^2}}{(\gamma z + z^{\frac{1}{2}}\coth z^{\frac{1}{2}})}\frac{dz}{z} \qquad \text{.....(7).}$$

Equations (6) and (7) give the complete solution of the differential equation in terms of contour integrals. The integrand on the right-hand side of equation (7) is a single-valued function of z with poles at $z = 0$, $z = -\beta_n^2$, where $n = 0, 1, 2$, etc., and β_n is the nth positive root of the transcendental trigonometric equation

β_n

$$\cot y = \gamma y \qquad \text{.....(8).}$$

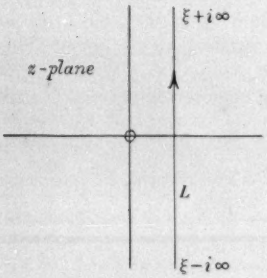

Fig. 1.

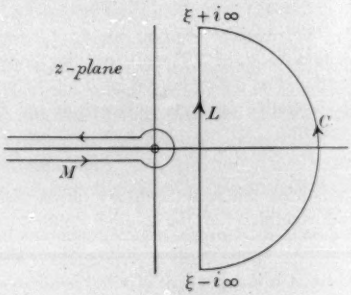

Fig. 2.

This equation also arises in connection with the vibrations of a loaded string* as well as in the process of expanding a function as a generalized Fourier series, where the roots of equation (8) play a part similar to that of multiples of $\pi/2$ in the ordinary Fourier expansions†. Some roots of this equation have been given by Schlömilch‡, and Jahnke and Emde§ give a short table and a graph from which values of the first root (β_1) can be obtained. The other roots can be obtained most accurately by the use of iterative methods or of inverse interpolation‖.

Returning now to equation (7), the coefficient of z is always positive or zero and the remaining term in the integrand approaches zero uniformly with respect to $\arg z$ as $|z| \to \infty$ with a possible exception when $-\pi + \epsilon \leqslant \arg z \leqslant \pi - \epsilon$, where ϵ is arbitrarily small; hence by a slight extension of Jordan's lemma¶ the path L is equivalent to the path M (see Fig. 2).

* H. Lamb, *The Dynamical Theory of Sound*, pp. 82–84 (1910). † W. E. Byerly, *loc. cit.*
‡ Schlömilch, *Übungsbuch*, **1**, 354.
§ E. Jahnke and F. Emde, *Funktionentafeln mit Formeln und Kurven*, pp. 1–2 (1928).
‖ E. T. Whittaker and G. Robinson, *The Calculus of Observations*, pp. 81–84 (1924).
¶ E. T. Whittaker and G. N. Watson, *A Course of Modern Analysis*, p. 115, 3rd ed. (1920).

Hence, using the principle of residues, we easily find

$$\theta = \theta_m \left[1 - \sum_1^\infty \frac{2e^{-k\beta_n^2 t/a^2} \cos(x\beta_n/a)/\sin\beta_n}{\beta_n^2(1+\gamma+\gamma^2\beta_n^2)} \right] \quad \text{.....(9)},$$

$$Q = Q_m \left[1 - \sum_1^\infty \frac{2e^{-k\beta_n^2 t/a^2}}{\beta_n^2(1+\gamma+\gamma^2\beta_n^2)} \right] \quad \text{.....(10)}.$$

We shall find it convenient to write $2/\beta_n^2(1+\gamma+\gamma^2\beta_n^2) = b_n$. It is evident that equations (9) and (10) completely satisfy the conditions laid down and are therefore the solutions sought. b_n

§ 4. DETERMINATION OF MOMENTS

The curve represented by equation (10), together with its asymptote and the quantity axis, bounds a finite area. We shall refer to this as "the area above the curve."

Let μ_n be the nth positive moment per unit Q_m about the quantity axis, of the area above the curve. It does not seem possible to find these moments by direct integration of equation (10), so we proceed as follows: μ_n

From equation (7)

$$\mu_n = \int_0^\infty \left(1 - \frac{Q}{Q_m}\right) t^n . dt = \frac{1}{2\pi i} \int_0^\infty \int_L e^{kzt/a^2} t^n \frac{f(z)}{z} . dz . dt,$$

where $f(z) = 1 - 1/(\gamma z + z^{\frac{1}{2}} \coth z^{\frac{1}{2}})$.

Hence $$\mu_n = \frac{a^2}{2\pi i k} \left[\int_L e^{kzt/a^2} \left(t^n - \frac{na^2}{k} \frac{t^{n-1}}{z} + \dots \right) f(z) . \frac{dz}{z^2} \right]_{t=0}^{t=\infty} \quad \text{.....(11)}.$$

Lower limit. When $t = 0$ the expression in brackets in equation (11) vanishes. The integral is a multiple of $\int_L \frac{f(z)}{z^{n+2}} . dz$. Since $f(z)/z^{n+2}$ is regular on and within the contour LC (see Fig. 2) the path L is equivalent to the path C. Now $|f(z)/z^{n+1}| \to 0$ uniformly with respect to $\arg z$ for all points on C as the radius of $C \to \infty$: consequently the integral vanishes.

Upper limit. We consider the path L to be replaced by the path M as before, and note that when t is large the exponential term is only appreciable in the immediate vicinity of the point $z = 0$. We can therefore replace the path M by a closed contour of small dimensions surrounding the origin. Evaluating the residue of the integrand at $z = 0$ we find that when t is large the right-hand side of equation (11) is given by

$$a^2/k[(1-n)(\gamma+\tfrac{1}{3})t^n + na^2/k\{\tfrac{1}{45} + (\gamma+\tfrac{1}{3})^2\}t^{n-1} + \text{terms which vanish when } n = 0 \text{ or } 1].$$

Now plainly we must give n its numerical value before we proceed to the limit, so that

$$\left.\begin{aligned} \mu_0 &= (a^2/k)(\gamma+\tfrac{1}{3}) \\ \mu_1 &= (a^4/k^2)[\tfrac{1}{45} + (\gamma+\tfrac{1}{3})^2] \end{aligned}\right\} \quad \text{.....(12)}.$$

These two expressions are, of course, found to agree with those otherwise deduced for the special cases when $C \to \infty$ (simple diffusion) or $k \to \infty$ (simple exponential).

§ 5. SPECIAL CASES OF THE GENERAL EQUATION

Simple exponential. When the diffusivity is very great (theoretically infinite) the absorption is determined solely on the basis of condition (2*b*) from which we have

$$Q = Q_m\,(1 - e^{-ct/2a}) \qquad \text{.....(13).}$$

Table 1. The simple diffusion function.

Values of the function $\psi(v)$, or $[1 - (8/\pi^2)\,(e^{-v} + \frac{1}{9}e^{-9v} + \frac{1}{25}e^{-25v} + \ldots)]$.

v	0·00	0·01	0·02	0·03	0·04	0·05	0·06	0·07	0·08	0·09
0·00	0·0000	0·0718	0·1016	0·1244	0·1437	0·1606	0·1760	0·1901	0·2032	0·2155
0·10	0·2272	0·2382	0·2488	0·2590	0·2688	0·2782	0·2873	0·2962	0·3048	0·3131
0·20	0·3213	0·3292	0·3369	0·3445	0·3519	0·3592	0·3663	0·3733	0·3801	0·3868
0·30	0·3934	0·4000	0·4064	0·4127	0·4188	0·4249	0·4309	0·4368	0·4427	0·4485
0·40	0·4542	0·4598	0·4654	0·4708	0·4763	0·4816	0·4869	0·4921	0·4972	0·5024
0·50	0·5074	0·5123	0·5173	0·5221	0·5270	0·5318	0·5364	0·5411	0·5457	0·5503
0·60	0·5548	0·5592	0·5637	0·5681	0·5723	0·5766	0·5808	0·5850	0·5892	0·5932
0·70	0·5973	0·6014	0·6053	0·6093	0·6132	0·6170	0·6208	0·6246	0·6284	0·6321
0·80	0·6357	0·6393	0·6430	0·6466	0·6501	0·6536	0·6570	0·6604	0·6638	0·6671
0·90	0·6704	0·6737	0·6770	0·6801	0·6834	0·6866	0·6896	0·6827	0·6958	0·6988
1·00	0·7018	0·7048	0·7077	0·7106	0·7135	0·7164	0·7191	0·7220	0·7249	0·7275
1·10	0·7302	0·7328	0·7355	0·7382	0·7408	0·7434	0·7459	0·7484	0·7509	0·7534
1·20	0·7559	0·7583	0·7607	0·7631	0·7654	0·7678	0·7700	0·7724	0·7747	0·7769
1·30	0·7791	0·7813	0·7835	0·7856	0·7878	0·7899	0·7919	0·7940	0·7961	0·7981
1·40	0·8001	0·8021	0·8041	0·8060	0·8080	0·8098	0·8118	0·8136	0·8155	0·8173
1·50	0·8192	0·8209	0·8227	0·8245	0·8262	0·8280	0·8297	0·8314	0·8330	0·8347
1·60	0·8364	0·8380	0·8396	0·8412	0·8427	0·8444	0·8459	0·8475	0·8489	0·8504
1·70	0·8519	0·8534	0·8548	0·8563	0·8577	0·8591	0·8606	0·8620	0·8633	0·8646
1·80	0·8660	0·8673	0·8687	0·8700	0·8713	0·8726	0·8738	0·8751	0·8763	0·8775
1·90	0·8787	0·8800	0·8812	0·8824	0·8835	0·8847	0·8858	0·8869	0·8881	0·8892
2·00	0·8903	0·8914	0·8924	0·8936	0·8946	0·8957	0·8967	0·8977	0·8988	0·8997
2·10	0·9007	0·9018	0·9027	0·9037	0·9046	0·9056	0·9065	0·9074	0·9084	0·9093
2·20	0·9102	0·9111	0·9120	0·9129	0·9137	0·9146	0·9154	0·9163	0·9171	0·9179
2·30	0·9187	0·9195	0·9203	0·9211	0·9219	0·9227	0·9235	0·9242	0·9249	0·9258
2·40	0·9265	0·9272	0·9279	0·9287	0·9293	0·9300	0·9308	0·9314	0·9322	0·9328
2·50	0·9335	0·9341	0·9347	0·9354	0·9360	0·9367	0·9373	0·9380	0·9386	0·9392
2·60	0·9398	0·9404	0·9410	0·9416	0·9421	0·9427	0·9433	0·9438	0·9444	0·9450
2·70	0·9455	0·9461	0·9466	0·9472	0·9476	0·9482	0·9487	0·9492	0·9497	0·9502
2·80	0·9507	0·9512	0·9517	0·9522	0·9527	0·9531	0·9536	0·9540	0·9545	0·9549
2·90	0·9554	0·9558	0·9563	0·9567	0·9571	0·9576	0·9580	0·9584	0·9588	0·9592
3·00	0·9596	0·9600	0·9604	0·9608	0·9613	0·9616	0·9620	0·9624	0·9627	0·9631
3·10	0·9635	0·9639	0·9643	0·9646	0·9650	0·9653	0·9656	0·9660	0·9664	0·9667
3·20	0·9669	0·9673	0·9676	0·9679	0·9682	0·9685	0·9689	0·9692	0·9694	0·9698
3·30	0·9702	0·9705	0·9707	0·9711	0·9713	0·9716	0·9719	0·9722	0·9724	0·9728
3·40	0·9729	0·9732	0·9735	0·9737	0·9740	0·9742	0·9745	0·9748	0·9750	0·9753
3·50	0·9755	0·9758	0·9760	0·9763	0·9765	0·9767	0·9770	0·9772	0·9774	0·9776

Simple diffusion. This is the term applied to the special case wherein the surface of the sheet instantaneously becomes saturated at the start. This occurs when the surface constant is very great, in which case the roots of equation (8) are

$$\beta_n = (2n - 1)\,\pi/2$$

and equation (10) reduces to

$$Q = Q_m\,[1 - (8/\pi^2)\,(e^{-\pi^2 kt/4a^2} + \tfrac{1}{9}e^{-9\pi^2 kt/4a^2} + \ldots)],$$

ψ, v or if $\psi(v) = [1 - (8/\pi^2)\,(e^{-v} + \frac{1}{9}e^{-9v} + \ldots)],$

$$Q = Q_m\,\psi(\pi^2 kt/4a^2) \qquad \text{.....(14).}$$

If, in equation (7), we write $t = 4a^2/\pi^2 k$ and $\gamma = 0$, we find

$$\psi(v) = \frac{1}{2\pi i}\int_L e^{\frac{4}{\pi^2}vz}\frac{\tanh z^{\frac{1}{2}}}{z^{\frac{1}{2}}}\frac{dz}{z};$$

now writing $q^2 \equiv d/dv$ this latter integral can be re-interpreted as an operator by q
using the Bromwich rule inversely, so that we get

$$\psi(v) = \tanh(\pi q/2)/(\pi q/2).$$

It is possible to show* that this last expression can be expanded provided we interpret $q^{2\alpha} = v^{-\alpha}/\Gamma(1-\alpha)$. When v is small q is therefore of order $(\pi v)^{-\frac{1}{2}}$, whence

$$\psi(v) = 2/\pi q \text{ when } v \text{ is small, or}$$
$$= 4v^{\frac{1}{2}}/\pi^{\frac{3}{2}}.$$

This latter is a very good approximation up to $v = 0{\cdot}5$ which is roughly as far as $\psi(v) = 0{\cdot}5$. The error when $v = 0{\cdot}5$ is less than 3 in 5000, and for lower values of v is smaller still. Table 1 gives values of $\psi(v)$ from $v = 0$ to $v = 3{\cdot}50$ at intervals of $0{\cdot}01$. Intermediate values in the lower part of the range should be determined by the above approximation, as direct interpolation is not very reliable.

§ 6. EQUATION CONSTANTS AND CURVE CRITERION

Returning to the equations (12), we find that

$$\text{Diffusivity } k = a^2/3\,(5\mu_1 - 5\mu_0^2)^{\frac{1}{2}} \qquad \ldots\ldots(15),$$

$$\text{Surface constant} = 2k/a\gamma = 2a/[\mu_0 - (5\mu_1 - 5\mu_0^2)^{\frac{1}{2}}] \qquad \ldots\ldots(16).$$

We now define the following curve criterion:

$$R = \mu_1/\mu_0^2,$$

so that $$1 \leqslant R \leqslant 1{\cdot}2$$

is a necessary condition attaching to equation (10). $R = 1$ corresponds to the R
special case of equation (13), and $R = 1{\cdot}2$ corresponds to equation (14). Further, we note that

$$\gamma = \tfrac{1}{3}\,[1/(5R-5)^{\frac{1}{2}} - 1],$$

so that the roots of equation (8) and the coefficients (b_n) of the exponential terms in the general equation are merely functions of R. The initial slope per unit Q_m is given by

$$\text{Initial slope} \div Q_m = 1/\mu_0\,[1 - (5R-5)^{\frac{1}{2}}] \geqslant 1/\mu_0.$$

Table 2 gives values of (γ), $(\mu_0 k/a^2)$, (b_1), $(\mu_0 k\beta_1^2/a^2)$ for a series of values of R, and these are usually sufficient to enable a large portion of the theoretical curve to be plotted and the constants to be determined when μ_0, μ_1, Q_m are known.

* W. E. Sumpner, *Proc. Phys. Soc.* **41**, 405–425 (1929).

§7. EXAMINATION OF EXPERIMENTAL DATA

We have now to consider how the foregoing theory can be utilized to study data, the general trend of which is not at variance with the form of the diffusion curves contemplated herein.

Table 2. Quantities for use in curve-fitting.

Criterion R	γ	$\mu_0 k/a^2$	b_1	$\mu_0 k \beta_1^2/a^2$
1·000	∞	∞	1·0000	1·0000
1·005	1·7749	2·1082	0·9948	0·9947
1·010	1·1574	1·4907	0·9891	0·9894
1·015	0·8838	1·2172	0·9833	0·9841
1·020	0·7208	1·0541	0·9774	0·9789
1·025	0·6095	0·9428	0·9716	0·9737
1·030	0·5273	0·8607	0·9658	0·9685
1·035	0·4635	0·7968	0·9600	0·9633
1·040	0·4120	0·7454	0·9543	0·9582
1·045	0·3694	0·7027	0·9487	0·9531
1·050	0·3333	0·6667	0·9431	0·9480
1·055	0·3023	0·6356	0·9376	0·9430
1·060	0·2752	0·6086	0·9322	0·9380
1·065	0·2514	0·5847	0·9268	0·9331
1·070	0·2301	0·5634	0·9215	0·9283
1·075	0·2110	0·5443	0·9162	0·9236
1·080	0·1937	0·5270	0·9111	0·9189
1·085	0·1780	0·5113	0·9060	0·9142
1·090	0·1636	0·4969	0·9010	0·9096
1·095	0·1503	0·4837	0·8960	0·9051
1·100	0·1381	0·4714	0·8913	0·9006
1·105	0·1267	0·4600	0·8866	0·8962
1·110	0·1161	0·4495	0·8819	0·8919
1·115	0·1062	0·4396	0·8774	0·8875
1·120	0·0970	0·4303	0·8730	0·8832
1·125	0·0883	0·4216	0·8685	0·8790
1·130	0·0801	0·4134	0·8641	0·8749
1·135	0·0724	0·4057	0·8597	0·8709
1·140	0·0651	0·3984	0·8556	0·8669
1·145	0·0581	0·3915	0·8515	0·8630
1·150	0·0516	0·3849	0·8475	0·8591
1·155	0·0453	0·3786	0·8435	0·8552
1·160	0·0393	0·3727	0·8395	0·8513
1·165	0·0337	0·3670	0·8356	0·8476
1·170	0·0282	0·3616	0·8319	0·8439
1·175	0·0230	0·3563	0·8282	0·8402
1·180	0·0180	0·3513	0·8245	0·8365
1·185	0·0133	0·3466	0·8210	0·8329
1·190	0·0086	0·3420	0·8175	0·8294
1·195	0·0042	0·3376	0·8140	0·8260
1·200	0·0000	0·3333	0·8106	0·8225

To make a test of fit it is necessary to find Q_m and the moments μ_0 and μ_1. There seems to be no entirely satisfactory way of determining a reliable estimate of Q_m otherwise than by experiments carried out under uniform conditions for a sufficiently long period. Further, if the readings are taken at equidistant intervals of time, the determination of μ_0 and μ_1 is much simplified, for the numerical integration can rapidly be effected by using Gregory's formula*, or any other similar formula†. If it is impracticable to take observations at equal intervals of

* E. T. Whittaker and G. Robinson, *loc. cit. ante*, p. 143.

† A. Henry, *Calculus and Probability for Actuarial Students*, pp. 114–124 (1927).

time, there is nothing for it but to pass a smooth curve through the experimental points. It is not satisfactory, generally, to do this graphically and the only really good method is to employ Lagrange's formula* for all or a selected number of the observations. Having obtained μ_0 and μ_1 to the same degree of accuracy as the experimental data are recorded, we can evaluate the criterion R. Now we have seen in a previous paragraph that a necessary though not sufficient condition for a fitting by the general curve (10) is that $1 < R < 1{\cdot}2$, so if the calculated R lies outside this range no fit is possible and one or more of the components of the general hypothesis, as given in the statement of the problem, must be invalid. On the other hand, a value of R within the above range implies the possibility of fit; we can only say "possibility" for conceivably there can be many curves yielding a plausible value for R yet differing considerably from equation (10). To complete the test of fit it is necessary to plot a theoretical curve and examine how much the experimental points diverge therefrom. The values of μ_0, μ_1, Q_m being known, the only plausible values of the diffusivity and surface constants are given by equations (15) and (16). Table 2 has been prepared to expedite the calculation of the numerical quantities in the first term of equation (10).

§ 8. ACKNOWLEDGMENT

In conclusion, I should like to express my thanks to the Council of the British Boot, Shoe and Allied Trades Research Association, in whose laboratories this work was done, for permission to publish the paper.

* E. T. Whittaker and G. Robinson, *loc. cit. ante*, pp. 28–30.

INTENSITY MODIFICATIONS IN THE SPECTRUM OF MERCURY

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ABSTRACT. Intensity modifications are noted in the high-frequency spectrum of mercury in the region $\lambda 7000$ to $\lambda 2400$. All the singlet series are strengthened. In the triplets the first members of the series are weakened by self-absorption. Those intercombination lines which begin on singlet levels are strengthened, whilst those which begin on triplet levels are weakened. The unclassified lines show three types of behaviour, namely strengthening, weakening, or freedom from change. It is suggested that the strengthened unclassified lines begin on singlet levels, and the weakened on triplet levels, and that the unchanged lines are due to displaced triplet levels in the excited atom. The unclassified lines are thus broadly divided. The degree of intensity modification can be made to vary by alteration of the current density in the discharge tube. Mercury films are produced by the discharge driving mercury into the walls of the tube. These are described and suggestions made as to their nature.

The line $6\,^1P_1 - 8\,^1S_0$, namely $\lambda 4916$, is examined for fine structure with a resolving power of over 3×10^6. It is quite single and very narrow, the half-width being less than 0·004 A.U. The intensities of the satellites in $\lambda 5461$ are found to be altered by self-absorption.

§ 1. INTRODUCTION

Intensity modifications in the high-frequency discharge in mercury were first observed by Clarke*, who recorded a relative strengthening of a small group of lines. Ponte†, using one internal electrode and exciting the mercury vapour with oscillations corresponding to wave-lengths of from 5 to 10 metres, found that the two singlet series $6\,^1P_1 - m\,^1S_0$, $6\,^1P_1 - m\,^1D_2$ and a few intercombination lines increased in intensity. Frayne and Montgomery‡, using an electrodeless discharge (3 metres), passing through a solenoid wound round a pyrex tube, extended these observations. Observing the region $\lambda 7000$ to $\lambda 3000$ they found an increase in strength, relative to $\lambda 5461$, in all the singlet series in this region, namely $6\,^1P_1 - m\,^1S_0$, $6\,^1P_1 - m\,^1D_2$ and $7\,^1S_0 - m\,^1P_1$, and also in a group of intercombination lines and in three unclassified lines. Intensities were measured with a photometer and compared with those in the arc spectrum.

In §§ 2 to 4 of the present paper, observations are extended from $\lambda 7000$ to $\lambda 2400$. The observations of Frayne and Montgomery on the singlet series are

* J. R. Clarke, *Nature*, **120**, 726 (1927).
† M. Ponte, *Nature*, **121**, 243 (1928).
‡ J. G. Frayne and C. G. Montgomery, *Phys. Rev.* **33**, 549 (1929).

confirmed and in addition many more intercombination and unclassified lines are observed. Some are strengthened and some weakened in the high frequency discharge, whilst some unclassified lines remain unaltered. Self-absorption is found to affect the first members of triplet series, reducing their intensities. Amongst the lines so affected is $\lambda 5461$, so that the next member of this series, $\lambda 3341$, had to be used as the comparison line for intensities. In § 5 the lines $\lambda 4916$ and $\lambda 5461$ are examined with high resolving power for fine structure.

§ 2. EXPERIMENTAL PROCEDURE

Mercury was distilled into small tubes of quartz and pyrex having central capillary portions. They were about 10 cm. long with a 2 cm. bore, the capillary being 1 cm. long with a bore of about 0·25 cm. A good vacuum was produced by means of a mercury vapour pump and charcoal in liquid air. Round the wider parts were wrapped strips of tin-foil, which were coupled to a 7-metre oscillator. The circuit used (due to Gill and Donaldson*) is shown in Fig. 1. *V* is a Mullard

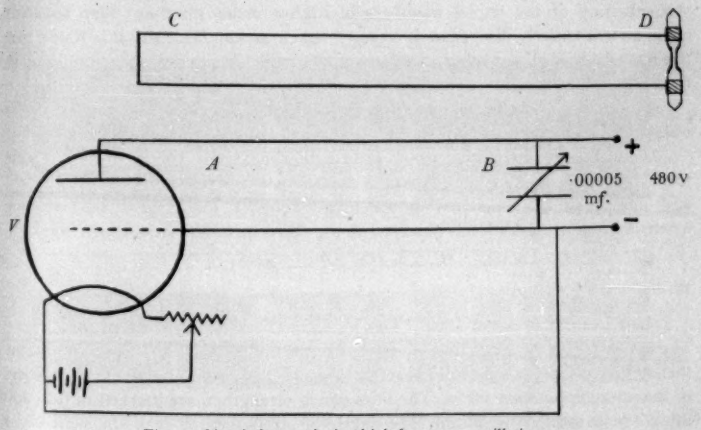


Fig. 1. Circuit for producing high frequency oscillations.

D.O. 40 valve, six volts being required for the filament. A simple Lecher wire system *AB*, about a metre long, with a tuning condenser is employed. A pair of wires *CD* of variable length is coupled inductively to *AB*, the ends being connected to the tin-foil on the tube. The discharge is brightest for a given length of *CD*. The plate voltage used was 480, the total input being about 40 watts. Full constructional details will be published at an early date by Mr S. F. Evans.

Observations were carried out on the capillary portion. The spectrum was photographed in the region $\lambda 7000$ to $\lambda 2400$ on both a Hilger small quartz and a

* E. W. B. Gill and R. H. Donaldson, *Phil. Mag.* **2**, 129 (1926).

large quartz E.I. spectrograph with Ilford Special Rapid Panchromatic and Ilford Monarch plates. The comparison spectrum was that emitted by a Kromayer water-cooled mercury arc taking 3 amps. Different exposure times, varying from one second to half an hour, were given. The high-frequency and arc spectra, for which $\lambda 3341$ was of about the same intensity, were matched for intensity comparisons.

§ 3. RESULTS

The colour of the discharge is very different from that of the arc, being an intense blue. On comparison of the high-frequency and arc spectra it is found that the singlet series $6\,^1P_1 - m\,^1S_0$, $6\,^1P_1 - m\,^1D_2$ and $7\,^1S_0 - m\,^1P_1$ are strengthened, see Plate I (*c*). In the triplet series, the first members only are affected, being reduced in intensity. Thus the first members of $6\,^3P_{012} - m\,^3S_1$, namely $\lambda\lambda 4046$, 4358, 5461, are considerably weakened with respect to the other members of the series. The diminution in $\lambda 5461$ is very marked, see Plate I (*b*). These intensity reductions were not recorded by Frayne and Montgomery owing to the fact that they took $\lambda 5461$ as the comparison line. The result was that they found an apparent strengthening of the triplet members of higher order number. This apparent effect almost entirely disappears if $\lambda 3341$ is taken as standard, and it is found that the intensities of all the other members of the triplet series are about the same in the arc and high-frequency discharges.

A reduction in intensity of the first member of the series $6\,^3P_0 - m\,^3D_1$ also is observed, but the effect is smaller than in $6\,^3P_{012} - m\,^3S_1$.

About twenty intercombination lines extending from $\lambda 6070$ to $\lambda 2537$ were observed. These lines form two distinct classes, namely those that are strengthened and those that are weakened in the high-frequency discharge. The following intercombination series (each observed to the third member) are strengthened:

$$6\,^3P_1 - m\,^1D_2,\ 6\,^3P_2 - m\,^1D_2,\ 6\,^3P_1 - m\,^1S_0,\ 7\,^3S_1 - m\,^1P_1,$$

whilst the following are weakened:

$$6\,^1P_1 - m\,^3D_2,\ 6\,^1P_1 - m\,^3D_1 \text{ and } 6\,^1P_1 - m\,^3S_0.$$

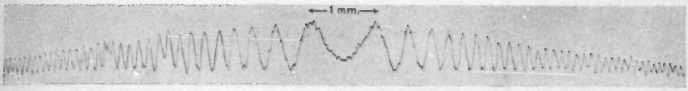
The line $\lambda 2537$ ($6\,^1S_0 - 6\,^3P_2$) is very much stronger in the high-frequency spectrum than in the arc. Fig. 2 is a term scheme showing the transitions for the first members of the intercombination series. The lines which strengthen are marked in full, and those which weaken are dotted. The degree of strengthening is of the same order as that of the singlet series.

There is a very marked difference in behaviour amongst the unclassified lines, and this should afford some clue to their classification. Some appear not to be affected, or at most to be affected very slightly. A number are strengthened, and a number are considerably reduced in intensity, the lines in fact disappearing as far as can be observed from the high-frequency spectrum. The strengthened lines lie mostly in the ultra-violet region. Amongst these are

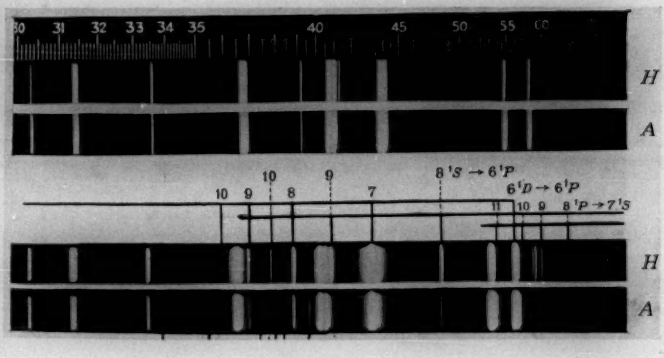
$$\lambda\lambda 2576,\ 2602,\ 2625,\ 2638,\ 2675.$$

In addition the red line $\lambda 6123$ is very strong.

There is a particularly striking group in the region $\lambda 3390$ to $\lambda 3984$ which seems to disappear entirely from the high-frequency spectrum. Its members are all of



(*a*)



(*b*)

(*c*)

(*a*) Photometer trace of Fabry-Perot fringes of λ4916, plate separation 10 cm.; (*b*) and (*c*) high-frequency spectrum of mercury (*H*) and arc spectrum of mercury (*A*).

similar intensity and may be related. They are shown in Plate I (*c*) and are marked with a dash. The members of this group are λλ3390, 3561, 3752, 3790, 3824, 3984.

The degree of enhancement could be made to vary considerably by alteration of the current density through the discharge tubes. This was achieved by loosening of the coupling and reduction of the filament current in the oscillator. With tight coupling and normal current the discharge was extremely bright. The capillary became warm although the bulbs remained cold. The spectrum showed all the characteristics described. On reduction of the current the total intensity fell off markedly, but the relative strengthening became greater. The reduction in intensity of $6\,^3P_{012} - 7\,^3S_1$ still appeared. The spectra reproduced in Plate I were taken with full current through the tubes.

The discharge was found to affect the mercury in the tube, driving it very forcibly into the walls. After some twenty hours' running the whole glass, excepting the parts under the electrodes, was covered with a practically opaque film of mercury which showed interference colours, violet predominating. The colours only appeared by reflected light, the film being a dirty brown when seen by transmitted light. The coloration of the film may not be the simple interference effect due to thin films, since its behaviour was different. After an hour's running there appeared a thin film which gave a brilliant violet colour by reflected daylight. This colour was independent of the angle of incidence. On further running the film thickened, becoming more opaque. The reflected colour still remained the same. On still further running new colours appeared, but the original violet was still strong. The phenomenon bore a striking resemblance to the optical resonance of thin films of sodium when viewed by transmitted light. The violet colour may therefore be due to optical resonance of mercury, the later colours being true thin-film interference colours arising as the film thickened. The extent to which the film was driven into the walls is shown by the fact that it was not removed by subsequent heating, even at a dull red heat.

§ 4. DISCUSSION OF RESULTS

There are three types of lines: strengthened, weakened, and unchanged (or not appreciably affected). The strengthened lines will be considered first. These consist of (*a*) the singlet series; (*b*) those intercombination lines which begin in singlet levels, see Fig. 2; and (*c*) certain of the unclassified lines. The line λ2537 is the only line beginning on a triplet level which is strengthened. This exception is explained later. The factor common to the first two groups (*a*) and (*b*) is that they begin in singlet levels. It is thus reasonable to expect that the unclassified lines which are strengthened also begin on singlet levels.

The weakened lines are (*a*) intercombination lines beginning in triplet levels, see Fig. 2; (*b*) certain of the unclassified lines; (*c*) the first members of the $^3P - {}^3S$ and $^3P - {}^3D$ series. Those unclassified lines which are weakened may possibly originate on triplet levels. The weakening of the first members of the triplet series is explained by the results obtained by Ponte*, who passed the light from a Cooper-

* M. Ponte, *Comptes Rendus*, **187**, 37 (1928).

Hewitt mercury arc through a tube containing mercury at 100° C., excited by a high-frequency discharge. He found that the singlet series were hardly affected but that members of the series $6\,^3P_{012} - m\,^3S_1$ and $6\,^3P_{012} - m\,^3D_{123}$ were absorbed. The effect was greater for the $^3P - {}^3S$ series, and for each series the first member was much more strongly absorbed than members of higher order number.

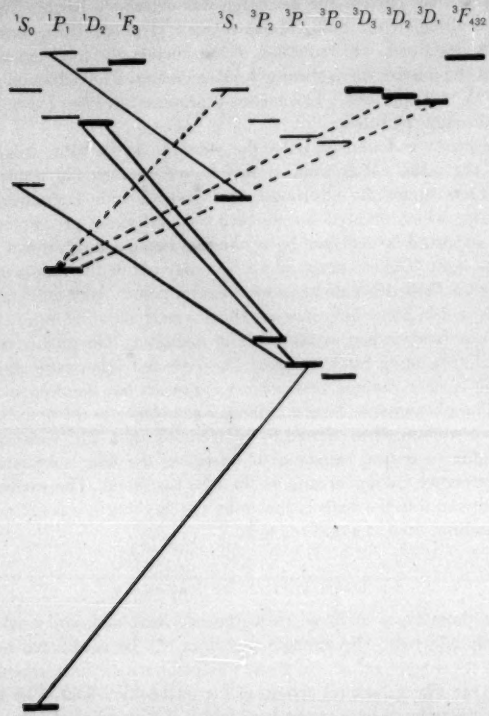


Fig. 2. Term scheme for mercury showing behaviour of intercombination lines.
Strengthened ——— Weakened – – – –

The following results are due to him, where R is the ratio of intensity when the light passes through unexcited vapour to that when it passes through excited vapour:

	$6\,^3P_0 - 7\,^3S_1$	$6\,^3P_1 - 7\,^3S_1$	$6\,^3P_2 - 7\,^3S_1$	$6\,^3P_0 - 7\,^3D_1$
λ	4046	4358	5461	2967
R	4·4	3·0	4·1	3·2

These are the only lines between $\lambda 5461$ and $\lambda 2650$ for which he gives an absorption ratio greater than 3·0, and they are the only triplet lines found to be weakened in emission. This reduction in emission must, then, be due to self-absorption in the excited vapour, and indicates the existence of a large number of excited atoms especially in the states $6\,^3P_{012}$. The states $6\,^3P_0$ and $6\,^3P_2$ are apparently favoured since they have bigger ratios, namely 4·4 and 4·1. They are metastable states and their greater probability is to be expected since the pressure is low (about 10^{-3} mm.).

The unaffected lines are (*a*) triplet series members of higher order number, and (*b*) certain unclassified lines. The triplet lines are not entirely unaffected. There is a relative increase in intensity in the direction of the series limit, but the effect is very slight. If the alterations due to absorption are neglected, then it appears that the triplet series are not affected very much. It is possible that the unchanged unclassified lines are triplet lines, due to displaced terms in the excited atom.

These intensity changes broadly divide the unclassified lines into three types: (*a*) strengthened, probably beginning on singlet levels; (*b*) weakened, probably beginning on triplet levels; and (*c*) unchanged, probably due to displaced triplet terms.

The great increase in the strength of $\lambda 2537$ probably arises from the fact that this is the resonance line. The pressure being low in the high-frequency tube and high in the arc, it follows that the line will be absorbed in the arc (with re-radiation) to a much greater extent, since there are more absorbing atoms. This results in an apparent increase in the intensity of the line in the high-frequency discharge, which explains how this line, although beginning on a triplet level, appears to be strengthened. In reality it is abnormally weakened in the arc.

Since the intensity modifications are greater with lower current density, they are probably due to diminished vapour pressure, because the temperature with smaller currents was decidedly lower, approximating to the room-temperature.

If it is assumed that singlet states are more probable at low pressures, then all the strengthened lines and unchanged lines which occur are explained by this assumption. The existence of the lines which are greatly weakened is not, however, explained by it.

§ 5. EXAMINATION OF λλ4916 AND 5461 FOR FINE STRUCTURE

Since the singlet system, especially $6\,^1P_1 - m\,^1S_0$, is so much strengthened, the second member, $\lambda 4916$, of this series was found sufficiently intense to be examined for fine structure with a Fabry-Perot interferometer. The interferometer, by Hilger, has quartz plates with a 6 cm. aperture and variable plate-separation. It was crossed with a Hilger large quartz E.I. spectrograph and fringes were photographed on Ilford special rapid panchromatic plates, an exposure of 10 minutes being necessary for the capillary portion of the quartz tube. This time could have been halved with Ilford soft-gradation panchromatic plates, which are about twice as fast as special rapid plates in this region.

The line was examined with various plate-separations from 0·5 cm. to 10 cm. The resolving power for a 10 cm. gap, for this region, is well over 3×10^6. The line proved to be quite single and extremely narrow. The fringes were so fine that it was estimated that rings would still be seen with a gap of from 40 to 50 cm. They were examined with a self-registering microphotometer, the record being shown in Plate I (*a*). The diameter of the first ring on the actual plate is about 1 mm. The sixteenth ring is distorted owing to an accidental scratch on the plate. The half-width of the fringes, being about one-third of the interval between adjacent orders, is approximately 0·004 A.U. This includes the instrumental widening produced by the interferometer. Since the Doppler half-width for a temperature of 30° C. is 0·002 A.U., there does not appear to be much broadening due to other causes than temperature. This is of interest, as it indicates that no appreciable Stark effect due to the high-frequency discharge exists. It also shows that there is very little tendency to self-reversal, which would broaden the line, and thus confirms Ponte's observation that singlet members are not appreciably absorbed.

The half-width of λ4916 has been estimated by Carrelli*, using the method of limiting visibility. The line was produced by a Hercuss arc taking 3 amp. and the half-width was given as 0·015 A.U. The line is thus very much narrower in the high-frequency discharge than in the arc.

Since the line is well isolated, the nearest bright lines being 500 A.U. away, and as it is bright enough in the high-frequency discharge to be seen with ease after passing through the Fabry-Perot interferometer, it is very suitable for the purpose of adjustment, and, being very easy to produce, it could be used with advantage in interferometry. It is proposed to make an accurate determination shortly of the wave-length and half-width. Fine structure has been observed in the $^1P \rightarrow {}^1S$ series and this is now being investigated.

The fine structure of the green line λ5461, in emission, was also observed with the pyrex tube and Fabry-Perot interferometer. Gaps of 10 mm. and 5·9 mm. were used, and the line was compared with that from a water-cooled Kromayer mercury arc running on 0·7 amp., which gives quite fine fringes. There was an alteration in the intensities of the satellites, which became more nearly equal. Ponte† has observed the fine structure in absorption by passing the light from a Cooper-Hewitt mercury arc through mercury vapour at 100° C. excited by a high-frequency discharge. He found that the stronger components were relatively more absorbed. The intensity changes in emission must then be due to selective self-absorption.

§ 6. ACKNOWLEDGMENTS

I wish to thank Prof. W. E. Curtis for his very valuable help and useful suggestions. I also wish to thank Mr S. F. Evans for permitting the use of the high-frequency oscillator which he designed and constructed.

* A. Carrelli, *Accad. Lincei Atti*, **7**, 1014 (1928).
† M. Ponte, *Comptes Rendus*, **187**, 37 (1928).

A VACUUM-TUBE COMMUTATOR: THE PRODUCTION OF A PERIODIC PULSE OF POTENTIAL OF SQUARE WAVE-FORM

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ABSTRACT. A type of vacuum tube for producing a periodic pulse of potential of square wave-form is described. It is possible to obtain with it wave-forms in which the ratio of the duration of the pulse t, to the time between pulses T, is of the order of one to twenty, at a pulse frequency of a thousand.

In the development of the 4-gauze method of measuring the mobility of ions in gases*, we have found it necessary to produce a fluctuating potential of which the wave-form approximates to that shown in Fig. 1. Since it is difficult to build commutators which will give an accurately defined wave-form at high frequencies and behave consistently over long intervals of time, we have developed a vacuum-tube method which has worked very satisfactorily. The method has other advantages besides its consistency and ability to operate at high frequencies, and we are giving this account of it since it seems probable that it may be useful in other fields of research.

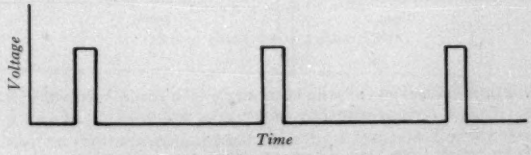


Fig. 1. Required wave-form of potential pulse.

A contact-breaker depending on the use of cathode rays has been described by Lübke† and has been used by him to study the wave-form of alternating currents by Joubert's method. The principle of our method is somewhat similar to his, and our tube is shown diagrammatically in Fig. 2. Electrons from the heavy tungsten filament (emission 100 milliampères) are accelerated to the grid and anode. A beam of electrons passes through the rectangular aperture in the anode

* Tyndall, Starr and Powell, *Proc. Roy. Soc.* **121**, p. 172 (1928). Tyndall and Powell, *Proc. Phys. Soc.* (1929).

† Lübke, *The Electrician* (Sept. 1919, p. 270).

and between two deflecting plates. If an alternating potential is applied between these plates, the beam waves backwards and forwards across the face of the metal screen shown on the right of Fig. 2. The screen is provided with an aperture similar to that in the anode and behind it is a Faraday collector. As a result, electrons enter the collector twice for every cycle of an alternating potential applied across the deflecting plates. The static curve for the instrument is shown in Fig. 3 in which the current to the collector is plotted against the potential difference between these plates. Curves are given for two values of a voltage bias which tends to maintain the collector at a fixed potential relative to the screen. The object

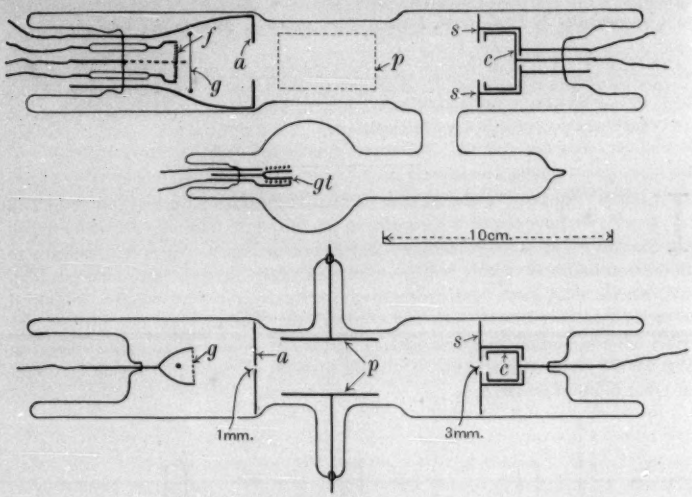


Fig. 2. Construction of tube: *f* main filament, *g* grid, *a* anode, *p* deflecting plates, *s* screen, *c* collector, *gt* getter.

of this bias is to prevent the loss of secondary electrons from the collector. It will be seen that the peak is sharp and that no serious broadening occurs as a result of the diffusion of the beam. The collector current is a maximum for a deflecting potential not of zero but of twelve volts because in our tube the filament and the two apertures are slightly out of line. The asymmetry of the curves is due to the fact that the deflecting plates are not symmetrically placed with respect to the electron beam, so that the beam when deflected in one direction strikes one of the deflecting plates and gives rise to scattered electrons, some of which enter the collector. As will be seen later these faults in construction do not lead to any difficulty in the production of the required wave-form over a wide range of frequencies.

The collector is connected to the grid of a triode and, through a high resistance, to the screen. Variations in the current to the collector cause the grid to fluctuate in potential so that the current through a resistance in the plate circuit of the valve changes. The potential fluctuations across this anode resistance are an amplified copy of those of the grid provided the grid is suitably biassed with respect to the filament.

It is necessary to ensure that the time constant, CR, associated with the capacity, C, of the collector-grid system and the grid leak R shall be small compared with the duration of the current pulse. Suppose that the frequency of the alternating

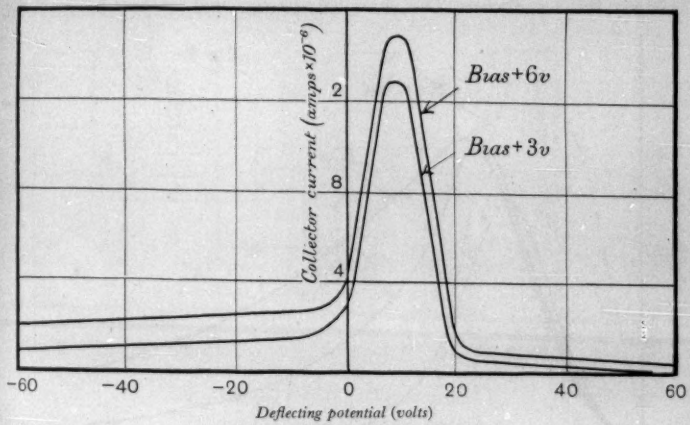


Fig. 3. Static characteristic of tube.

potential applied to the deflecting plates is f and the periodic time T, so that $T = 1/f$. From the calibration curve, Fig. 3, it will be seen that the current to the collector may be represented approximately by a function I of the time t where

$$I = I_0 \sin 2\pi pt \text{ from } t = 0 \text{ to } t = 1/2p,$$

the relation between p and f being determined by the peak value of the alternating potential applied across the deflecting plates. When this peak value is large the electron beam suffers large deflections, the time spent in crossing the aperture in the screen is proportionately small, and the ratio p/f is large. If, for example, it is desired to make the duration t of the pulse equal to one-tenth the time $T/2$ between pulses, then p will be equal to $20f$.

The potential, V, of the grid is then given by the equation

$$I_0 \sin 2\pi pt = C dV/dt + V/R.$$

n The solution of this equation, where $n \equiv 2\pi p$, is

$$V = \frac{I_0}{C(n^2 + 1/C^2R^2)} \cdot \left(n\epsilon^{-t/CR} + \frac{1}{CR} \cdot \sin nt - n \cos nt\right) \quad \ldots\ldots(1),$$

for the interval from $t = 0$ to $t = 1/2p$.

V' If $V = V'$ when $t = 1/2p$, then for the interval from $t = 1/2p$ to $t = 1/2f$,

$$V = V'\epsilon^{-t/CR} \quad \ldots\ldots(2).$$

Values of V, deduced from (1) and (2), have been plotted against the time in Fig. 4. For this purpose the capacity C has been assumed to be equal to 50 e.s.u., and the resistance R to be 10^5 ohms. The curves have been plotted for three values of the

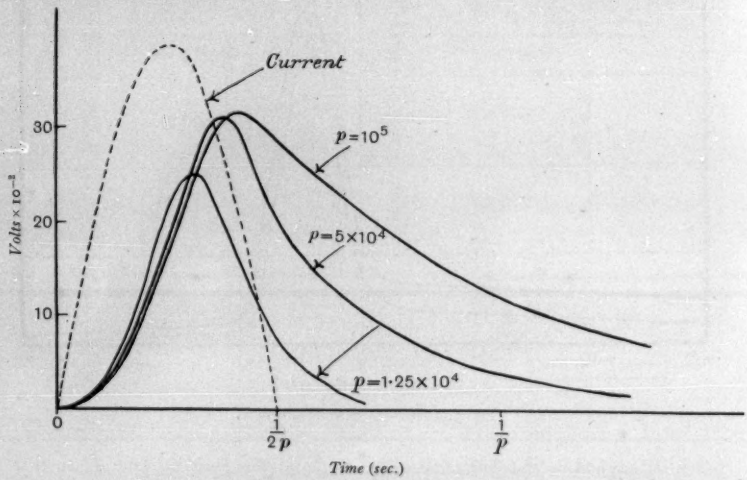
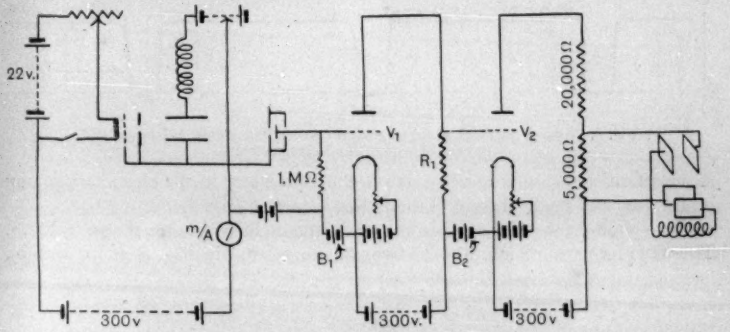


Fig. 4. Curves calculated for various frequencies. Peak current 10^{-4} amp.
$C = 50$ e.s.u. $R = 10^5$ ohms.

quantity p. The time scale for each has been made inversely proportional to the corresponding value of p. As a result the form of the current pulse giving rise to the potential fluctuations is always represented by the same curve whatever the value of p. This curve is shown by the dotted line in Fig. 4. At very low frequencies the changes in the potential of the collector will be very small but they will follow the fluctuations in the current closely. At higher frequencies the potential will tend to lag behind the current. The extent of this lag can be seen in the figure. It is desirable to ensure that the "width" of the potential pulse shall remain the same as the frequency is varied, other quantities remaining constant; that is to say, that the duration of the pulse shall be equal to a given fraction of the periodic

time of the alternations applied to the deflecting plates. It is evident that this condition can be very nearly fulfilled over the range of frequencies provided by our oscillator, namely from 60 to 6000~, for which the maximum value of p is approximately 10^5, if the time constant CR is sufficiently small.

In order to test the conclusions of the last paragraph, the amplified pulse was examined by means of a cathode ray oscillograph possessing two pairs of deflecting plates at right angles. Across one pair of plates was applied a simple harmonic potential in phase with that applied to the deflecting plates of the pulse-producing tube; across the other pair, the potential pulse. This arrangement gives a time base inversely proportional to the frequency somewhat similar to that used in plotting the curves in Fig. 4.



V_1—Marconi L.S. 5*b*. V_2—Marconi L.S. 5.

Fig. 5. Diagram of complete circuit.

The resulting patterns were exactly of the type to be expected from the analysis, but the spread of the curves at different frequencies indicated that the capacity of the grid-collector system had been over-estimated. The time-constant associated with this system was made as small as possible by removal of the ebonite base of the valve. The width of the pulse was then sensibly constant over the whole range of frequencies provided by the oscillator.

For the production of a potential pulse of the required amplitude a second stage of amplification was introduced and the complete circuit employed in this case is shown in Fig. 5.

The potential changes applied to the grid of the first valve are of the form shown in Fig. 6 *a*. When the grid potential reaches a certain negative value the anode current ceases. By the choice of a suitable bias B_1 the potential changes across the anode resistance R_1 can, therefore, be made to take the form shown in Fig. 6 *b*. This squaring off of the wave-top may be followed very clearly on the oscillograph as the bias B_1 is varied. In a similar way at the second stage

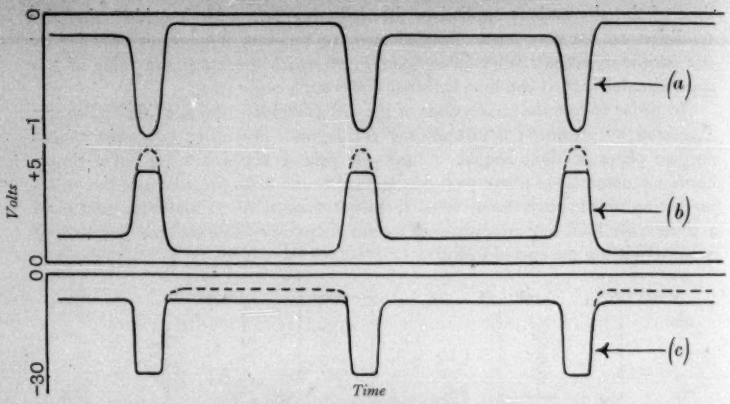


Fig. 6. Voltage fluctuations (*a*) of grid, (*b*) across first anode resistance, and (*c*) across second anode resistance.

of amplification the uneven effect due to the asymmetry in the characteristic may be removed and the final form of the pulses is seen in Fig. 6 *c*.

Two photographs of the pulses as seen in the oscillograph are shown in Fig. 7. The wave-forms to which they correspond are plotted in Fig. 8 at (*a*) and (*b*).

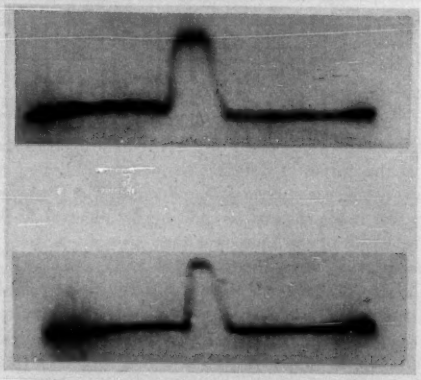


Fig. 7.

They were taken at a frequency of 400 ~ with two different peak values of the deflecting alternating potential, and they illustrate the very simple way in which the wave-form may be controlled.

In addition, by deflection of the electron beam by a magnet, or electrostatically by the biassing of one of the deflecting plates relative to the other, wave-forms of the type shown at (*c*), Fig. 8, may be produced.

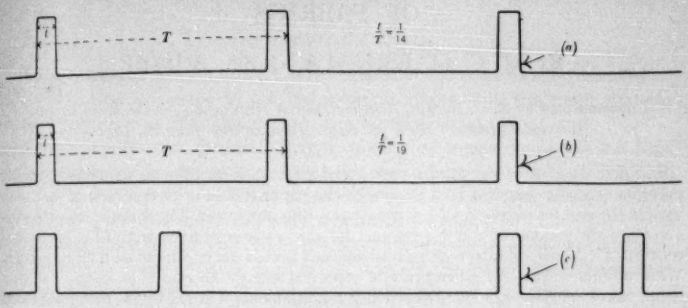


Fig. 8. Types of wave-form at 400 ~.

Up to the present we have not succeeded in getting satisfactory wave-forms at frequencies much above 2000 ~. In the present tube the electron-beam is not electrostatically shielded and the electrode system is not symmetrical with respect to the beam. At high frequencies the asymmetry in the charges on the walls of the tube results in the pulse due to the beam as it swings in one direction being different from that on its return. The pictures we have obtained leave no doubt, however, that the method can be employed at considerably higher frequencies, and we are now constructing a tube with electrostatic shielding which will avoid this difficulty.

We have great pleasure in acknowledging our indebtedness to Prof. A. M. Tyndall for his interest during the course of this research.

THE THERMAL INSULATING PROPERTIES OF FABRICS

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ABSTRACT. One of the chief properties of a fabric is its thermal insulation, which prevents excessive heat-loss from the body. The paper gives a critical review of methods used in the past for measuring the thermal insulating properties. These are discussed with a view to the making of a new apparatus for the study of the subject. This apparatus consists of a copper cylinder with guard-ring ends heated electrically to skin temperature. With various fabrics the expenditure of electrical energy necessary to keep it at that temperature is measured. The surrounding conditions are defined by its being enclosed in a larger concentric tube whose temperature is regulated by the circulation of water round it. The humidity of the air can also be regulated. The temperature measurements are all by thermo-couple. The weight per unit area and the thickness of the fabrics are measured. There seems to be no definite relation between thermal insulating value and weight, but there is a relation which is very approximately linear between thermal insulating value and thickness. A detailed analysis of the results is being made.

§ 1. INTRODUCTION

The value of a material as a thermal insulator is usually judged by its conductivity as measured by well-known methods. There are, however, materials to which such methods cannot be applied to any purpose. Of these, fabrics are a good example for, owing to their compressibility, it is impossible to measure their conductivity in the usual way without causing deformation. It would be possible to put forward a scheme for effecting this measurement without compression but great difficulties are introduced by the lack of homogeneity of the materials and the absence of definite surfaces.

With a discontinuous substance such as a fabric the transmission of heat by convection and radiation as well as conduction must be considered. Further, unless the fabric is in thermal and hygroscopic equilibrium with the surroundings, changes in the moisture content of various parts may cause heat to be transferred. When a fibre takes up water, heat is produced (heat of wetting[1]) in addition to latent heat effects. In this paper, however, only equilibrium conditions are studied, so that conduction, convection, and radiation are the only effects to be considered.

It seemed, therefore, advisable to determine the total heat loss of a body with and without a wrapping of fabric and to divide the work into two parts, viz.: (*a*) A study of the effect of all possible variations of conditions and circumstances on the total heat loss from the body. (*b*) Based on these results, an attempt to define

rigid and repeatable conditions and under these to compare different fabrics approximating as nearly as possible to those obtaining in practical use. Measurements of the physical properties of fabrics would also need to be made.

§ 2. PREVIOUS WORK

A large amount of work of very varied character has been done on this subject, mainly by those interested in the technological aspect, with the result that the physical properties of the fabrics have often been neglected.

A very popular and apparently simple method of comparing fabrics has been to determine cooling curves for bodies wrapped in them[2]. Its simplicity has often led to the neglect of essential precautions such as the stirring of liquid inside the heated body and the defining of surrounding conditions. Some have even worked with damp fabrics and totally neglected the latent heat of water evaporated. The only work approximating to precision involves the use of the Hill katathermometer[3] but the results obtained with this instrument are still the subject of much discussion. The whole method bristles with difficulties. If the temperature range is small, the lag of the thermometer, temperature gradient between various parts, etc. may cause large errors. If the range is large, the change in hygroscopic capacity of the fabric has to be considered, since water will be absorbed as the fabric cools. On the constructional side difficulties again appear. With this method guard-rings cannot be used, so it is necessary to fit the fabric uniformly over the whole surface. A woven fabric cannot be fitted over a sphere. A cylinder is better, but there is difficulty in introducing a stirrer, thermometer, etc.

A very great improvement on the cooling-curve method is one in which a body is kept at a constant temperature by electric heating. With this method the main measurements become electrical; a guard-ring system may be used so that the surface from which the heat is escaping may be very simple in form and the fabric may be left a sufficient time to get into hygroscopic equilibrium with the surrounding atmosphere. Barker and Tunstall[4] have shown recently that the time for an average fabric to reach the equilibrium state is much longer than had been previously supposed.

Haven[5] used a cylinder 60 in. long and $4\frac{1}{2}$ in. in diameter filled with water, with electric heating controlled by thermostat and relay. Instead of guard-rings he had insulating caps, which are less definite thermally and mechanically, on the ends of the cylinder. The thermostat and thermometer were inserted into the cylinder radially, and this involved cutting holes in the specimen. The heat capacity of the tube and water was very great so that three thicknesses of blanket had to be used. For thinner materials the apparatus would therefore have been useless. At the Bureau of Standards[6, 7, 8, 9] experiments were carried out with a square plate let into a board as the hot body. This had the great advantage that the fabric could be laid on without cutting or stitching, but the guard-ring system was very complicated, consisting as it did of four separate sides of the square and another similar heating system on the other side of the board, making nine guard-ring

heaters to be watched and controlled. The temperature measurement was made with a complete set of thermo-couples. The outside conditions were rather vague as the whole apparatus was in an ordinary-sized room and no precautions were taken to avoid draughts. The plane surface makes it very difficult to vary the contact pressure between the fabric and the hot surface. Floyd and Baker[10] have used an oil-filled cylinder and Muller[11] a sphere. The surroundings were carefully defined by outer jackets but with both methods difficulty was experienced in covering the whole body uniformly.

Much good work has been done on measurements of thermal conductivity of fabrics. As has been already pointed out, there is considerable doubt whether thermal conductivity is a criterion for the comparison of discontinuous substances like these, and further, the fabrics are compressed during the experiment, the pressure often being unmeasured. The results are often stated as the thermal conductivity of the fibre (wool, cotton, etc.), whereas what is measured is an average "mass" conductivity of compressed fabric.

In most of this work, a few fabrics are mentioned incidentally among other insulators with the result that full specifications of the material are not given. Under this heading comes the work of Lees[12, 13], Lees and Chorlton[14], Randolph[15] and Griffiths and Kaye[16]. Of those who worked specifically on fabrics the following may be mentioned. Bauer[17] has used an empirical method of little value. Lees' disc method with corrections for change of emissivity with temperature has been employed by Rood[18]. Staff[19], with an apparatus similar in essentials to that of Griffiths and Kaye, studied the change of conductivity with moisture content. Speakman and Chamberlain[20] have recently used a guard-ringed Bunsen ice calorimeter method. The thesis[21] on which this paper is based gives a complete summary of results of all conductivity work on fabrics and loose fibres. Loose fibres strictly do not come into this subject, but it may be noted that Nusselt[22] and Lamb and Wilson[23], using concentric spheres and coaxial cylinders respectively, measured the conductivities of several types of fibres at various densities.

Of other methods, the only one which need be mentioned is by Gregory[24, 25], who worked on the suitability of fabrics as protectors against the radiant heat of the sun.

§ 3. EXPERIMENTAL DETAILS

After some preliminary trials it was decided to use the constant temperature method and for this a cylindrical "heater," similar to Haven's but much smaller, was constructed. A guard-ring is provided at each end so that all difficulty about end losses is completely removed. Thermo-couples eliminate the need for a mercury thermometer and others are used to detect the temperature-difference between the central part and the guard-rings. The outside conditions are made definite by enclosure of the heater inside another cylinder whose temperature can be controlled. The ends of this cylinder are closed to prevent draughts and to

allow for the control of the humidity of the air. Rheostat-controlled electric heating is used in preference to timed intermittent heating, and a.c. was rejected on account of possible induction currents. For sensitivity it was desired that the heat capacity should be small and for this reason the heater was not filled with oil or water. The result may have been to give a greater lag in response to change of current, but the arrangement is thought to be worth while.

A detailed description of the final form of apparatus will now be given.

The heater. *A*, Figs. 1 and 2, consists essentially of a copper tube containing heating coils. The diameter of the tube is 5·5 cm. approximately, and the central portion is 25·0 cm. long while the guard-rings are about 10 cm. long. The heat capacity of the central copper tube is only 69 gm. of water while its thickness is calculated to be such that temperature-differences round the circumference due to disposition of the heating coils are inappreciable.

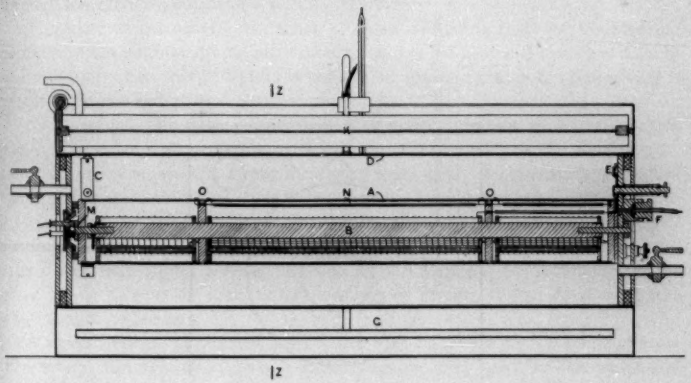

Fig. 1.

Two turned discs of red vulcanized fibre insulate the end tubes from the central one electrically and to some extent thermally. Two similar discs close the ends and are drawn together at their centres by screws projecting from the ends of a rod of vulcanized fibre *B*, thus clamping the whole heater together solidly.

The heater is coated with shellac varnish to give a non-tarnishing surface which could be repeated fairly accurately if necessary.

On the central rod are supported the three heating coils of Eureka wire, each of which is wound on quartz insulators threaded on brass rods. The resistance of the central coil is about 6 ohms and that of the guard-ring coils about 10 ohms. The leads to these coils are heavy copper wires insulated with glass tubes. All the internal connections are soldered in order to reduce contact resistance. The three leads to the coils come to three terminals at the front end of the tube while the common return is soldered to a plate *M* at the other end.

The thermo-couple *N* for measuring the temperature of the central portion of the heater is at the middle point. As was pointed out by Aberdeen and Laby[26], with a long tube of this kind there will be an approximately constant temperature along a considerable portion of the tube even if the guard-rings are not at exactly the right temperature. There is thus an "internal" guard-ring effect. Lamb and Wilson[23] showed that it was enough to measure the temperature of a cylinder used in this way at one point. In the same radial plane differential thermo-couples *OO* are provided between the central and end tubes. The thermo-couple leads are all carried inside the tube and insulated and protected from injury by glass tubing.

The far end of the heater is supported on a three-legged spider *C* which is clamped round it by three screws. Each leg is provided with a small rubber-tyred roller. These hold the heater centrally in the outer tube and allow it to slide in and out without scratching the surface.

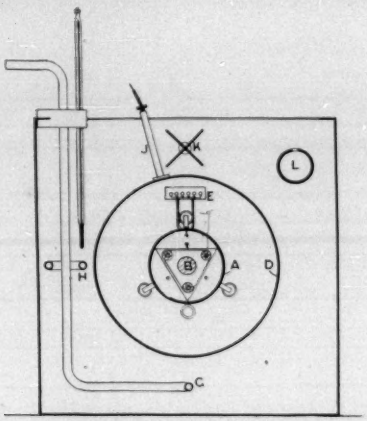

Fig. 2. Cross-section at *ZZ*, Fig. 1.

The end-plates. This heater, covered by a fabric, is placed during an experiment in a large concentric copper tube *D* surrounded by water. It was desirable that the ends of the tube should be closed with airtight plates to prevent draughts along it and also to allow the humidity of the air inside to be controlled. As one of them is attached to the heater the description of both may be put in here.

Each plate consists of two stiff Monel-metal discs which are an easy fit into the outer tube. Between them is held a rubber ring of the same outside diameter and this can be squeezed between the discs by six screws (not shown). When squeezed it increases in outside diameter and thus makes an airtight joint with the outer tube. If necessary these plates can withstand considerable differences of pressure. With both plates the inner disc is thicker and everything passing through

it does so with airtight joints while the outer one simply serves to squeeze the rubber ring. Both plates are provided with a large tap to enable air to be circulated through the annular space between the heater and outer tube.

Through the plate attached to the heater it was necessary to carry the three leads for the heating currents and the thermo-couple wires. The current is brought through the plates by three screws which are insulated from the plate and are made airtight by threading over them a piece of thick-walled rubber tubing whose outside diameter is equal to the diameter of the hole in the inner disc. When this rubber is compressed flanges are formed at the ends so as to provide an insulating airtight joint. This method of making such a joint has found several applications and has been described elsewhere[27]. The thermo-element wires from the heater are brought to a small terminal block *E* on the back of the plate, to which they are soldered. Similar wires run from there and are enclosed in a sheath which passes through the plate by means of a packing gland *F*.

The plate at the other end carries a central insulating bush for three spring contacts which impinge on the plate *M* at the end of the heater. To prevent damage to the heater when this end-plate is pushed up against it, a rubber buffer ring is provided on the end-plate.

Surroundings. The outer copper tube *D* is built into a tank which can contain water. The water is stirred and circulated round *D* by a long paddle *K*, above the tube, driven by an electric motor through a worm gear. A convenient method of constructing this worm gear so as to be silent has been described elsewhere[28].

Water can be supplied directly into the tank from the mains by a pipe *G* running along the bottom and perforated by a number of small holes. A large tube *L* with many holes prevents overflowing and a drain tap is provided. On the other hand, there is an independent cooling or heating system *H* running the whole length of the tank.

A thermo-couple is placed on this outer tube but is electrically insulated from it by a very thin mica sheet, as a precaution against stray e.m.f.'s. The couple and mica are held on the main tube by a screw inside a side tube *J*. For alteration of humidity inside this chamber an air circulation system is provided (see Fig. 3). For this purpose a small centrifugal fan having an aluminium rotor 10 cm. in diameter and mounted on ball-bearings was constructed. The drive is from the motor which works the stirrer. This fan pumps air through the wetting or drying system to the chamber round the heater. The air returns to the fan through a sample bottle for testing the humidity, then through a combined flowmeter and manometer. Conditions are always adjusted so that the manometer shows a pressure inside the chamber slightly in excess of atmospheric, so that all leaks in the system should be outwards. The only inward leak is through the fan bearings, so outside air has to pass through either the wetting or the drying system before reaching the chamber. The drying system consists of several calcium chloride tubes in series, while the wetting system is a number of wash-bottles immersed in the water in the tank. This precaution prevents the air from taking up too much water and depositing it as dew on the walls of the outer tube. The sample bottle *Y*

was packed with some cuttings of the fabric under test and by removing and weighing the bottle from time to time the observer was able to estimate the hygroscopic condition of the fabric under test. It was sometimes convenient to use the drying system, when no special humidity was desired, to prevent the formation of dew on the outer tube as the result of the giving up of moisture by the fabric when heated. In all cases the air current was cut off for some time before any readings were taken.

The inside of the outer tube was originally bright, but in order to get a surface which would not change greatly owing to atmospheric impurities, it was blackened with a resulting large increase in its heat-absorption.

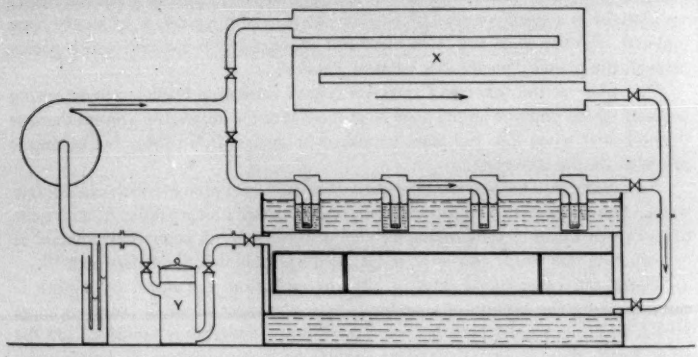

Fig. 3.

The fabrics were initially made into sleeves which fitted over the heater. Later, work was done with a definite air film under the fabric. For this purpose three frames were used made of woven wire of a fine gauge and very open mesh, (i) wrapped directly on the heater surface; (ii) 6·5 cm. in diameter and supported only on the guard-rings; (iii) 8·0 cm. in diameter and supported only on the guard-rings. With the last two the fabric is held completely clear of the heater surface. It is estimated that the wire occupies only 8·5 per cent. of the area of the frame. When the frames are in use an insulating ring is pushed on the heater first to prevent the frame from making contact with any of the electrical leads.

Energy control and measurement. In order to get a very steady current for the heating coils, large-capacity accumulators were used. It was quickly found that variations in the guard-ring circuit currents caused small but perceptible variations in the main circuit. A separate battery was therefore provided for the guard-ring heaters. It was found that the resistance of some of the leads common to both batteries had an effect, so the circuits were separated completely except for the common wire inside the heater, which had no measurable effect. This change involved putting two spring contacts in the insulating bush at the centre of the

back end-plate. These were later increased to three, the third being for a voltmeter lead, as it was found that the contact resistance between the spring plungers and the end-plate of the heater was not small compared with the resistance of the heater coil and that, being variable, it made the voltmeter readings uncertain to an extent rather greater than the experimental error. The third contact completely eliminated this effect. The complete circuit is shown in Fig. 4.

To allow the guard-ring currents to be set at definite values an ammeter is provided which can be switched into either circuit as desired. An equal resistance completes the other of the two circuits.

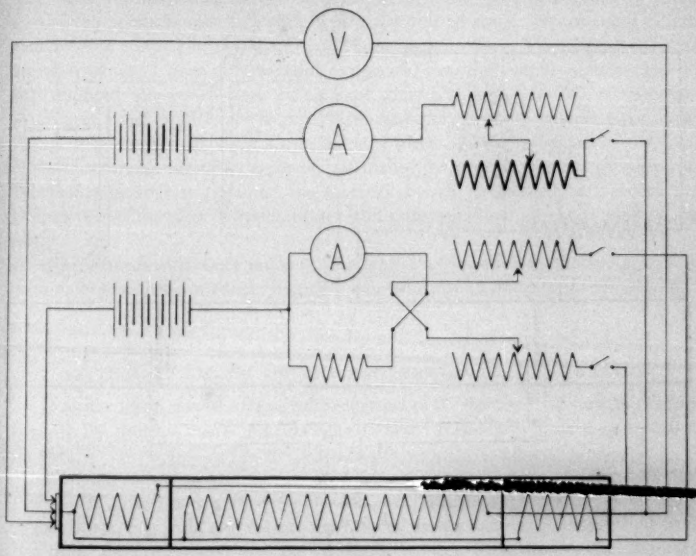

Fig. 4.

The control of the currents is by ordinary rheostat but the circuit for the central section is provided with a carbon compression resistance in series with the rheostat to allow very fine adjustments to be effected.

Temperature measurement. There are two distinct thermo-electric systems in the apparatus. They have the following uses: (i) The measurement of the temperature-difference between the heater surface and the outer tube. It is also important to be able to detect very slow changes in this temperature-difference. (ii) The detection of the temperature-difference between the central and guard-ring parts of the heater.

The temperature of the bath is measured with a N.P.L. certificated mercury thermometer.

The measurement of temperature-difference between the heater and outer tube is made with a calibrated millivoltmeter (see Fig. 5). In order to get rid of resistance effects the millivoltmeter measures an e.m.f. provided by a potentiometer-type circuit which is balanced against the thermo-electric e.m.f. The balancing is indicated by a sensitive mirror-type galvanometer and this also serves to show any slow change in temperature if the potentiometer setting is kept constant. Such a change indicates that the heating current is not correct. This combination of galvanometer and millivoltmeter proved very convenient.

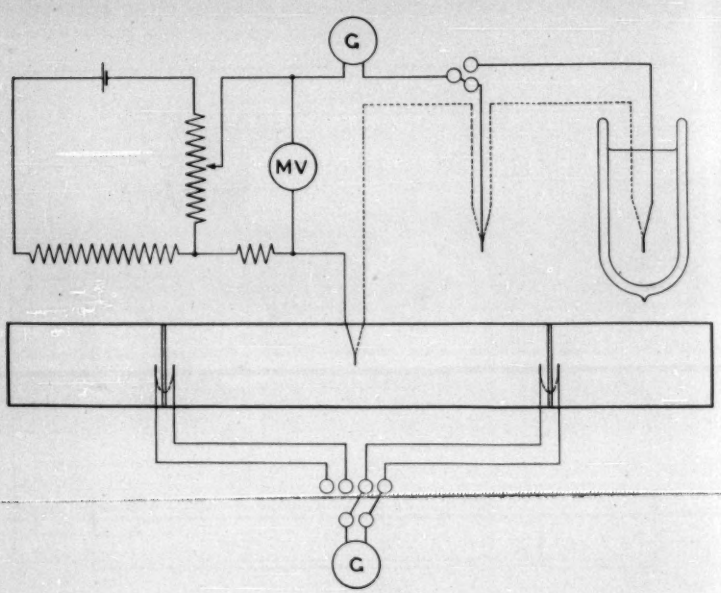

Fig. 5.

As soon as the work was started, it was found difficult to prevent variations in the temperature of the water, and hence of the outer tube, of the order of 0·1° C. Though these were small compared with the temperature-difference they made it impossible to know whether the heater was varying in temperature. This difficulty was overcome by the use of a large volume of water in a Dewar flask, and during the experiment the temperature-difference between the heater and the water was indicated. When a good balance was obtained the circuit was changed by the mercury switch and the temperature-difference between heater and outer tube measured.

The detection of temperature-differences between the central portion and the two guard-rings was done by differential thermo-couples connected to a galvanometer by a two-way two-pole mercury switch. The two galvanometers were of the same type and they indicated on the same scale. Their sensitivities were of the order of 2×10^{-7} volt/mm. at 1 metre radius, which corresponds to about 200 mm. divisions per 1° C.

Method of experiment. The sleeve is fitted on the heater with or without a frame, as required, and the spider is put on the end. The whole is put into position in the outer tube and the six nuts on the end-plate are screwed up. The tank is filled with water and the stirrer and fan are started. Heating currents are then switched on and the potentiometer adjusted to give the desired e.m.f. The mercury switch connects the couples in the heater and in the vacuum flask. The heating currents are then adjusted to give a constant temperature on the central portion close to that required, the guard-rings being kept constantly within 0·1° C. of the temperature attained. With care it is possible to adjust the current to yield a temperature-change of no more than 0·005° C. per minute, which, with a heat capacity of 70 gm. of water, corresponds to an error of less than ½ per cent. in the energy supplied. When this balance is obtained the mercury switch is changed to measure the temperature-difference between heater and outer tube and all measurements are made.

All measurements were made on the fabrics as supplied by the makers. Any preliminary washing or other treatment may alter the characteristics considerably.

§ 4. MEASUREMENT OF THE PROPERTIES OF THE FABRICS

In order to specify the physical properties of a fabric with a view to relating them to the thermal insulating properties, it was necessary to make measurements on each.

Material. Most of the fabrics used consisted of one type of fibre only and these were generally specified by the makers. Where there have been mixtures of two fibres, e.g. wool and cotton, the makers' statements of the proportions have not been definite enough and the fabrics have been submitted to the Bradford Conditioning House for standard tests. These give the proportions of various fibres calculated on the clean dry weight.

Weight per unit area. A sample of the fabric was cut out with a square knife under a cloth cutter, which is essentially a hand screw-press. The samples were then placed in a room whose humidity and temperature are controlled automatically. After three days they were weighed without being removed from the room. For all samples the relative humidity was 70 per cent. and the temperature 73° F. (22·8° C.).

Thickness. It was realized from the start that the measurement of thickness would present difficulties, since the thickness depends on the pressure. An apparatus for this purpose should therefore be capable of measuring the thickness under

very small pressures and yet, without the cloths being moved, be able to put on any reasonable pressure and measure the thickness at the same time. An instrument which has proved satisfactory for this purpose has been described in detail elsewhere[29]. Thicknesses can be measured to 0·01 mm. at all pressures from 1 to 100 mgm./cm.^{2} without the fabric being moved.

Light transmission. In order to get some measure of the "openness" of the structure of a fabric to radiant heat, a photo-electric method was used. The apparatus was an adaptation with slight alterations of that used by Barker and Stanbury[30] for the measurement of levelness of yarn. A constant, parallel beam of light is focussed on to a slit, under which is a photo-electric cell connected to a Lindemann electrometer. The deflection of the electrometer is proportional to the quantity of light passing through the slit. If a fabric is placed in the beam the ratio of the deflection to that obtained when there is no obstruction in the beam is the fraction of the fabric which has a clear aperture from side to side. There are quite open fabrics in which the apertures are not perpendicular to the surface, so that exact concordance is not to be expected between air and light transmission.

Permeability to air. After some attempts to compare the resistance of a fabric with standard orifices, tapped viscosity tubes, etc., it was decided that the simplest method would be to draw air of known temperature and humidity through a defined area of fabric, to determine the pressure-drop and to measure the air passing with a calibrated gas meter. In the case of the pressure-drop, a micromanometer was used for measuring differences too small for an ordinary manometer. This micromanometer consisted of a U-tube with wide limbs and narrow connecting tube so that the damping was high. Two hook gauges moved by micrometer heads entered through mercury sealed joints. Large differences were found between samples from the same piece, but these are insignificant compared with the enormous range of resistances measured, and all that is required for the present purpose is the order of the resistance. This section of the work has found other applications and will be described fully in a subsequent paper.[31]

Colour. No measurements of colour have been made.

§ 5. RESULTS

Before the actual results of the experiments are discussed, the mode of expressing them may be noticed. In the preliminary work on the effect of various factors, results are generally stated as the total heat loss from the central portion of the heater in watts.

For the comparison of fabrics or of one fabric under different conditions, it has been found convenient to define a new quantity depending only on the heat lost by the heater uncovered and covered *in a definite specified way*. This quantity, called the thermal insulating value (T.I.V.), is defined to be:

$$\left\{1 - \frac{\text{heat lost by covered heater}}{\text{heat lost by uncovered heater}}\right\} \times 100 \text{ per cent.}$$

It would have a zero value if the heat loss were the same with as without the fabric, and a value 100 if there were no heat loss. It has been found that some thin fabrics placed directly on the heater cause an increase in the emission of heat. These will have a negative value when calculated by the above expression.

These negative values may appear strange at first sight but the reason for their occurrence is quite clear. The effective surface for loss of heat is increased when a fabric is put over a smooth varnished surface. Negative values have been obtained with some suitings and hosiery fabrics wrapped directly on the heater surface.

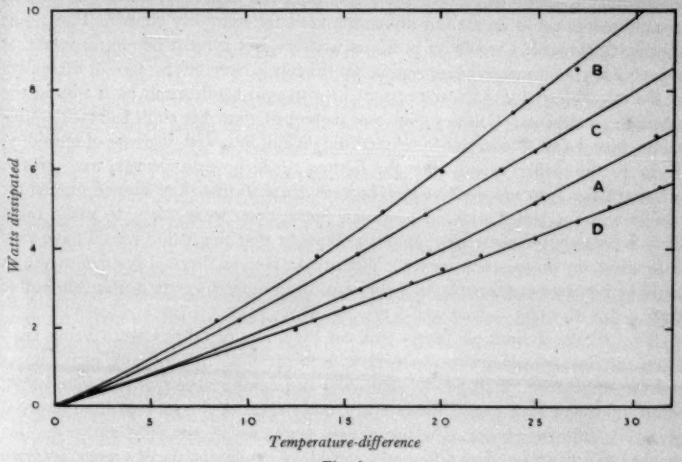

Fig. 6.

The first experiments undertaken were to find the relation between the energy supplied (i.e. total heat loss) and the temperature-difference between the heater and the outer tube. Various surfaces and conditions were used and the results are shown in Fig. 6.

Curves *A* and *B* relate to the uncovered heater when the inner surface of the outer tube is bright and black respectively. These curves show the importance of a definite surface as well as temperature in the boundaries of the space. As bright surfaces very easily tarnish it was decided to use a black surface on the outer tube for all work. Curves *C* and *D* show the relation for the heater covered with a fabric, directly in contact, and supported on a frame, respectively.

It will be noticed that the curves do not differ greatly from straight lines through the origin, such as would be given by Newton's law of cooling. This allows experiments to be done at temperature-differences approximating to a selected standard, small corrections being made on the assumption that the power required is proportional to the temperature-difference with only second-order errors.

From the curves it may also be deduced that the ratio of the ordinates of any two curves is practically independent of temperature. It is therefore possible to select any convenient temperature for the comparison of fabrics.

Very large changes of humidity, such as would not be met in normal practice, were tried. These were produced by the continual passage of the air through the wetting system, so that it became saturated at a temperature just lower than that of the cold surface, or by passage of the air over the calcium chloride. They had very little effect on the total heat loss and it was concluded that the differences due to ordinary changes of humidity are less than the experimental errors, so that measurements could be made without the need for humidity control. This result appears to contradict results of previous workers, but it must be emphasized that in practically no previous investigation has the fabric been left for two or three days in the experimental condition to reach hygroscopic equilibrium as it was in the present experiments. Gregory, who was careful to allow his cloth to reach equilibrium, found a small decrease in transmitted radiant heat with increase of humidity.

In all the earlier experiments the fabrics, made up into sleeves, were on the surface of the heater itself. The sleeves were made so that they slipped on and off readily without being slack. No special precautions were taken to make them all fit in exactly the same way, as it was thought that the fitting would have very little effect on the surface density. With these sleeves thermal insulation values could be repeated consistently to 1 per cent. and numerous tests were made in this way.

The effects of multiple layers and combinations of fabrics were being tried when certain anomalies were discovered in the results. After many experiments and the elimination of all possible variables and complications, it was found that, while any sleeve gave results consistently, two sleeves of the same knitted material gave very different results. The two sleeves were closely examined and tested for weight per unit area and no differences capable of explaining the discrepancies were found. It was noticed, however, that one sleeve was more slack than the other on the heater: it gave a much higher insulating value, but when it was tightened the value fell eventually to below that for the other sleeve.

As these results were for a knitted fabric which is easily stretched, it was decided to test woven fabrics, and a flannel and a blanket were chosen. The same effect was again obtained as is seen in the following table:

Effect of tension of fabric on thermal insulation

Description	Thermal insulating value. Tension increasing		
Hosiery (Wool)	10·5	− 3·5	− 6·1
Flannel	10·5	5·0	2·9
Blanket	40·7	31·2	—

It is thus clearly established that it is very important in comparing fabrics either to find a standard or comparative tension for each fabric and work to that, or to eliminate the effect of tension completely.

Since in the above cases the stretching was never more than a few per cent. of the original length (say 3 to 7 per cent.) the fall in insulating value cannot be explained by changes of surface density or the opening of the interstices of the fabric so as to allow a freer passage of air. The only possible explanation appears to be that a fabric resting on a surface ordinarily is supported by the more projecting fibres and that an air film, which is comparable in its insulating power to the fabric itself, exists between the fabric and the surface. When the fabric is tightened on to a curved surface this film is diminished in thickness and the fabric establishes a better thermal contact with the surface and thus the apparent thermal insulation is diminished.

This suggestion was tested out as follows. A more definite air film between the fabric and the heater was created by the insertion of the very open-mesh wire gauze. This gave a much greater insulating value, but there was still a large difference on the fabrics being stretched. It was noticed that it was still possible for the fabric to sag through the meshes and come into contact with the heater.

The two larger frames of wire netting of different diameters were then tried, and further increase in the insulating value of the fabrics on these was found in all cases. But now that the fabric, whether stretched or loose, was quite clear of the heater surface it was found that the insulating value was unchanged on the fabrics being tightened on the frame. This result gave a method of comparison of fabrics *independent of tension* within the accuracy of experiment. In all previous work the effect of this air film beneath the fabric has been completely overlooked. The results are therefore open to very grave criticism as the effect must have occurred in all work involving the wrapping of a body in the fabric. It further confirms all that has been said above as to methods involving the compression of the fabric between two surfaces. Once this effect has been seen it becomes quite obvious, and it is remarkable that it should not have been detected before. Clearly, therefore, a fabric must be measured with both surfaces free from solid bodies as far as possible. This condition was approached by the supporting of the fabric on the very open wire frame, 6·5 cm. in diameter, for all comparative measurements.

§ 6. COMPARISON OF DIFFERENT FABRICS

From the results given above it is now possible to formulate a scheme for the comparison of different fabrics under defined conditions. It was decided to make the comparisons on the 6·5 cm. frame and at a standard temperature-difference of 25° C. approximately, the values being corrected to 25·0° C., while the cold temperature was kept as near as possible to 12° C.

The thicknesses of the fabrics were measured at three pressures, viz. 0·1, 1, and 10 gm./cm.^{2}. The results for thermal insulating value and thickness at 10 gm./cm.^{2} are shown plotted in Fig. 7. At this pressure the points were less scattered than at the other pressures. This would indicate that it is the more solid part of a fabric which is important in thermal insulation, while the projecting fibres are not of great importance. It would have been better to have determined a thickness

pressure curve for each fabric, as from such a curve the point where the solid structure of the fabric is reached can be seen. But the average of a number of such

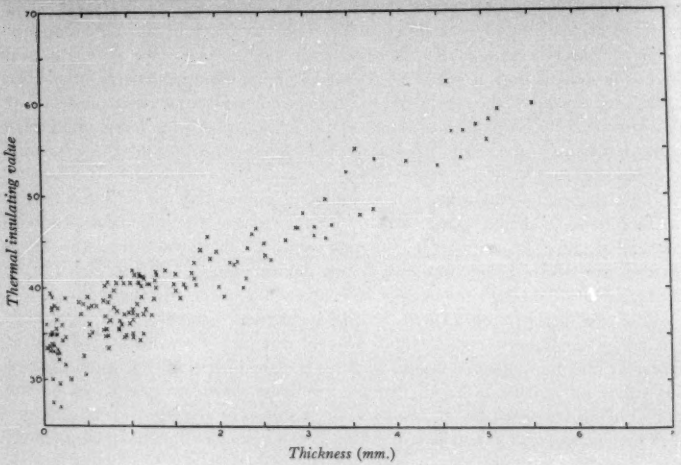

Fig. 7.

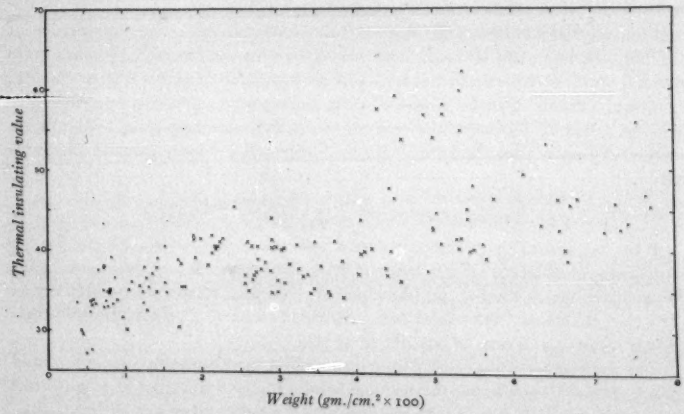

Fig. 8.

curves showed that this point was not very different from that given by a pressure of 10 gm./cm.², and measurement at a definite pressure is much more convenient.

The points lie very approximately on a straight line which does not pass through the origin. The deviations from the straight line are largely capable of explanation by reference to the other physical properties and thus give definite information as to the method of transfer of heat through a fabric. This will be discussed later.

Fig. 8 shows thermal insulating value plotted against weight per unit area and shows that there is no direct relationship except perhaps a slight increase in thermal insulating value with weight. Again, however, the position of certain fabrics and groups of fabrics, related to their other physical properties, give definite information.

From the thickness at 10 gm./cm.^{2} and the weight per unit area the density has been calculated. No definite relationship appears to exist between thermal insulating value and density for the same weight or thickness, but there is a marked tendency for the less dense fabrics to show a higher thermal insulating value than others.

While textile details need not be entered into here, the general trend of detailed analysis may be given as it has some physical importance. Most of the work on heat flow through fabrics has been done with the object of deciding the relative values of different fibres and many opinions have been expressed. The great difficulty is to get comparable fabrics of two different materials. It is found that there are in general use very few non-woollen fabrics that are more than 1 mm. thick and not many made of wool less than 0·5 mm. thick. Comparison has therefore to be made between rather extreme fabrics of each material. Similarly, as regards weight per unit area, wool occupies practically the whole region above 0·02 gm./cm.^{2}. Any other materials which come in this region are not of a comparable type of fabric. In the parts where wool overlaps with other materials, either in weight or in thickness, there is strong evidence that it is superior to cotton and artificial silk. Direct comparison cannot be made with silk or linen.

Cotton, silk, linen and artificial silk all fall into the same region, but there are greater differences between different fabrics of the same fibre than between those of different fibres. Thus, structure is much more important than material for these. Silk, however, is capable of producing a better insulator than the others with a proper structure.

Quality as affecting thermal insulation has only been studied in wool fabrics and shows itself especially in the case of blankets. The finer the wool the higher the insulating value is a general rule.

In general, a large light-transmission and a low air-resistance go together and cause a low insulating value, especially in the thinner fabrics. In the case of fabrics under 1 mm. thick every fabric with a thermal insulating value less than the average has a very low air-resistance. With thicker fabrics air-resistance seems to have little significance. A fabric with a shiny surface appears to be a good insulator, but direct comparison is very difficult. Again, as far as can be ascertained from these experiments, the colour of the fabric has no effect on the thermal insulation. The difficulty of doing direct experiments on this point is the change which necessarily takes place in a fabric on its being dyed.

§ 7. DISCUSSION OF RESULTS

The (thickness)/(thermal insulating value) diagram may be taken as a basis for the discussion of results. From it can be formed a fairly accurate idea of the mode of heat-transfer through a fabric or through insulators of a similar character used with a free air space on each side.

It may be taken that there is approximately a linear relationship between thermal insulating value and the thickness of the fabric excluding projecting fibres. This holds over a very wide range of thicknesses covering every type of fabric.

It should be noted that an average line drawn through these points does not pass through the origin, but that for small thicknesses there is a thermal insulating value of approximately 30 per cent. This would indicate that there is a large heat loss by convection from the body when uncovered and that the thinnest fabric breaks up the air stream, thus affording a considerable amount of insulation. This conclusion is in accordance with the general known methods of reducing heat loss by convection. Confirmation is obtained from the results which show that all thin fabrics with a small resistance to the passage of air have a smaller insulating value. In this case the warm air can pass directly through the interstices of the fabric, thus causing a greater heat loss than would be expected from the thickness measurement.

Similarly those fabrics which have a high light-transmission give a low insulating value, as would be expected since they allow direct passage of heat radiation and convection.

In order that these ideas might be tried out by extreme tests, several unusual coverings were investigated. A thick very open net gave a value much lower than any other fabric of the same thickness. On the other hand, tracing-linen and paper, which are impervious to air, gave high results compared with those of pervious fabrics.

A further very interesting experiment was on thin sheet brass. It gave a thermal insulating value of about 54 per cent. when blackened and about 72 per cent. when polished. Here the conductivity of the metal is so high that the resistance to passage of heat through it is negligible. Under these conditions, on the assumption that the heat passing across the space is proportional to the temperature-difference, the conducting plane should cut down heat loss to 50 per cent. if its surfaces are perfectly absorbing and radiating. Actually the blackened surfaces do not fulfil the condition completely, so that the thermal insulating value is rather over 50 per cent. When the surfaces are bright this effect is increased so that the thermal insulating value is greatly increased. This is a similar effect to that shown in the earlier experiments on the loss of heat from the uncovered heater to polished and black surfaces.

It may now be asked why a good conductor such as brass is, under these conditions, a far better insulator than fabrics, whose conductivity as ordinarily determined is much lower. This cannot be altogether explained by the imperviousness to air of the metals, as some of the fabrics did not allow any passage of air at all.

It is suggested that the effect is due to the larger surface of the fabric in which more heat can be received and lost by convection. Confirmation of this is given by the fact that thin sleeves fitted directly on the heater caused it to lose more heat than it lost when bare. It was also found that a smooth, shiny-surfaced fabric had a high insulating value. Gregory[25] has shown recently that fabrics have an emissivity ten times as great as polished copper.

From a theoretical standpoint the passage of heat through an insulator such as a fabric is very complicated. Smoluchowski[32] has worked out the problem of the conduction of heat through spheres regularly packed, making certain assumptions as to discontinuity of temperature at the surfaces. This theory and the results to which it leads do not fit in at all with the work of Aberdeen and Laby[26], so that even in this relatively simple case there are very great difficulties. When we consider a fibrous insulator such as a fabric the difficulties increase enormously, because: (i) fibres are of irregular shapes and very different cross-section*; (ii) the arrangement of the fibres in the yarn may be somewhat haphazard, and they often project from the yarn; and (iii) the processes subsequent to weaving tend to merge the threads of yarn into each other.

The mathematical treatment of the heat-flow through a fibrous insulator therefore becomes impossible, but an approximate general idea, which is confirmed at many points by the results obtained, may be formulated.

The heat passing directly through the fabric by convection and radiation may be ignored and attention may be concentrated on that part which passes through the solid structure. This solid has a much higher conductivity than the air and thus affords an easy passage to the heat. Many who have worked on the subject have reached the conclusion that it is the air which functions as an insulator and that the fibres simply serve to keep down convectional heat-flow though they increase the conduction effect. This conduction effect may, however, be quite small, as the general direction of fibres will be at small angles with the plane of the fabric. Heat will therefore encounter much resistance due to poor contact between fibres, especially in the case of those with scales, like wool. The higher insulating value of the less dense fabrics would be evidence for this point. It seems there will be little local convection inside the fabric, but a considerable amount of radiation from the warmer parts to the cooler.

From a practical point of view, however, the relations between the insulating-value and the physical properties are so uncertain, owing to the many variables encountered, that the only method of estimation of insulating-value is by direct measurement. No mathematical relations definite enough to be of utility can be deduced. The main point which comes out in this work is the great importance of the surface conditions in measurements of thermal insulation.

* For wool, see the work of Barker and Burgess [33].

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DEMONSTRATIONS

The regional absorption of dyes by growing crystals*. *Demonstration given on May* 9, 1930, *by* Dr A. G. MILLIGAN, Admiralty Research Laboratory.

The purpose of the exhibits was to demonstrate that, when a crystal is coloured by a dyestuff dissolved in the mother liquor from which it is grown, the colour is not uniform throughout the crystal, but is confined to the wedge-shaped regions swept out by certain faces as they grow. When a dye is absorbed upon any face the deposition of solute upon that face is inhibited, and, since by a well-known paradox the faces which tend to predominate are those upon which deposition is slowest, the presence of a dyestuff may profoundly modify the habit of the crystal.

The exhibits included:

1. Crystals of potash alum coloured by chlorazol sky-blue *FF*. In moderate concentrations the dye was absorbed by the cube faces, which were abnormally developed. A wedge of blue ran from the centre of growth to each cube face and the usual octahedral habit of alum crystals was almost lost. In higher concentrations the dyestuff so inhibited the deposition of salt upon the cube faces that vicinal faces developed, and the colour was confined to a lozenge-shaped patch in the centre of each cube face.

2. Crystals of potash alum coloured by methyl violet. This dye coloured the octahedron faces. The normal octahedral habit was enhanced, and cube faces were rare. Where they occurred, however, the regions below them were colourless.

3. Crystals of Rochelle salt coloured by chlorazol sky-blue *FF*. This salt forms rhombic prisms with many faces developed; but the dye took only upon the $b\{010\}$ prism faces. A transverse section of one large prism was projected on the screen to illustrate the sharp definition of the coloured wedges.

4. Crystals of Rochelle salt coloured by acid green *G*. This dye coloured not only the $b\{010\}$ faces but also, less strongly, the $l\{201\}$ faces as well. No other instance of a dye taking upon two forms simultaneously was ever observed.

5. Crystals of Rochelle salt coloured by phloxine. None of the prism faces was coloured but a wedge of intense pink ran from the centre of growth to each of the end faces.

A modified form of Callendar recorder for the automatic control of a high-temperature oil-bath. *Demonstration given on June* 13, 1930, *by* H. R. LANG, Ph.D., F.Inst.P., Imperial College of Science.

The usual method of using a recorder for the temperature control of a bath is to place on either side of the recording pen contacts which operate suitable relays. In the Callendar recorder such a method involves the movement of the bridge-wire contact, and produces hunting of the order of a few tenths of a degree centigrade.

* *Journ. Phys. Chem.* **33**, 1363 (1929).

This has been overcome in the following way. A long arm is fixed to the screw over which the driving-belt for the pen usually passes, and to the end of the arm a short length of platinum wire is soldered. As the screw is turned by the clockwork mechanism, the platinum wire makes contact with a fixed contact-wire, closing a relay circuit. Suitable stops prevent the arm from turning too far. The relay puts a 100-watt lamp into series with a 250-watt heating-coil wound around the stirrer in the oil-bath. Such a method is better than switching the heating-coil off altogether, and tends to reduce hunting. In its present form the hunting is three or four hundredths of a degree, and it would be possible to reduce it further if necessary. By means of a friction drive it would also be possible to make the apparatus recording, if desired. The apparatus possesses all the advantages of the form devised by the late Prof. Callendar, particularly that of non-sticking contacts, which are made and broken by clockwork. There seems to be no reason why this form of thermostat should not operate equally well at any temperature where a resistance thermometer can be used.

The demonstration showed the oil-bath maintained at 180° C. and the period of hunting was arranged to be about two minutes. By means of another platinum thermometer in the bath the magnitude of the fluctuations was shown to be less than 1/25° C.

FIELD INTENSITY MEASUREMENTS AROUND SOME AUSTRALIAN BROADCAST STATIONS

By R. O. CHERRY*

Author's reply to the discussion on the above paper

In reply to Mr R. A. Watson Watt: The accuracy of this method of measuring field intensities depends only on two factors: (i) measurement of the induced voltage and (ii) the constancy of the ratio E/V for the field loop and the accuracy with which this ratio is known. The calibration of the voltmeter immediately after each reading of V in the field disposes of the first factor, as no evidence has been obtained for any sudden change in the calibration of the voltmeter. The absolute value of the ratio E/V depends on several factors which are difficult to determine accurately but may, with care, be kept constant. Thus the relative positions of the apparatus, the car and the observer were all reproduced for each observation. The effective loop-resistance is made up of copper losses, eddy-current and dielectric losses. With the use of solid wire the first two losses should be constant; the very small amount of solid material used in the construction of the loop should diminish the dielectric losses, the most serious of which are probably the losses in the valve socket and base. Any arrangements for measuring this loop resistance in the field would have meant a complication of apparatus, with the possibility, under field conditions, of errors in measurement exceeding the probable change in loop resistance. Frequent laboratory tests failed to show the necessity for this elaboration of the apparatus.

The ratio $V/E = (2\pi An/\lambda)\,(\omega L/R) = \text{const.}/\lambda^2 R$.

Hence E/V is only proportional to λ^2 if R is independent of λ, and this of course is not even approximately the case, even at frequencies far removed from the fundamental of the coil. I do not know of any simple formula which gives the ratio of resistance to frequency, but since $107/86 = 1{\cdot}24$ and $484/371 = 1{\cdot}30$ (where 107 is the value of E/V for $\lambda = 484$, and 86 its value for $\lambda = 371$) it appears that in the present case R is roughly proportional to $1/\lambda$.

A small condenser was actually inserted across the other half of the loop after the above work had been carried out. Comparison of the results obtained with and without this condenser showed that no appreciable change was produced in either the maximum induced voltage or in the direction of the minima whose angular separation differed from 180° by less than 1°. These minima were not however exactly zero.

The figures quoted by Mr Watson Watt with regard to the importance of the grid-leak resistance were obtained for very large input voltages. The corresponding figures for an applied voltage of, say, 0·01 volt would be interesting. I have not seen any account of a valve voltmeter, using 1 valve only, of equal sensitivity and

* See *Proc. Phys. Soc.* **42**, 192 (1930).

negligible input conductance. The mode of action of the rectifier is not clear to me, but all the tests that have been performed on the voltmeter show that it fulfils satisfactorily all the requirements of such an instrument.

The anode-bend instrument used in the comparison was a commercial instrument of conventional design.

The information from which Table 4 was compiled is rather scanty and it was only inserted to show the very marked decrease in attenuation with the use of the longer wave-length. Comparisons involving the product $E.d$ are not valid in this table, as the two stations are not equidistant from the areas considered.

Noise levels. Further information obtained during another investigation compels me to withdraw the suggestion that atmospherics are less prevalent in Victoria than elsewhere. Mr G. H. Munro has recently set up a cathode-ray direction-finder near Melbourne and he tells me that during his month's observations, he finds atmospherics more prevalent in Victoria than in England.

In reply to Dr Smith Rose: The estimated accuracy in the determination of the intensity applies only to the standard loop. The two methods of determining $\omega L/R$ for this loop agree to 2 per cent. and for the actual measurement of the intensity an additional resistance of 6·16 ohms was inserted in series with the loop. While the actual loop resistance ($0{\cdot}91_1$ ohm) is not known to better than 2 per cent. or 3 per cent., the addition of the short length of straight-wire resistance considerably reduces this error. The suggestion made by Dr Smith Rose to explain the results of measurements taken over sea water is original and interesting. The conditions under which these experiments were carried out were far from ideal, but it appeared desirable to make the investigation because in it surface conditions were as uniform as possible. The unexpected nature of the results emphasizes the necessity for repeating this series of observations under conditions where the possibility of coastal reflection and refraction are absent. If the suggestion of coastal reflection were correct, it would affect the measurements taken, on both sides of Port Phillip Bay, to determine the attenuation due to sea water. I regret that I have no further relevant data on these measurements.

THE MASSES OF THE PROTON AND ELECTRON

By H. T. FLINT, D.Sc.*

Addition to the discussion on the above paper

Mr William Band (Yenching University): I am much interested in the article by Dr Flint on the masses of the proton and electron, particularly because his suggestion that the difference between these masses may be equivalent to a difference between the space-gauge to be used for the two types of particles is similar to the idea which started the theory of electron-tracks that has already received preliminary announcement in *Nature* and the *Physical Review*†. In that theory the metric tensor, or in other words the gauge, is an arbitrary factor which must be chosen as an initial boundary condition as it were. The gauge is not a determinate function of position, and is defined only along the track of the particle. It had not occurred to me that this might be connected with the relations between the proton and electron, but obviously in the light of Dr Flint's suggestions, the connection is simple. Thus a particle is defined as a region bounded by zero field (external and particle-field neutralized) and the mass is defined by the equation

$$m = e^2/4\pi a \text{ in "natural" units,}$$

where a is the radius (average) of the region of zero field. This, as Dr Flint suggests, will depend on the gauge used; and the gauge being initially arbitrary in the mathematical theory, we can choose it so that it will give a measure of a that will give the required empirical value of m. But while this seems to verify that the mass-difference is similar to or connected with a gauge difference, it scarcely explains it.

I think that the real explanation is only to be found by admitting that the simple contrastability suggested by polarity of charge is false. We have already, in Dirac's fanciful suggestion that the proton is a hole in the otherwise full negative energy-states of electrons, the suggestion that protons and electrons are of essentially different natures. And we have evidence that protons will not excite radiation in the way that electrons will. If we accept Whitehead's philosophy that an object is an abstract permanency among the relations between events, then an electron is an abstract entity invented to relate quantum jumps of otherwise independent nature occurring in different atoms, whereas a proton is another invention to connect quantum jumps occurring in the same atom and definitely related as regards frequency numbers, etc. There seems no reason that the two objects as so defined should be directly contrastable in the $(+, -)$ manner, and if their charge is so contrasted, the contrast would seem rather fortuitous, and a property of the definition of charge rather than of the nature of the particles concerned.

* See *Proc. Phys. Soc.* **42**, 239 (1930).

† *Nature*, Jan. 25, 1930; *Phys. Rev.* Jan. 15 and April 15, 1930.

AUTHOR's reply: Mr Band's note is a very interesting contribution to the discussion on my paper on the masses of the proton and electron. I had not seen his work on this subject and it is very interesting to find that he has been led to similar views on this important question. It is difficult to say if we shall be led to alter our conceptions of positive and negative electric charges, but in our present groping in the dark we may find some guidance in approaching our difficulties from another point of view.

PRESENTATION OF THE DUDDELL MEDAL FOR 1929

To Prof. A. A. MICHELSON,

University of Chicago.

The President of the Society, in presenting the medal, made the following remarks:

At the meeting in December last the Council selected from the world of physicists Prof. Michelson as the recipient of the medal for 1929. I think I may say that the general membership of the Society and others outside have cordially approved the selection; indeed messages of warm approval have been received.

The interferometers invented by Prof. Michelson, of which the first was used for carrying out in 1887 the famous Michelson and Morley experiment, have been applied by him, always with complete adequacy of design, to other important and difficult problems, most of them of audacious novelty. These problems included the measurement in 1892 and 1893 of the metre in wave-lengths of light[1]: the measurement of the diameters of stars: the re-measurement of the earth tides: and the testing of the effect of the earth's rotation on the velocity of light[2]. These measurements have had great consequences, of which the following examples may be mentioned:

The difficulties of reconciling the result of the Michelson-Morley experiment with the then prevailing physical conception of the nature of the universe were the direct cause of the enquiry of Albert Einstein[3], which resulted in the theory of relativity. The measurement of the metre in wave-lengths of light[4] resulted in establishing a standard of length free from the uncertainty concerning possible variation which attaches to all material standards. The interferometer for the measurement of the diameter of stars, suggested by Michelson in 1890[5] and first applied by him to Betelgeuse[6], has not only confirmed the correctness of the previously almost incredible dimensions yielded by indirect means of calculation, but has detected fresh stellar phenomena in the variable diameter of Mira Ceti, and the separation of double stars too close for resolution by the unassisted telescope.

The invention by Prof. Michelson of the echelon diffraction grating[7] provided physicists with a potent tool for the investigation of the fine structure of spectral lines, knowledge concerning which has become of such great importance in modern physics. Prof. Michelson has also designed a ruling-engine with which very large gratings have been ruled.

As a final example of Prof. Michelson's work on scientific instruments for the advancement of knowledge, mention may be made of the completion by him in 1926, with apparatus designed by himself, of a redetermination of the velocity of light[8]. The elaborate precautions taken to secure freedom from error included means whereby the distance of 82 miles traversed by the light was measured to a higher degree of accuracy than had ever been reached in triangulation.

We regret that Prof. Michelson's state of health and his distance from us have conjoined to prevent him from being with us to-night, but we welcome very heartily the official representative of the great American Republic who attends in his stead. May I venture to convey through you, Sir, to the United States of America, the congratulations of this Society that you number among your citizens so distinguished a physicist as Prof. Michelson.

Mr David McK. Key, a Secretary of the United States Embassy, received the medal on behalf of Prof. Michelson and expressed his thanks for this honour.

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OBITUARY NOTICES

A. A. CAMPBELL SWINTON, F.R.S.

ALAN ARCHIBALD CAMPBELL SWINTON was born on October 18, 1863. His father was a professor of civil law in the University of Edinburgh. His father's sister was the mother of Archbishop Lord Davidson. The family could prove direct descent from the royal house of Scotland. His brother Capt. George S. C. Swinton has been Chairman of the London County Council, and of the town planning committee of the new City of Delhi. He was also Lord Lyon King of Arms.

From his earliest days Alan showed a strong bent towards engineering and physics. The headmaster of the first school he went to encouraged his bent, but when he went to Fettes College the masters endeavoured to make him devote most of his time to classical studies. His interests however were mainly connected with constructing telephones and taking photographs. At the age of 15, two years after the telephone was invented, he constructed two which worked excellently. His house master at Fettes sent them home so that they should not interfere with his classical studies. As he disliked games his school life at Fettes was not happy. The training he received left a mark on his after life. He always liked talking about classical history and his visits to Italy he found extraordinarily interesting. The impressions produced led to his writing several poems.

After a short visit in 1881 to France, where he improved his mathematics and his knowledge of French and saw the wonderful electrical inventions at the Paris Exhibition, he was apprenticed to Lord Armstrong in the works at Elswick. In 1887 he left Elswick and set up in London as an electrical contractor and consulting engineer. In the former capacity he installed the electric light in many town and country mansions. In 1895 he was elected a Fellow of the Physical Society. In January 1896, after reading an account in the morning paper of Prof. W. C. Röntgen's discovery of X-rays, Swinton was successful in obtaining the same afternoon a shadow photograph by means of a Crookes tube which he happened to possess. A few days later he obtained a shadow photograph of the bones in his own hand. He at once recognized the great benefit of this discovery to mankind. Yet in those days when one of his photographs was shown to an eminent personage, the latter's comment was "How disgusting!"

In February 1898 he showed many interesting experiments with cathode- and X-rays at the Royal Institution. In conjunction with Sir Charles Parsons he converted diamond into coke by heating it in a vacuum by cathode-rays. As the diamond became carbonized it split up and frothed and became much larger in size. The temperature at which this conversion took place was 1890° C.

In 1904 Campbell Swinton gave up contracting work and became exclusively a consulting engineer. He gave many lectures to various societies, including the

Physical Society, and did much to spread a general interest in science. I particularly remember hearing him lecture to the Camera Club, and the sensation he caused, I think in 1896, by throwing an X-ray picture of his own head on the screen when speaking. On another occasion when giving a lecture he passed sufficient high-frequency current through his own body to light a 100-volt 5-candle-power lamp.

He was specially interested in radio work. When Marconi (now the Marchese Marconi) came to England Swinton introduced him to Sir William Preece, and long-distance radio-telegraphy was the result. In 1910 Swinton first heard articulate speech by radio when listening to tests with the Poulsen system. He was associated with the development of the Parsons turbine, and was a pioneer of motoring in England. In 1915 he was elected a Fellow of the Royal Society. He was unmarried. In his private life he was very hospitable. I remember once when he had tests done in the testing laboratories at Faraday House and had received the report which proved that his expectations were wrong, he wrote and thanked not only the Principal but also the assistant who had done the test. He was very interested in the application of a cathode-ray oscillograph to television and hoped that by its help a practical system might be obtained. This perhaps prejudiced him against the Baird system. But only last year, after seeing a practical demonstration of the Baird system working, he wrote to *The Times* withdrawing his strictures on its feasibility.

In 1924 he showed me a sciagram, which had been taken recently, of his own heart and the arteries connected with it. It proved that his life might be cut short at any minute and that he would have to take the greatest care of himself. He did not allow this knowledge to interfere with his work or to prevent his entertaining his friends as cheerfully as he had always done. The end came on February 19 of this year. Physical research has lost one who did much valuable work.

A. R.

WILLIAM R. BOWER

The death occurred on November 20, 1929, at his home at Langley Terrace, Oakes, Huddersfield, of Mr William Richard Bower, who was for over thirty years head of the Physics and Electrical Engineering Department of the Technical College, Huddersfield. Mr Bower was educated at Hartley College, Southampton, and the Royal College of Science, South Kensington, where for a time he acted as demonstrator in physics. After serving on the physics staff at University College, Aberystwyth, he became head of the physics department of the Brighton Technical College, whence he was appointed to Huddersfield. He served there until his retirement at the end of 1926.

Mr Bower was one of the pioneer workers on the application of X-rays to medical work, and installed the necessary apparatus at the Royal Infirmary, Huddersfield.

In addition to his work as a teacher, Mr Bower was well known as a writer on scientific subjects. He was joint author of a popular text-book, Bower and Satterley's *Practical Physics*, and author of *Primary Physical Science*. He contributed also a number of original papers, mainly on optical subjects, to the proceedings of scientific societies. He was a Fellow of the Physical Society (1892–1929) and often attended its meetings in London and the provinces—some of our fellows will remember seeing him at the Birmingham meeting last summer.

In addition to his many other activities Mr Bower was interested in the work of the Association of Teachers in Technical Institutions and for many years served on the national executive council, ultimately becoming president of that association.

He was widely known in his profession, and the announcement of his death will be received with regret by his old students and by a large circle of teachers of physics and Fellows of the Physical Society of London.

A. B. W.

JAMES WALKER

James Walker, born at Dover in 1857, was the only son of Thomas Walker and nephew of the Rev. R. Walker, Professor of Experimental Philosophy in the University of Oxford. Educated from 1872 to 1876 at Rugby, he matriculated at Oxford in 1876, having obtained a junior studentship in Mathematics at Christ Church. He was placed in the First Class in the Final Honour School of Mathematics and from 1883 to 1885 acted as a college lecturer in this subject. In 1883 he was also appointed a demonstrator in experimental philosophy at the Clarendon Laboratory, Oxford, under Professor Clifton, and for 36 years until his resignation in 1919 he was intimately connected with the laboratory and with the general development and teaching of physics in the University. He was a strict disciplinarian, most meticulous in the handling of delicate instruments and in the treatment and discussion of the results of experiments, but students could always rely on sympathetic help from him. Although his interests tended to the mathematical side of physics, as his published papers show, he would never allow difficulties of technique to be slurred over in the laboratory. Deviations and irregularities of observations had always to be explained and traced to their source.

His first published paper appeared in 1887 and was concerned with Cauchy's theory of reflection and refraction. From that year to 1914 papers by him appeared in the *Philosophical Magazine*, the *Proceedings of the Royal Society* and the *Proceedings of the Physical Society*. They were all connected with his favourite study of optics. He was a member of the Council of the Society, and eventually a Vice-President. His constant volumes of reference were the original memoirs of the great physicists from Fresnel to Stokes. His treatise on *The Analytical Theory of Light*, published by the Cambridge University Press in 1904, will always be a memorial to his vigour and power of exposition and logical deduction. In this book he endeavoured "to give an account of physical optics without recourse to any hypotheses respecting the nature of the influence that constitutes light or the character of the medium of propagation."

As a demonstrator in a physical laboratory, he was naturally interested in modern developments of physical theories, but by training and natural inclination his mind was attracted to the logical statement and derivation of the fundamental laws underlying the subject-matter of physics.

He married in 1879 and is survived by his widow, a son and two daughters. Generations of Oxford students will look back with grateful memories of hours spent in discussions with a delightful host and stimulating teacher, and to the generous hospitality which for so long emanated from his house in Oxford.

I. O. G.

REVIEWS OF BOOKS

The Mechanism of Nature, by E. N. Da C. Andrade, D.Sc., Ph.D. Pp. xii + 170. (London: G. Bell & Sons.)

It is not often that he who knows and he who can communicate his knowledge are subsumed under one identity, and the lay world is fortunate in that Professor Andrade possesses both these qualities, and is content to spend his holiday time in making the fundamentals of science as plain as may be to the plain man. He writes, and being Professor Andrade he writes clearly and fascinatingly, about heat and energy, sound and vibrations, light and radiation, electricity and magnetism, the quantum theory and the atom. Even though the book is quite untechnical, it touches many philosophical problems of fundamental importance, and if it raises more questions than it resolves, that is very much to the good.

The book is a model of clear exposition, of apt analogy and illustration; and it is with unalloyed pleasure that we record a complete absence of the heavy and cheap humour characteristic of some attempts to make the truths of modern science evident to the man in the street. But we wish that Professor Andrade would not "claim" where he should "assert."

A. F.

Definitions and Formulae for Students (*Light and Sound*), compiled by P. K. Bowes, M.A., B.Sc. Pp. iv + 36. (London: Sir Isaac Pitman & Sons, Ltd.) 6*d*.

This book, like the lady's baby in *Midshipman Easy*, is "only a very little one," and its size is the main excuse for its existence. It certainly represents a marvel of compression and, judiciously and critically used, may serve to recall a number of leading facts and formulae, elementary and advanced, in sound and optics. But the pemmicanizing of knowledge is an adventure not to be undertaken light-heartedly—indeed it is easier to write a book on a single topic than to condense that topic into a single definition of unimpeachable correctness. Many of the definitions given are crisp and clear, some are mere expansions of the term defined, others are incorrect.

Fermat's law, for example, is better given as the law of the stationary rather than the minimum path, and the definitions of intensity and illuminating power ("Intensity, see illuminating-power." "Illuminating power—the ratio of light emitted by a source of light to that emitted by a standard source") may be left without comment. In his collection of formulae, the author gives no indication of the sign-convention followed.

A. F.

Das ultrarote Spektrum by Cl. Schaefer and F. Matossi. (Berlin: Springer.) Price 28 RM, or bound 29.80 RM.

This work constitutes Volume **10** of the series of monographs on the structure of matter issued under the editorship of Profs. Born and Franck. The names of its authors, Prof. Clemens Schaefer and Dr Matossi, are so well known in the field of infra-red spectra that one might expect an authoritative treatise on the subject. Indeed the authors themselves announce in the preface that their aim is to produce a work which shall be comprehensive both on the theoretical and on the experimental side. The book undoubtedly fills a much felt want. The first section contains an admirable account of experimental

methods. The second and third deal mainly with standard work on black-body radiation, metallic reflection and cognate subjects, an account of which could not be excluded from a treatise of this character.

Readers will probably, however, be more directly interested in the fourth and fifth sections, which deal with the analysis of infra-red spectra in solids, liquids and gases. These sections are clearly set out and well illustrated, while the theoretical treatment is admirably suited to those whose natural approach to theory is from the experimental side. The rapid growth of such a subject is a source of embarrassment to any author, and readers may well be satisfied with the fact that the literature is covered up to the end of 1929. We can confidently recommend the work as a valuable contribution to the literature of modern physics.

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